EAGER VISITOR, RELUCTANT HOST: THE ANTHROPOLOGIST AS STRANGER

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Dans le domaine de la recherche appliquée, le rôle de l'anthropologue est ambigu par nature, qu'il s'agisse, dans notre culture, de l'analyse d'une autre culture ou de celle d'une sous-culture non familière. Face à la culture étrangère, l'anthropologue est l'étranger par excellence, tant au point de vue physique qu'au point de vue culturel. Cet article cherche à réévaluer l'expérience de contact des ethnologues avec la culture étrangère, en particulier sur le plan de leur situation de passage, compte tenu de la notion fondamentale "d'étranger" avancée par Simmel. Ici l'argument consiste à montrer que l'anthropologue lui-même, à la fois hôte et étranger, représente une donnée cruciale du problème soulevé par l'enquête, sur le plan de ses relations interpersonnelles avec les autres membres de l'équipe des chercheurs, un fait qui a des conséquences sérieuses sur les données acquises et sur leur interprétation. Comme peuvent l'illustrer les rapports ethnographiques, la situation au niveau de l'expérience sur le terrain reflète considérablement les facteurs psychosociaux, les relations interpersonnelles et les tendances cognitives résultant des problèmes d'adaptation auxquels l'étranger fait face. Malgré ses limites, la considération de ces facteurs fait surface dans les rapports des ethnographes. L'article procède donc méthodiquement à l'analyse de la masse croissante des rapports ethnographiques, en s'appliquant à mesurer la validité et la valeur de leurs conclusions sur le plan de la recherche.

The role of the anthropologist in field research is by its nature ambiguous, whether we study an alien culture or an unfamiliar subculture in our own society. In an alien culture, the anthropologist is a stranger *par excellence*, an outsider in both the physical and cultural sense. This paper reevaluates the fieldwork situation as encountered by initiate ethnographers in alien cultures, and analyzes the ethnographer's transient position in terms of Simmel's seminal notion of "the stranger." The paper argues that the anthropologist's stranger/host interpersonal relationships with members of the research community constitute a crucial datum with serious implications for the acquisition and analysis of research data. As presented in ethnographic accounts, the field situation largely reflects the influence of psychosocial

factors, interpersonal relationships, and cognitive bias due to the adaptive problems of being a stranger. However, insufficient consideration has been taken of these factors in ethnographic reports. Methodologically, this paper utilizes the growing volume of *ex post facto* ethnographic reports to question the validity and reliability of some research conclusions.

The stranger that sojourneth with you shall be unto you as the homeborn among you. . . . (Leviticus 19:3)

INTRODUCTION

From antiquity, the stranger appears to have always been the object of special treatment. Georg Simmel (1950), writing on the ambiguous position of the stranger, might well have had anthropological field-workers in mind when he portrayed the stranger's relationship with the host community as one of "nearness and remoteness." The stranger is an outsider who is nevertheless "fixed within a particular spatial group"; yet "his position in this group is determined, essentially, by the fact that he has not belonged to it from the beginning, that he imports qualities into it, which do not and cannot stem from the group itself" (1950:402). Simmel further touches on other important elements which are characteristic of anthropologists as strangers in the field: objectivity, detachment, and involvement in relation to the host community (1950:404-405). It is this constellation of strangerhood relations on which the present discussion is focused.

This paper attempts to reevaluate one of the most crucial aspects of anthropological research: the fieldwork situation as encountered by the initiate ethnographer in an alien culture. Starting from Simmel's well-known seminal concept of "the stranger," the paper applies this notion to the position of the anthropologist in the field. The paper argues that the anthropologist in the field is at once "the stranger and an element of the group" he or she is studying; and that the concept of "stranger" as an integral factor of the ethnographer's social situation merits serious analysis for its implications for research results. The paper further suggests that the interpersonal relations of field-workers with members of a society under investigation are in their own right a *datum* that is liable to significantly influence field data and the ultimate conclusions about a society. Attention is drawn to the apparent difficulty of making objective evaluations of field reports (ethnographies) without concurrent consideration of the role of the anthropologist as a stranger in the community that was studied.

In the welcome but belated efforts by some anthropologists to explain their field methods and to provide further insight into their published ethnographies, the critical "stranger role" of the field-worker is still inadequately considered. The relationship of the researcher to the people who are the subject of ethnographic study constitutes the core of the anthropological enterprise in the field. This is because when reduced to its basic element, anthropological fieldwork is essentially a human relations situation. The role of the anthropologist as the outsider, the stranger *par excellence*, constitutes an integral part of field data that ultimately find their way into the report about the culture being investigated. It is this role that needs more attention in the research methodology of fieldwork.

One of the earliest essays on the concept of the stranger in sociological research is that of Schuetz (1944) which focuses on the psychological aspects of strangerhood and the problem of cultural orientation faced by an outsider. "The stranger," Schuetz notes, "has to face the fact that he lacks any status as a member of the social group he is about to join, and is therefore unable to get a starting point to take his bearings" (1944:504). However, Schuetz is mindful of the ineluctable link between the psychological situation of the stranger and the social environment to which he must adapt if he is to maintain meaningful interaction with his hosts. In this process of adaptation, the stranger moves from "thinking about" the host group to "acting within" such a group. As Schuetz graphically puts it, "Jumping from the stalls to the stage . . ., the former onlooker becomes a member of the cast, enters as a partner into social relations with his coactors, and participates henceforth in the action in progress" (1944:503).

This is precisely the situation that any ethnographer in the field confronts, and it is this transformation in which the observed community passes from being only a "subject matter of his thoughts" (a theoretical model) to being a "segment of the world which has to be dominated by his actions" (a social situation) (ibid.). The very moment the ethnographer leaves the stalls of passive observation and enters the stage as a participant observer, he or she becomes a datum in the corpus of data collected about "his" or "her" people. Thus, the adaptive problems of strangerhood have more to do with the psychosocial environment than with physical existence, as so many field-workers claim.

The role of the anthropologist as a stranger has been specifically analyzed by Nash (1963) and Nash and Wintrob (1972), and has been commented upon by Richardson (1975), among others. Nash (1963) pays particular attention to the problems of adaptation to the sociocultural environment which constitutes the ethnographer's field. The gist of Nash's argument is that "the ethnologist in the field is a stranger, that he faces the problem of

adapting to this role, and that the objectivity of his field report will, through the mechanisms of perception and cognition, reflect the nature of his adaptation" (1963:150; see also Nash and Wintrob 1972:532).

This paper argues that a great deal of current discussion about the "adaptive problems" of field researchers tend to focus on sociophysical factors such as culture shock, lack of privacy, material discomfort, and so forth rather than on psychosocial factors such as cognitive bias, stress, and interpersonal relations. Field situations presented in ethnographic reports are largely the result of the influence of all these factors, and reflect the subjective view of both the ethnographer and "trusted informants" who, by the evidence of most ethnographic accounts, are marginal in their own culture. This point will be discussed later in the paper.

Before we delve into the main discussion of the paper, it is essential at this stage to clarify a number of points in order to obviate any misconceptions about the paper's aim. This paper is not a denunciation of the discipline of anthropology as ethnography, nor of fieldwork as a principal method of ethnographic research; nor is this paper a critique of anthropologists as social scientists. Rather, the main thrust of the paper is directed at fieldwork problems, qua problems, and the manner in which these problems affect or influence both the acquisition of ethnographic field data and the quality of the cultural information gathered. For this reason, the paper argues that what either happens or does not happen to the anthropologist as a stranger in the field, and the manner in which the anthropologist reacts to the experience of fieldwork, constitutes a major aspect of the field data that ultimately shape an ethnographic report. As will be pointed out below, it has not been traditional for anthropologists to report on the fieldwork experience itself and the attendant methodological problems which are inevitably entailed in the stranger-host relationship in "other cultures." Consequently, the objective evaluation of ethnographic research was not always as self-evident as it might seem from the reported results. Lately, because anthropological field-workers have either included pertinent information about their field experiences or have subsequently published reflective reports of those experiences, the anthropological community and other social scientists have been able to make informed, retrospective evaluation of their endeavors (Chagnon 1968, 1974; Hicks 1976; Malinowski 1967; Middleton 1970; Rabinow 1977; Siskind 1973; Spindler, ed. 1970; see also the highly instructive collection of reflective articles in Lawless and Vinton, eds. 1983).

The changing nature of anthropological research has concomitantly resulted in a refinement of research methodology in

order to conform to the standardized canons of research. Closer attention is now being paid to much more intrusive as well as obtrusive factors which are an intrinsic hazard of fieldwork. The cardinal point of this paper is that the ethnographer is a primary factor in this process. A major catalyst in this heightened awareness of the methodological problems of fieldwork is twofold: (1) the changing nature the field situation in traditional research locales as "native" societies have become more literate and reflective about the anthropological enterprise; and (2) increasing anthropological interest in the segments of "complex" societies. While this is not intended to be a categorical detraction from the quality of the work of earlier generations of more traditional field-workers, it allows for a more realistic evaluation of the often uncritical acceptance of published field results. Pointing out problems in a discipline does not denounce the enduring contribution of its pioneers; rather, this procedure heightens awareness of the need for rectification as shown by the growing literature on anthropological research methodology, something that was painfully lacking in anthropological field preparation a generation ago. Thus, to construe the ensuing reexamination of such works as an unfilial symbolic devouring of anthropological ancestors in a ritual cannibalistic feast of atonement would be erroneous; instead, it is a reaffirmation of the humanity of the discipline whose focus is the study of humankind by humans themselves.

Until recently, it has not been in vogue for anthropological field-workers to include their frustrations and difficulties with their subjects, their failures, disillusionments, and even disenchantment with the cultures studied in their ethnographies. Rather, the tendency has been to stress the strategies which were adopted for eliciting data that enabled the researcher to present an "integrated structure" of that society. We know more about the perennial problem of bridging the conceptual boundaries between the anthropologist as a cultural stranger and his or her native informants than we know about the psychological stress produced by an uneasy and often artificial relationship between an eager visitor and reluctant hosts. Occasionally, a negative comment about some reluctant or recalcitrant informant provides insight into the anthropologist's relations with some of his or her subjects. In many cases, difficult relationships are dismissed as incidental to, or are mentioned in order to emphasize, the much touted friendly acceptance of the researcher by the "natives."

Recently, a spate of *ex post facto* reports on stress and response in fieldwork have been published in various formats in an attempt to provide insight into the tribulations of field-workers and as guides to methodology and research techniques in the field. Few, if any of these works dwell on the social and psychological impact that the stranger exerts on his hosts. Given the personal nature of

anthropological fieldwork, it is legitimate to ask: To what extent is the highly touted claim that field-workers were "fully accepted as adopted kinsmen" of their tribes reliable or verifiable in the absence of corroborative information? The apparent fact that an anthropologist was permitted to live and work among unfamiliar people and even to receive their cooperation in a venture from which they have no interest and derive no benefits, material or otherwise, does not constitute evidence that the people concerned did not harbor serious misgivings about the disturbance of their life by the intrusion of the stranger in their midst (Briggs 1978:26; Barnes 1967:198).

STRANGER-HOST FIELD RELATIONS

The role of the anthropologist in field research is always ambiguous, whether we study an alien culture or an unfamiliar subculture in our own society. In an alien culture, the anthropologist is naturally the stranger *par excellence*, an outsider in both the physical and cultural sense. Within a subcultural segment of their own society, anthropologists are enigmas to members of the subsection about whose social behavior they ask questions. Furthermore, informants may be convinced that anthropologists either ought to know the answers to these questions naturally, or have no business knowing (Spradley 1979).

Traditionally, anthropologists have tended to claim that "I was adopted and initiated as a kinsman by the tribe," or "I was accepted and treated as one of them, as a member of the tribe." The ethnographer will then refer to some traditional custom among "his people" of adopting strangers as fictive kin. For example, Barnett invokes local custom as a basis for his claim to Palauan (fictive) kinship.

> In fact, my intimate acquaintance with this family, and my entree into the community which it provided, convinced me that I could and should write something like *Being a Palauan*. Kai spoke of me as his elder brother. He treated me as such and I was so accepted by other residents of Ngara. (1970:30)

The last sentence raises an interesting epistemological question about subjectivity in fieldwork. To what extent can it be shown that the researcher's perception of, and feeling about, this relationship coincide with his hosts' own evaluation of the makeshift arrangement? In the absence of a Palauan viewpoint, it is difficult to verify the ethnographer's own assessment that:

> This was neither false nor as difficult as it might be in other societies because the establishment of fictive

> kinship is a Palauan custom. It gave me a place in their social system in addition to the one I inevitably was assigned as a prestigious outsider. (ibid.)

It should be emphasized that the ethnographer's sincerity is not in question; what is at stake is how to bridge the gap between objectivity and subjectivity in fieldwork, how the ethnographer perceives this, and what actually occurs in the ethnographer's relations with hosts.

As teachers, anthropologists often stress the overriding importance of the kinship idiom in the social relations of "primitive societies." By implication, they enculturate students into thought patterns that lead to the assumption that being "adopted as a kinsman" by the natives is a key to fieldwork success. Nothing could be further from the truth (see Chagnon 1968:4-5). Firth warns against exaggerated or sentimental claims about overcoming strangerhood on the basis of fictive kinship:

> . . . I regard with skepticism the claim of any European writer that he has "been accepted by the natives as one of themselves." . . . such a claim is usually founded upon a misapprehension of native politeness or of a momentary emotional verbal identification with themselves of a person who shares their sympathies . . . as in dancing with them and observing the etiquette of (pseudo-) kinship . . . (1957:11)

Similarly, Hart (1970:151-152) gives an interesting but skeptical account of the "fictive kinship game" in fieldwork. And Freilich cautions that "the anthropologists's role is somewhere between 'native' and 'privileged stranger'. . . Irrespective of what role he assumes, the anthropologist remains a marginal man in the community, an outsider" (1977:2). Schwab also shows an awareness of the dubiousness of the popular claim to fictive kinship: "The anthropologist under the best circumstances never becomes a fully accepted member of the culture he is studying . . ." (1977:47).

Beals provides yet another insight into the relationship problem between stranger and host when describing his and his wife's early experience in Gopalpur, where villagers "Were unaccustomed to strangers and they would not let us remain as strangers" (1970:45). After some initial adaptive adjustment "we came to feel at home in Gopalpur. . . . We were a part of the community. . . ."(1970:46). The picture presented thus far suggests nothing of the feelings and attitudes of the hosts about the strangers among them. Ethnographic accounts are by nature one-sided, although based on dyadic interaction. Most ethnographies, and lately "field method guides," provide insight into

the ethnographer's tribulations in dealing with the "natives." There is concern about the lack of privacy experienced by ethnographers in the field--that they cannot enjoy a few moments of quiet and privacy without being bothered by prying natives (Evans-Pritchard 1940:13-15; Chagnon 1968:3-6; and for a perceptive treatment, see Dentan 1970:104-105, 107). Yet one hears far less about the effects of the stranger's own intrusion into the social life of the hosts, and the possible resentment this might cause towards the ethnographer.

The fact that anthropologists in the field are strangers and are subject to all the vagaries of that role *vis-à-vis* host communities has serious implications for the acquisition and analysis of research data. The experiences of those field-workers who have paid some attention to their relationship with their hosts as a datum in their research, and reported on this, raise a host of methodological problems about data gathered from reluctant hosts. Dentan directs attention to this problem:

> We were afraid that if the people thought of us as "outsiders" they would tell us whatever they thought we wanted to hear and would conceal anything intimate or anything of which they thought we might disapprove. . . . They distrusted outsiders (mai) so much that they use a special slang to conceal what they are talking about in the presence of an outsider. . . . (1970:92)

Similarly, in his much celebrated study of the Nuer, Evans-Pritchard illustrates the tactics adopted by these people to fend off curious ethnographers. "Nuer are expert at sabotaging an inquiry and until one has resided with them for some weeks, they steadfastly stultify all efforts to elicit the simplest facts and to elucidate the most innocent practices" (1940:12). Strange as it may seem, in his analysis of Nuer society, Evans-Pritchard appears to gloss over this difficulty and does not see it as an impediment that might vitiate his data. By a curious turn of logic, he compares his field experiences among the Azande and the Nuer, and antithetical as the two situations were, he comes to the same conclusion about the validity and reliability of data from both. He writes:

> Because I had to live in such close contact with the Nuer I know them more intimately than the Azande, about whom I am able to write a much more detailed account. (1940:15)

What were the ethnographer's relations with these two societies respectively? We are told epigrammatically that:

> Azande would not allow me to live as one of themselves; Nuer would not allow me to live otherwise. Among

> Azande I was compelled to be outside the community; among Nuer I was compelled to be a member of it. Azande treated me as a superior, Nuer as an equal. (ibid.)

Yet in subsequent accounts of these respective societies, Evans-Pritchard is silent on the relationship between data and these varied conditions under which data were collected.

Malinowski was the first among the earliest ethnographers to explicitly recognize the role of the stranger in fieldwork, his impact on the host community, and the possible implications of such an impact for field relations. Malinowski observes of the Trobrianders that " . . . they knew I would thrust my nose into everything, even where a well-mannered native would not dream of intruding, they finished regarding me as part and parcel of their life, a necessary evil or nuisance, mitigated by donations of tobacco" (1928:8). I suggest that for all anthropologists in the field, this observation constitutes the limits of so-called full acceptance or adoption as fictive kinsmen in the culture they are studying. There are many ways of dealing with a "nuisance"--eliminate it, tolerate it, ignore it, or in its human form, accede to its pestilent questions to get it off your back. "Natives" often adopt the latter solution and supply the kind of information they think will get the inquisitive anthropologist off their backs. Often, an exploitative situation develops in which the "informant" not only trades off information with the anthropologist, but intentionally deceives him (Chagnon 1968).

Chagnon's self-revealing attitude towards the Yanomamo epitomizes the peculiar relationship between the stranger/ ethnographer and the native hosts. Both parties played the "bluff-upmanship" game to a fine degree:

> On another occasion I was eating a can of frankfurters and growing very weary of the demands of one of my guests for a share in my meal. When he asked me what I was eating, I replied "Beef." He then asked,"What part of the animal are you eating?" to which I replied, "Guess!" He stopped asking for a share. (1968:7)

Similarly, the Yanomamo would call Chagnon's bluff or respond with their own:

> "Give me an axe or I'll break into your hut when you are away visiting and steal one!" And so I was bombarded by such demands day after day, month on end, until I could not bear to see an Indian. (1968:8)

Apparently, this bluffing relationship continued throughout

Chagnon's fieldwork: "As I became more proficient in their language and learned more about their political tactics, I became more sophisticated in the art of bluffing" (1968:9). The consequences of all of this were reflected in falsified data that were supplied to Chagnon over a five month period (1968:10-13).

Defined in stranger/host terms, the informant-ethnographer relationship is the pivot upon which the whole anthropological enterprise in any given community revolves. Success in fieldwork may hinge precisely on the nature of this relationship. The informant-ethnographer relationship is most intimately linked with the quality in terms of veracity of the data supplied by informants, since the informant's task as imposed by the ethnographer is to "teach" this stranger all the "facts" about the society (Richardson 1975:520-521). The question of bias cannot be ignored in such a contrived social relationship. First, there are categories of information (cultural data) which are deemed good for strangers, some of which are considered appropriate for friends, and others of which are shared only among clansmen and close kin. These are universal cultural principles of information management. Second, both parties are in a manipulative situation which influences the exchange of desired "commodities" (information for gain). In addition, the respective attitudes of the participants toward each other are governed by this contrived reciprocal relationship (see Firth 1957:10-13; Beattie 1964:86; Beals 1970:47-48; Dentan 1970:108.)

It is all the more surprising that an eminent anthropologist and superb field-worker such as Evans-Pritchard would pay scant attention to the interpersonal relationship between ethnographer and informants in his methodological lecture on fieldwork. Rather, Evans-Pritchard seems to treat all of "native society" or "primitive society" much as an abstraction which constitutes the "field" for the ethnographer, and not as a vibrant community of individuals with whom the field-worker must interact on a personal basis: "The native society has to be in the anthropologist himself and not merely in his notebooks if he is to understand it, and the capacity to think and feel alternately as a savage and as a European is not easily acquired, if indeed it can be acquired at all" (1951:82). The obtrusiveness of the field-worker is also minimized: "He is not there to change their way of life but as a humble learner of it" (1951:79).

However, it may be argued that by its reliance on "key-informant interviewing," fieldwork affects changes in the community (Edgerton and Langness 1974:33-35). But informants and informant interviewing are not an integral part of "native society," for informants have to be trained in the ways of the anthropologist. At least one ethnographer has pointed out that: "The problem of finding, cultivating and changing informants is one of the most delicate facing the anthropologist" (Rabinow 1977:92).

The role of the anthropologist as a stranger limits access to certain kinds of information and encourages increasing reliance on so-called "trusted informants." But trusted informants are not altruistic conveyors of information. Instead, they are purveyors; that is, partners in an exchange relationship in which the stranger is at a disadvantage (Barnes 1967:198). Since anthropologists need certain information which is not normally accessible to strangers, they must rely on "informants" who have complete control over what information the stranger ought or ought not to have, and when and how this information will be transmitted.

Chagnon's experience among the Yanomamo is instructive. After apparently a whole year of collecting uncertain genealogies, he writes:

> . . another individual came to my aid. It was Kaobawä, the headman. . . . He visited me one day after the others had left the hut and volunteered to help me on the genealogies. He was poor he explained, and needed a machete. He would work only on the condition that I did not ask him about his own parents and other very close kinsmen who were dead. He also added that he would not lie to me as the others had done in the past. This was perhaps the most important single event in my fieldwork, for out of this meeting evolved a very warm friendship and a very profitable informant-field-worker relationship. (1968:13)

The pitfalls in this kind of relationship are obvious as we learn that the ethnographer later relied heavily on one self-ingratiating informant. "If there were things he did not know intimately, he would advise me to wait until he could check things out with someone in the village. This he would do clandestinely, giving me a report the next day" (ibid.). Reliability of data was checked against the opinions of a close kinsman of the informant, Rerebawa, who in the ethnographer's evaluation "is one of few Yanomamo that I feel I can trust . . . he is the most genuine and most devoted to his culture's ways and values. I admire him for that. . . ." (1968:16-17; see also Chagnon 1974:101-103).

Manning and Fabrega have directed attention to the role of informants and the implications of this for field research. They point out that the role of informant has become specialized and is dependent upon mutual socialization between ethnographer and informant; that is, it is a role involving the exchange of information for "prestige, money, affection, and ingratiation" (1976:44). The ramifications of this relationship lead to the development of a quasi-professional cadre of informants who are willing to serve at a stipulated rate of pay as informants on

certain types of problems by adopting a tactical stance which includes strategies of avoidance, information-management, and social control of the investigators (ibid.; see also Pelto 1970:95, 97).

Berreman's experience in an Indian village is very instructive because it dispels the prevalent notion that the field-worker's adaptive problems are effectively solved by fictive kin adoption in simple societies. In Sirkanda, strangers "are avoided or discouraged from remaining long in the vicinity. To escape such a reception a person must be able to identify himself as a member of a familiar group through kinship ties, caste (Jati) ties and/or community affiliation" (1962:4-5). It should be noted that the norm that strangers must be able to identify with local groups in order to gain acceptance is not the same thing as the assertion that acceptance will follow because a particular community has a custom of fictive kinship by which strangership may be transformed. To suggest, as many field-workers have, that a norm which obtains for a local community is *a fortiori* a justification for ultimate strangers to claim the same privilege as members of adjacent or local groups misconstrues the social customs of "simple" societies.

Berreman speaks of being constantly under suspicion:

> Nearly four months had passed before overt suspicion . . . was substantially dissipated, although . . . some people had been convinced of the innocence of our motives relatively early and others remained suspicious throughout our stay. (1962:7)

With candid honesty, Berreman states that in the eyes of the natives he remained an alien, that he was never made to feel his presence in the village was "actively desired by most of its members," and that he was "tolerated with considerable indulgence" (1962:8). Apparently this indulgence was coupled with information-management on the part of the villagers whenever they thought this was necessary, and "did not mean we therefore could learn what we wanted to learn in the village" (1962:9).

STRANGERHOOD AND CULTURAL DATA

The relationship between the eager visitor and reluctant host and its implications for the reliability of ethnographic data should never be underestimated. The adaptive problems of the anthropologist as a stranger, and the personal and cultural obstacles that may impede the acquisition of reliable data have been aptly spelled out by Nash (1963). He points to the enormous jump from one culture to another, the need to establish rapport and to acquire fairly complete data in both a limited amount of time and under extreme conditions of strangership, and pressure on

field-workers to succeed because their careers and reputations depended on successful adaptation to the field. And as Chagnon puts it, "Scientific curiosity brought me to this village and professional obligation kept me there in circumstances I did not particularly enjoy" (1974:196). In a similar fashion, the much revered field-worker, Malinowski, described his native subjects in less than flattering terms. He saw their life as "utterly devoid of interest and importance, something as remote from me as the life of a dog" (1967:167). His main interest was to "somehow document" all aspects of their lifestyle so that he would have valuable material for his project.

As many field-workers have eloquently testified, the majority of "trusted and useful informants" are marginal individuals in their cultures. This applies with even greater force to academically-trained, local informants. The possible pitfalls that the choice of academically-trained field assistants-cum-informants might pose are well-illustrated in Diamond's revealing but instructive field-method report on her work in Taiwan (1970). Among her principal field assistants-cum-interpreters were a Taiwanese "from one of the modern fishing ports on the eastern coast . . . trained in economics at Taiwan National University," while another "came from a small farming village, but had studied a number of years in Taipei. He had a Masters (*sic*) degree in political science and possessed a strong interest in sociology." Furthermore, this assistant was "Widely read in the social sciences . . . " and ". . . his academic training and reading enabled him to do a considerable amount of work on his own" (1970:132-133). The others were "two young women who taught in a city middle school"; and "One of them had taken anthropology as her major in college. . . ." Yet another key field assistant-cum-informant was "an active leader in the local Fishermen's Association and Farmer's Association, as well as being a member of the school board and temple committee." Moreover, he was "Literate, (and) `modern' in his attitudes towards technological innovations and education, and at the same time deeply concerned with the preservation of tradition . . . ." (1970:134).

Such excellent qualifications on the part of an ethnographer's field assistants or informants may be presumed to enhance the quality of the investigator's data; yet the crucial question remains: To what extent may data collected under such conditions be taken as representative of the "native culture"? Could an informant who is labelled "modern in his attitudes towards technological innovations and education" be free of bias against native culture? An even stronger objection may be raised against the utilization of, and reliance upon, trained field assistants-cum-informants from the culture under study. Since field researchers are wont to declare that their key or trusted informants were somewhat marginal to their cultures, one wonders why the information they provide should be treated as "social facts" about traditional native cultures

(see Chagnon 1974:18; den Hollander 1967:15).

Even more serious doubts may be raised with respect to the use of native informants or assistants trained in the social sciences, since such training is *a fortiori* an enculturative experience in itself. In other words, a scientifically trained native assistant is bound to observe his/her society's culture with "interpretative eyes," and is liable to respond with "analytic verbal responses" rather than with factual answers to cultural questions. This point may be more forcefully appreciated if an analogy is drawn between social scientists and other members of their own society who are *not* trained in sociology or anthropology. The social scientist's analytic perception of his/ her culture is qualitatively different from that of lay members of the same culture. Thus, it is not without a grain of truth that anthropologists in particular are often viewed, and view themselves, as marginal persons in their own societies, including the academic community in general (see Nash 1963:159). It would be academic naïveté to suggest that any student who has been enculturated into socioanthropological thought-patterns would still be capable of looking at social phenomena without tinted lenses. As Nash aptly puts it, "The novice anthropologist is recruited into and trained by a group with a particular formal and informal ideology which, to a greater or lesser extent, he absorbs and carries with him into the field" (1963:149).

A most instructive caveat against the uncritical use of informants who are enculturated in the social science milieu is poignantly illustrated by Spradley's narrative of his field encounter with a former Harvard University graduate student in anthropology who had become a skid row habitué. Initially, Spradley was "excited about the possibilities of working with Bob as a key informant," but soon discovered that Bob was responding to interview questions with:

> . . . the standard analytic categories that many social scientists use. . . . [and] tended to analyze the motives men had for drinking and other behavior, but his analysis always reflected his background in college. (1979:53)

This candid observation is a far cry from the traditional habit of most field-workers of ignoring the potential distortions of data that inevitably result from information provided by "enculturated" informants. It is well-known that the principal aim of anthropological fieldwork is the first-hand acquisition of raw data on a given research community. Paradoxically, such data are to be obtained from native informants who are knowledgeable about their culture, but is to be conveyed in idioms which are not consistent with the scientist's expectations of order, coherence, and rationality. Perhaps the insidious temptation to rely increasingly

upon "trained informants" is fueled by what den Hollander calls "a passion for neatly smoothing out a disordered social reality which thus distorts the social life of the people being reported on" (den Hollander 1967:20).

The problem of biased social perception and data collection by anthropologically trained observers is pointedly discussed by Keiser on the basis of his research on an urban subculture in his own society. As Keiser frankly puts it, "What I saw as facts and therefore recorded, was directly related to my theoretical orientation. Because of my orientation, I did not record certain things that are undoubtedly important" (1970:233). This is precisely what den Hollander so aptly terms "arranged truth," that is, truth (read data) that must look organized to be veracious (1967:25). As every anthropologist knows, this dilemma confronts all anthropological field reporting.

Philosopher Karl Popper has directed some attention to this incessant habit of the subconscious reorganization of phenomena in scientific observation, and reports:

> Our propensity to look for regularities, and to impose laws upon nature, leads to the psychological phenomenon of dogmatic thinking or, more generally, dogmatic behaviour; we expect regularities everywhere and attempt to find them even where there are none; events which do not yield to these attempts we are inclined to treat as a kind of "background noise," and we stick to our expectations even when they are inadequate and we ought to accept defeat. . . .(1963:49)

A similar poignant point is made by den Hollander to the effect that "we avoid or reduce any information that creates dissonance and welcome anything consonant with our conditioning for order and structure" (1967:20). It is thus reasonable to suspect that anthropologically trained informants would subconsciously make *a priori* deductions from perceived "social facts" and imply "structural" linkages and "functions" which are not warranted by the observed social phenomena.

Turning now to a "confession" by a practicing anthropologist, we find confirmation of this penchant for "arranged truth" in anthropological field reports. In a foreword to *Political Systems of Highland Burma* (Leach 1954:vii), Raymond Firth confides that in order to keep anthropological reporting neat, "Some of us . . . have not hesitated to tell our students in private that ethnographic facts may be irrelevant--that it does not matter so much if they get the facts wrong so long as they can argue the theories logically." This is a seriously disturbing commentary on anthropology as a science. Sacrificing empirical facts for theoretical models, for equilibrium,

for order and organization, and for style appears to be a well-entrenched practice in anthropological thinking, particularly in British anthropology as witness the works of Evans-Pritchard (1937, 1940, 1951, 1956); Fortes (1945, 1949a, 1949b, 1969); Gluckman (1955); Leach (1954); Lienhardt (1961); Radcliffe-Brown (1950, 1952); et al. Barnes (1962) has drawn attention to the British structuralist influence on researchers working on lineage/kinship systems outside of the African context. He criticizes the tendency to predicate lineage models on New Guinea societies which have been extrapolated from African ethnography.

Because fieldwork is essentially a human relations situation, ethnographic field reports sometimes inadvertently reveal the personal biases of researchers with regard to some of their informants. Individuals whom the ethnographer considers as unfriendly or disagreeable are often by-passed, or, if discussed at all, cast in negative light. Regarding the people of Takashima, Japan, Edward Norbeck writes: "At the time of my first interview with the head man of the community, I had suspected that he was generally disliked and I did not like him myself. He seemed arrogant, rude, and terribly contemptuous of his neighbors . . . . " (1970:250). Because of this negative attitude toward the headman, Norbeck "talked with him only often enough to meet the demands of etiquette. . . ." (ibid.). Similarly, Chagnon shunned a Yanomamo headman he considered "unpleasant, selfish and self-centred"; their "relationships were fairly cold and strained" and he was thus never used as an informant (1974:167). Would it not make anthropological sense to treat the alleged unfriendly or uncooperative behavior of such native individuals as a datum in itself, rather than merely as anecdotes?

Again, those individuals whom ethnographic researchers are wont to call "excellent informants" are often marginal members of their societies. Note the following observation:

> Ali was a first-rate informant. He was intelligent, quick to learn, patient, cooperative, and vivacious. . . . Ali, like several other people with whom I worked, was a marginal character in his own social world. He was not the average villager, he was far from the solid citizen stereotype of Serfrou . . . Ali was more self-reflective about his society and his place in it than most Moroccans I knew. . . . He had rejected village life. . . . Already being ostracized by large segments of the community, he would mock the bonds of social control by flaunting his freedom. (Rabinow 1977:73)

Information provided by such channels provokes serious questions of validity. Our discomfiture is further increased when the researcher reveals that Ali was a professional information

purveyor: "Ali had deliberately pursued me . . . partly because he saw the possibility of income and partly because he was relatively immune to the community's social control. He had worked with other anthropologists who had come to Serfrou; he knew the ropes . . ." (1977:75; see also Chagnon 1968:13).

The stranger/ethnographer is liable to make value-loaded judgments when comparing his favored informants who display "an imaginative ability to objectify one's own culture for a foreigner," with those who are deemed to lack this quality. Yet the so-called village idiot may provide valuable insights into the "unrationalized" aspects of his society's culture (Rabinow 1977:94-95).

Most ethnographic accounts of fieldwork experiences reveal interesting similarities in the tendency of researchers to stress the supposed good qualities of their favored informants. For example, one of Rabinow's favored Moroccan informants, Rashid, "was incredibly quick, intelligent, sensitive and overflowing with gossip and slander about almost everyone in the village" (1977:96). Yet when the anthropologist was warned of Rashid's moral character, he dismissed it as due to "simple jealousy." By the ethnographer's own account:

> The moral attacks on his character never impressed me very much. . . . In retrospect, all these charges and several more turned out to be basically true. But for some of the same reasons he was an excellent informant, he was on the fringe of community control. . . . He would say things and talk about people in a manner which the anthropologist cherished. . . . He was more than happy to tell me almost everything I wanted to know. (1977:98-99)

To what extent can such admittedly subjective information be accepted as reliable and valid data?

One suspects that anthropological field-workers tend to exaggerate their informants' capacities for comprehension of their respective cultures and/or their informants' credence. It is well-acknowledged that culture in preliterate traditional societies is "lived" and acted out rather than philosophized about (cf. Bloch 1971:86). Consequently, members of such societies cannot be assumed to have contemplated most of the cultural elements about which the ethnographer asks questions in isolation from their role in the scheme of social behavior. Similarly, the average lay person in technologically advanced societies generally has only a vague notion of the "patterning," "interdependence," or "functional relationship" of the disparate elements of his culture. Anyone who cares to try out the anthropological informant interview method on a class of undergraduates will soon perceive the truth of this

analogy.

It is thus with some hesitation that one would be inclined to accept uncritically the objectivity and validity of data sources such as the following: "In Sensuron we identified and selected a girl of sixteen for our household assistant . . . The girl had been trained as a ritual specialist . . . She served as a key informant . . . Her special ritual knowledge and her ability to give meaningful and accurate details of widely shared aspects of her culture were invaluable" (Williams 1967:29). Apart from the declared immaturity of the informant, serious doubts may be raised about the validity of information rendered by an equivalent of a medical neophyte concerning medical science and health practices in an advanced society. By what standards were her ritual knowledge, ability, and accuracy of detail measured?

It should be remembered that the role of informant is not an integral part of the structure of native society, but an additional datum in a new structural relationship that includes the ethnographer as a stranger. The place and position of the ethnographer in the society being studied determine the channels of information that are open to him or her. More importantly, "The channels of information in turn are crucial in defining the information itself" (Buechler 1969:1).

In retrospect, Berreman (1962) appears to take full cognizance of this stranger-host relationship and its implications for field data. Berreman's observations suggest a further rebuttal to the idealistic claims of some field-workers that mere entry into a research community entails acceptance as a kinsman. Berreman writes:

> In such a society the ethnographer is inevitably an outsider and never becomes otherwise. . . . The nature of his data is largely determined by his identity as seen by his subjects. Polite acceptance and even friendship do not always mean that access will be granted to confidential back stage regions of the life of those who extend [these courtesies]. (1962:21)

There are encouraging signs of an increasing awareness that all may not have been well with field data obtained under conditions of doubtful relations, at times bordering on resistance, deceit, and fabrication of information purveyed to the unsuspecting ethnographer. Retrospective revelations of field experiences by various ethnographers suggest that a hitherto unacknowledged truth may be that the enthnographer's "stranger status constitutes a major constraint" in gaining unfettered acceptance and acquiring unsullied data (Uchendu 1970:231). Subtle rejection of the stranger may be the general order rather than the exception in field relations.

By and large, anthropological field reports tend to emphasize the positive aspects of their authors' relations with members of the societies studied. These relations are often presented as a process of progression from initial problems of adaptation, through uneasy acceptance, to successful integration (the "as-one-of-them" fictive kinsman claim). There is something uncannily analogous to *rites de passage* in this process. In its mathematical sense, the initiate's successful integration becomes a function of the degree of difficulty in adaptation and acceptance; hence the tendency to exaggerate the degree of rapport with, and acceptance by, the native communities reported on. However, as Schwab rightly points out, "The anthropologist under the best circumstances never becomes a fully accepted member of the culture he is studying. . . . Usually people look upon him as outside the indigenous culture and do not expect him to follow their cultural norms" (1977:47).

I suggest that in their initial field experience, most anthropologists have tended to exaggerate their supposed adoption as "kinsman" by "their people" in the field. To begin with, kinship is not primarily a status; it is a jural relationship which by its nature implies reciprocal interaction in behavior patterns. One of the attendant obligations of a kinsman, fictive or otherwise, is mutual support in cases of need or conflict. Yet it is a truism that most anthropologists eschew any involvement in personal or intracommunal problems which are extrinsic to the researcher's immediate concerns, nor do they become involved in problems that pertain to members of the local community *vis-à-vis* officialdom. An instructive example of the divergence of interests and expectations between the field researcher and "his people" can be gleaned from Gutkind's graphic description of his encounter with the urban unemployed in a Nigerian city (1969:26-34). As an anthropologist with a strong sense of social justice, Gutkind experienced the dilemma of being confronted by a segment of the community that expected tangible benefits as reciprocity for information provided about labor conditions, while as an ethnographer, Gutkind strove to maintain a posture of scientific noninvolvement in the affairs of the people. The unemployed locals apparently construed Gutkind's scientific interest in them as sympathy for their economic condition for which he might be able to provide help; that is, to find work for them. Obviously, the native informant would consider such reluctance to reciprocate on the part of a "kinsman" as a serious breach of the "axiom of amity" (Fortes 1969:Chapter XII).

IMPLICATIONS FOR ETHNOGRAPHIC RESEARCH

The main purpose of this paper has been a critical reevaluation of ethnographic methodology by focusing on the role of the anthropological field-worker as a stranger in the field. The

paper has attempted to show, by the evidence of their own reports, that most field-workers have not paid sufficient attention to their role as a datum in the field enterprise, and that this inattention may have deleterious implications for the validity of ethnographic results. Although a spate of methodological literature on fieldwork has emerged in the last two decades, and analytic techniques have been sophisticated, the subject of informant-ethnographer relations as *cultural data* has not been treated as a focal point in the ethnographic enterprise. Yet the validity of field data cannot be any more reliable than the quality of their sources.

If anthropological reports about remote societies are to have a reasonable degree of validity and reliability, then field-workers must recognize the severe limitations of their sources of data and remain wary of distortions which will probably result from the ineluctable impress of their "disciplinary culture." In social and cultural anthropology, the field-worker confronts the academic community with "social facts" presented as data whose reliability rests on the *bona fides* of the researcher. The researcher's own acceptance of these "social facts" depends in turn on faith in "key informants." To be sure, fellow anthropologists and the scholarly community can and do subject *stated* cultural "facts" and authors' analyses of these facts to rigorous assessment according to the prevailing canons of scientific tests. Nevertheless, the scientific community cannot vouch for the validity of raw data. In order to do so adequately, they would need sufficient factual data on the ethnographic enterprise. As Winch has pointed out:

> To understand the activities of an individual scientific investigator we must take account of two sets of relations: first, his relation to the phenomena which he investigates; second, his relation to his fellow scientists. (1958:84)

Schneider states this point more pertinently by focusing on the ethnographer's process of "sifting" empirical facts:

> When we read about kinship in some society foreign to our own we have only the facts which the author chooses to present to us, and we usually have no independent source of knowledge against which we can check his facts. It is thus very hard to evaluate his theory for ordering those facts. (1968:vi)

Deleterious implications for an anthropology based on field reports of doubtful validity extend beyond the scientific community. What field-workers publish may be difficult for colleagues who are remotely removed from the communities in question to validate with any certainty. However, this is not the case for members of "native" communities who may be able to verify raw facts and

question interpretations of cultural data. Gross or even unintentional distortions of a culture may lead to the development of hostile reactions toward ethnographic research. Brislin and Holwill (1979) raise a timely question in this respect: "Why are social scientists, who have developed such fields as human relations and culture communication, disliked in many circles among the people they study?" The answer they advance is that "social scientists have not systematically examined the opinions about their writings that are held *by those written about*" (1979:65, emphasis original).

Brislin and Holwill have compiled a corpus of data based on their survey of "Indigenous Views of the Writings of Behavioral/ Social Scientists: Toward Increased Cross-Cultural Understanding" in which they sought "insiders' opinions" of standard ethnographic works. Assessments were made on the basis of the accuracy of ethnographic observation, location, information, interpretation, and reporting. A sample of native opinion on selected standard ethnographies yielded the following observations:

> I think the person in our culture who (*sic*) the anthropologist worked with only wanted money. That is why the interpretation is not accurate. People who really know should not give secrets to a stranger, only to members of their family. (1979:68-69)

Another person wrote:

> I feel that the author does not know what he is talking about because the author, in talking about Samoa, mentioned . . . work which was done in Manua. Western Samoa is very different from Manua and American Samoa. (1979:69)

For authors who complain about the habitual indirect response of natives, the following insider's comment provides a more plausible and verifiable cultural explanation than the psychological assumptions which are often suggested in the literature:

> We (Laotians) consider it polite, Westerners consider it not frank . . . Being in that kind of society with these values, we have learned to understand the message that other people are sending without it being stated in words. It is not so much what you say but the way you say it that counts. (ibid.)

A case in point is illustrated by errors made by the late Margaret Mead in her Samoan data as revealed in a review by Derek Freeman (1972:70-78). Freeman gives a detailed comparison of the ethnography of native terms and translations provided by an

informant named Mea with native explanations for these same terms and translations which were collected by further cross-checking with other informants. This exercise reveals: (a) the pitfalls that a stranger-ethnographer is likely to encounter in an alien culture; (b) that native informants may not be aware of, or able to delineate subtle linguistic nuances when interpreting terms for an ethnographer; and (c) that the symbolic cultural transformation which a linguistic term undergoes due to context may be missed.

The anthropological record is replete with evidence of misdecoded verbal and nonverbal behaviors of members of unfamiliar cultures which has resulted in unwarranted assertions and value judgments about such cultures. The clue to this dilemma may lie in our ethnocentric designation of other cultures as "simple societies," implying that they are "easy to understand." But are they? Holistic-empiricist attempts to understand simple societies suggest that the answer is a resounding "no." Anthropological ethnocentrism *vis-à-vis* research communities is all the more dangerous precisely because it often goes unchallenged or is clothed in the garb of "scientific" detachment. We paternalistically write of "my people," "my tribe," or of ourselves as the "prestigious visitor," "honored guest," and "gracious anthropologist" without conceding our veiled "superiority" over the people we study. Whether or not an ethnographer employs the subjective and pejorative terms "savage," "primitive," "backward," "nonliterate," "technologically simple," "uncivilized," "non-Western," or "small-scale" societies, all such euphemisms entail the same conceptual image of the research communities as manipulative objects to be studied at will.

Ethnographers have often confessed to being irritated by natives who do not live up to an image of the good native. The native, not the stranger and unwelcomed visitor, gets the blame:

> . . . the anthropologist may find it difficult getting honest answers to his questions, and may suffer the unwanted attentions of local snobs who see themselves as heralds of a new and progressive order. Several of these, dressed in European fashion and aggressively mouthing their Portuguese, used to crowd around us during our first visits to the hamlets. But when they realized we valued real Tetum customs above pseudo-European ones, they left us alone. (Hicks 1976:14)

Few anthropologists would honestly concede the right of a "native" community to object to being studied without the ethnographer viewing such action as a threat to his project. Hence, the often caustic comments leveled at intractable and uncooperative "informants" in ethnographies (Chagnon 1968; Foster 1979; Rabinow 1977; Turnbull 1972).

In conclusion, only an unsuccessful stranger-host relationship is likely to compel a noted anthropologist to depict a research community as "this strange and outwardly rather horrid society," whose two catechists "Both wore those indisputable signs of Christianity and progress, pants, and . . . carried rosaries to show how holy they were"; a society moreover that appears "to have disposed of virtually all the qualities . . . that differentiate us from other primates," and in whom the ethnographer could not find "one lingering trace of humanity." The ethnographer could not believe he "was studying a human society; it was rather like looking at a singularly well-ordered community of baboons" (Turnbull 1972; 114, 228-229, 234, 236). This may represent an extreme case of cathartic reporting, but is illustrative of the truism that anthropological research "makes extreme demands on imaginative intelligence," as Rodney Needham has recently pointed out, "and in the crucial setting of field research it can subject the investigator to the severest intellectual and moral test . . . of his life" (1981:29). It is thus reasonable to suspect that much to the detriment of the discipline, most of the unsatisfactory results from fieldwork are due to difficulties in stranger/host relationships.

EPILOGUE

A commonplace reaction to critical comment is to demand that the critic offer a better mousetrap. But the role of critical analysis is not to revolutionize literature or art by substituting himself as a better author or artist. As is true for the role of culture in human development, the viability of anthropological fieldwork as a principal tool in ethnographic research rests on the complementary contribution of all of its diverse practitioners, and not on the individual prescriptions of only one of its adherents who can offer no more than a pointer to some of the shortcomings in the method of data collection and presentation.

There is no panacea suggested here apart from the need for ethnographers to specify and validate their data bases in a manner that increases the reliability not only of the proffered analysis of alien cultural phenomena, but also of the raw "social facts" constituting the data base. The stuff of anthropological fieldwork are the field journal and field notes, and since monographs are extrapolations of the content of these records, it is essential that all material for clarification of otherwise unverifiable assertions ought to be adduced from these records in the absence of other "tests of goodness."

Due to imprecise presentation of field reports, we often cannot distinguish between emic normative statements and etic explanatory assertions. Fieldwork as qualitative research is not mutually exclusive of the quantitative aspect; yet data are often

vaguely presented as "native opinion holds that. . . ."; "villagers state that. . . ."; or "sacred twines represent the unity of the tribe" without any corroborative evidence as to the sample population that constitutes the basis of these assertions, the number of informants who agree or disagree over such "facts," or how many households conform to or deviate from a stated norm. Only the field-worker can supply such substantive clarificatory data, and those who take care to do so enhance the reliability of their fieldwork.

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