# Ethnographic Fieldwork

## AN ANTHROPOLOGICAL READER

Edited by

*Antonius C.G.M. Robben and Jeffrey A. Sluka*

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## An Anthropological Reader

*Edited by*

Antonius C. G. M. Robben and
Jeffrey A. Sluka

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To our mentor and teacher Gerald D. Berreman,
who taught us about ethics and "hanging out" in the field

# About the Editors

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We want to express our gratitude to our editor Jane Huber and series editor Parker Shipton for entrusting the editorship of a reader on ethnographic fieldwork to us. No other discipline in the humanities and social sciences has reflected more deeply on its methodology than cultural anthropology. Ethnographic fieldwork touches on the very core of anthropology and continues to be a rite of passage for aspiring practitioners. Aware of our professional responsibility to compose a reader that is representative of the field, we have drawn on the expertise of more than a dozen colleagues to arrive at the present selection of articles, essays, and book excerpts. Most helpful were the ten anonymous reviews of our initial proposal, leading us to make significant changes. The refashioned proposal was further readjusted after detailed comments from Parker Shipton at Boston University, Jeff Sissons at Massey University, and Geert Mommersteeg and Jan de Wolf at Utrecht University. Parker Shipton and Geert Mommersteeg deserve a particular mention for responding quickly to additional questions and for volunteering many bibliographic suggestions. Their generosity is uncommon in a time of overworked academics. The Blackwell staff in Malden, Massachusetts, have been unsurpassable. Not once did Jane Huber lose her patience and good cheer as we procrastinated over our selection of texts and asked her yet again for permission to move the deadline. Emily Martin has accompanied the production process with great professionalism and reminded us in the most gentle of ways about pending editorial queries. Finally, we want to thank our excellent copy editor Juanita Bullough and Editorial Controller Angela Cohen for their flawless work.

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# Fieldwork in Cultural Anthropology: An Introduction

*Jeffrey A. Sluka and Antonius C. G. M. Robben*

To understand a strange society, the anthropologist has traditionally immersed himself in it, learning, as far as possible, to think, see, feel, and sometimes act as a member of its culture and at the same time as a trained anthropologist from another culture. This is the heart of the participant observation method – involvement and detachment. Its practice is both an art and a science. Involvement is necessary to understand the psychological realities of a culture, that is, its meanings for the indigenous members. Detachment is necessary to construct the abstract reality: a network of social relations, including the rules and how they function – not necessarily real to the people studied. Fieldwork is a deeply human as well as a scientific experience and a detailed knowledge of both aspects is an important source of data in itself, and necessary for any comparative study of methodology. (Powdermaker 1966:9)

For young graduate students preparing for their first field trip, fieldwork represents mystery, opportunity, and excitement. Fieldwork is also a trial through battle in a war for which the novice has little preparation. The student knows that this is a challenge he will have to face, a major rite of passage that will provide him with the opportunity to prove his ability, courage, and temperamental suitability for the profession. He knows that, in doing fieldwork and in working with the ethnographic data he will collect, a number of transformations will occur... Much like the rites of passage of many primitive societies, success in fieldwork is more a function of personal ability than of previous training in specific techniques. Success in fieldwork proclaims manhood and generates a major transformation: a student of culture becomes an anthropologist. (Freilich 1970:16)

Fieldwork *is* the central activity of anthropology. (Howell 1990:4)

One of the most enduring, perhaps *the* most enduring, metaphors, or "keywords"... in modern anthropology is "fieldwork"... "fieldwork" is – it goes without saying, and thus must be said – the *sine qua non* of modern anthropology, the ritual initiation experience in the discipline. (Berger 1993:174)

> The tradition of fieldwork, and a conceptual repertoire derived from deep immersion in local ways of life, has been the source of anthropology's strength. (Keesing and Strathern 1998:7)

These opening quotations, spanning the last half of the twentieth century, amply demonstrate the enduring central place of fieldwork in cultural anthropology. Every discipline has its own particularity in methods of gathering data. In cultural anthropology it is fieldwork based on participant observation, which hinges on the dynamic and contradictory synthesis of subjective insider and objective outsider. As an insider, the fieldworker learns what behavior means to the people themselves. As an outsider, the fieldworker observes, experiences, and makes comparisons in ways that insiders can or would not. Nearly every professional anthropologist has undertaken fieldwork, and it is still viewed as the characteristic rite of passage into professional status in the discipline. Anthropologists "pioneered" ethnographic fieldwork, and cultural or social anthropology remains the only social science that relies on fieldwork based on participant observation as its central method. One of the main developments in cultural anthropology since the 1970s has been an increasing readiness of anthropologists to talk candidly about their experiences in the field, their relationships with informants, and the contexts in which they gathered their material. While in the past some dismissed these reflections on fieldwork as self-indulgent "navel-gazing," it is now generally accepted that they have made an important contribution to the discipline because we can better understand and evaluate an ethnographic text if we know something about the writer, the experiences upon which the text is based, and the circumstances of its production. Furthermore, these reflections turn anthropologists into better fieldworkers by making them aware of their own practices, emotions, biases, and experiences.

Today, "the field" has broadened to include everywhere there are human beings, and every imaginable human group and context has become an actual or potential research site. Cultural anthropologists are conducting fieldwork in institutional and many other settings, with "little people" (dwarves), perpetrators and survivors of violence in war zones, elderly patients in hospices, HIV patients, firefighters, drug dealers, political militants, new religious movements, motor-home nomads, Olympic athletes, poker players, and modern Japanese geishas. Reports on fieldwork are a regular feature in *Anthropology News*, the newsletter of the American Anthropological Association. For example, the May 2005 issue highlighted the expanding "new venues" of fieldwork. Edward Bruner (2005) reported on fieldwork traveling with tourists and visiting tourist destinations, Linda Scarangella (2005) discussed fieldwork at the Buffalo Bill's Wild West Show attraction at EuroDisney, and Jonathan Marion (2005) described multi-sited fieldwork with ballroom dancers at their studios and competitions. And when we say that every *imaginable* human context can be a potential fieldwork site, we mean it literally, since there is already a speculative "space" or "extraterrestrial" anthropology preparing for future fieldwork on space stations, spacecraft, and proposed human settlements on the Moon, Mars, and beyond, including the possibility of fieldwork with intelligent nonhuman extraterrestrials (see Maruyama and Harkins 1975).

In the first decade of the new millennium, fieldwork has become even more central to the discipline, to the extent that there are claims that it is the *only* common ground that holds it together. While this would seem to be an exaggeration, since fieldwork is only one of several key shared "paradigmatic" elements (described below) that have always distinguished the anthropological perspective, it again stresses the importance of fieldwork in the discipline today, and helps establish the *raison d'être* for this reader.

Today, there is a vast and ever-expanding literature on the practical experience of fieldwork, covering many disciplines and a wide range of field situations in most parts of the world. Learning how to conduct research based on participant observation has become a priority for scholars in an expanding number of disciplines beyond anthropology and sociology, including folklore, economics, psychology, geography, history, education, social work, nursing, and development studies, all of which have now published their own texts on fieldwork methods and edited volumes of fieldwork accounts (e.g., Jackson 1987, Delamont 2002, Scheyvens and Storey 2003, Devereux and Hoddinott 1992, Jackson and Ives 1996, Lareau and Shultz 1996). Along with dozens of books and edited volumes, there are hundreds if not thousands of articles on fieldwork in journals and other publications. Evidence of the emerging cross-disciplinary "trendiness" of fieldwork is the publication between 1999 and 2005 of five separate major multi-volume sets on ethnographic fieldwork, totaling 21 volumes (Pole 2005, Denzin and Lincoln 2001, Bryman 2001, Bryman and Burgess 1999, Schensul and LeCompte 1999).

Beginning in the 1960s, a steadily increasing flow of edited volumes of fieldwork accounts began to emerge, and in the 1970s so too did book-length monographs centering on a particular anthropologist's fieldwork experiences and edited volumes on anthropological fieldwork (see Appendices 2 and 3). In cultural anthropology there are now numerous publications on nearly every imaginable aspect of fieldwork, including *ethics and fieldwork* (e.g., Rynkiewich and Spradley 1976, Appell 1978, Fluehr-Lobban 2003, Caplan 2003), the *psychology of fieldwork* (e.g., Wengle 1988, Hunt 1989), *field notes* (Sanjek 1990, Emerson, Fretz, and Shaw 1995), *women in the field* (e.g., Golde 1970, Roberts 1981, Cesara 1982, Altorki and El-Solh 1988, Marcus 1993, Yu 1997), *feminist issues in fieldwork* (e.g., Wolf 1996), *sex and gender in fieldwork* (e.g., Whitehead and Conaway 1986), *intimacy and erotic subjectivity in fieldwork* (e.g., Kulick and Willson 1995, Markowitz and Ashkenazi 1999, Wolcott 2002), *gay and lesbian anthropologists in the field* (e.g., Lewin and Leap 1996), *fieldwork and globalization* (e.g., Robbins and Bamford 1997), *occupational safety and health in fieldwork* (e.g., Howell 1990), *violence and fieldwork* (Lee 1995, Nordstrom and Robben 1995, Greenhouse, Mertz, and Warren 2002), *long-term fieldwork* (e.g., Foster, Scudder, Colson, and Kemper 1979, Kemper and Royce 2002), *fieldwork and friendship* (e.g., Chiñas 1993, Kumar 1992, Grindal and Salamone 1995, Ridler 1996, Powdermaker 1966), and *emotions and fieldwork* (e.g., Kleinman and Copp 1993). There are *humorous accounts of fieldwork* (e.g., Barley 1983, 1985, 1988, Borgerhoff Mulder and Logsdon 1996), and books and volumes on fieldwork in particular countries (e.g., for Japan see Hendry 1999, Bestor, Steinhoff, and Bestor 2003) and most regions of the world (e.g., for the Pacific see DeVita 1988). In fact, so much has been written about fieldwork since the 1970s that in 1988 Gravel and Ridinger published an annotated bibliography of just *anthropological* fieldwork that included 700 entries, and that number has probably doubled since then. Of particular value has been the publication of a large number of reflexive personal accounts of field experiences (see Appendices 2 and 3).

The articles in this collection have been selected from among this wealth of publications because they belong to the standard knowledge of most experienced fieldworkers in the discipline, and deserve to be passed on to a new generation. This reader is intended to help facilitate this goal by providing students with an historical, methodological, ethical, reflexive, and stylistic context to fieldwork, and is ideally suited to be read in conjunction with a text on formal research methods (see Appendix 4) and an ethnography which includes a reflexive account of the fieldwork on which it is based (see Appendix 3). These articles present qualitative, subjective, and reflexive perspectives on fieldwork, and serve to develop the reader's knowledge and appreciation of the perspectives, approaches, methods, problems, experiences, and ethics involved in anthropological fieldwork. This collection aims to provide readers with a good sense of classical and contemporary reflections on fieldwork, the tensions between self and other, the relationships between anthropologists and research participants, conflicts and ethical problems, various types of ethnographic research, and different styles of writing about fieldwork. It is about *fieldwork* in general rather than formal "fieldwork methods," because the intention is to not only highlight the central role of fieldwork in anthropology, but also to explore its wider significance to the discipline.

This introduction provides a basic introduction to fieldwork from the perspective of cultural anthropology, including how it fits in the disciplinary paradigm and has been defined. It includes, furthermore, an overview of the history of fieldwork, a review of fieldwork in the postmodern era, a discussion of culture shock and the taboo against "going native," a general literature review and many suggestions and references for further reading and exploration of this topic, and, finally, a description of the construction and structure of the reader itself.

## Fieldwork in the Anthropological Paradigm

Fieldwork represents one of the fundamental or "paradigmatic" elements of anthropology as an academic discipline. Thomas Kuhn (1962) argued that a paradigm was a conceptual model of an academic discipline, the learning of which represented the socialization process through which students are prepared for membership in a scientific or academic discipline. The basic paradigm of anthropology, outlined below, shows where fieldwork "fits in" or integrates with the rest of the core elements that define the discipline:

1 Culture: The core concept of cultural anthropology.

2 Fieldwork: The primary method of cultural anthropology. Long-term *fieldwork* based on participant observation. A "field" as opposed to "laboratory" science.

3 Cross-cultural comparative perspective: A scientific approach in anthropology, looking for regularities, patterns, generalizations, "rules," or "laws" concerning human and social behavior. Macroanalysis. Generalizing.

4 Ethnography: A case-study approach. In-depth study of the culture of a people, group, or community. Microanalysis. Particularizing.

5 Holistic perspective: Looks at human beings from all perspectives, stresses the interrelationships between different aspects of life, and asserts that any culture has

to be understood not only in its local manifestation, but in relation to the wider global context in which it occurs.

6 **Eclectic approach:** Eclectic means "to pick and choose," in this case both topics and theories. There is a very wide variety of interests and no dominant theoretical model.

7 **Humanistic perspective:** "Humanism" focuses on people, with the applied aim of reducing suffering and improving the human condition. Referred to as "the Enlightenment vision." Includes anthropology as a form of cultural critique and advocacy.

8 **Scientific approach:** Empiricism and the scientific method. "Science" refers to the observation, identification, description, experimental investigation, and theoretical explanation of natural phenomena. Based on direct experience and observation.

9 **Combined subjective/objective perspective:** Equally interested in the subjective, participant's, actor's, or "emic" perspective and the objective, observer's, anthropologist's, or "etic" perspective.

These nine elements are *interrelated* – for example, one can see how fieldwork is affected by each of the other eight elements – and one of the central characteristics of the anthropological paradigm is that it sets up, and is based upon, a series of conceptual and theoretical oppositions, including:

*Ethnography (microanalysis)/Cross-cultural comparison (macroanalysis)*
*Differences (particularizing)/Similarities (generalizing)*
*Synchronic (the "ethnographic present")/Diachronic (long-term)*
*Humanism/Science*
*Participant (subjective)/Observer (objective)*

Since the earliest days of the discipline, it has been a fundamental theoretical tenet that the dynamic tension created by these oppositions has been the wellspring or fundamental source of the "anthropological imagination," and produces a unique perspective and important insights distinct from any other social science perspective.

## Humanistic and Scientific Approaches: Fieldwork as Both Art and Science

> Anthropology is the most humanistic of the sciences and the most scientific of the humanities.
>
> – Alfred L. Kroeber (1876–1960; date of quotation unknown)

Traditionally, the self-image of anthropologists has been that of humanists and scientists, and they did not see any necessary contradiction between the two approaches. Eric Wolf is famous for his assertion that anthropology is a "bridging" discipline between the sciences and humanities, and his observation, after Kroeber, that anthropology has been "a bond between subject matters . . . in part history, part literature; in part natural science, part social science; it strives to study men from within and without . . . the most scientific of the humanities, the most humanist of the sciences" (1964:88). This conception of the dual nature of anthropology was described by Hortense Powdermaker in her classic fieldwork account, *Stranger and Friend*, in these terms: "There are some anthropologists who rather arbitrarily reserve the label 'scientific' for nonhumanistic studies. A scientific attitude ignores no level of understanding . . . If the dual nature of anthropology – an art and a science, a humanistic science – is accepted, there is no reason why each cannot be expanded. The inherent ambiguities of this approach are only a reflection of those which exist in life itself" (1966:306).

<u>Humanities – Sciences Continuum in Anthropological Fieldwork</u>

| *Humanities – Fieldwork as an Art Form* | *Sciences – Fieldwork as a Scientific Method* |
|---|---|
| Humanistic | Scientific |
| Qualitative | Quantitative |
| Subjective | Objective |
| Participant (emic) | Observer (etic) |
| Postmodernism | Positivism/empiricism |

This point is stressed here because, as stated above, this reader is about *fieldwork in its most general sense*, rather than formal "fieldwork methods," because the practice of fieldwork has proven to be as much an art as a science. If anthropologists are "scientific instruments" they are also human ones, and successful fieldwork requires both well-developed research skills and interpersonal relations skills. This has long been recognized in the discipline. For example, in the 1960s, Hortense Powdermaker observed that "In addition to a capacity for open involvements and for becoming detached from them, personal qualities such as kindness, patience, tact, endurance, and the ability to 'take' both loneliness and ambiguity are helpful. Other idiosyncratic characteristics may be useful in one field situation and not in another" (1969:420). Similarly, Charlotte Seymour-Smith (1986b:429) refers to "the imaginative leap involved in coming to terms with an alien culture or way of life." Can a formal methods approach teach or train aspiring fieldworkers qualitative cognitive and interpersonal skills – Powdermaker's "idiosyncratic characteristics" – such as how to be kind, tactful, and empathetic, how to be accepted and get along with people who are different from you, or how to make the "imaginative leaps" Seymour-Smith says are required for successful fieldwork? This is not to say that formal methods training is not useful, but only that it is incomplete.

## Fieldwork Defined

A good way to introduce fieldwork is to begin by considering how it has generally been defined in the discipline. This can be done by reviewing some of the most popular definitions as presented in (1) the *International Encyclopedia of the Social Sciences*, (2) a popular introductory anthropology text, and (3) a leading anthropology dictionary. These also show how the definition of fieldwork has gradually changed, with the inclusion of new concerns and ideas about ethics, politics, reflexivity, collaboration, and reciprocity.

## *"Fieldwork," from the* International Encyclopedia of the Social Sciences *(1969)*

Hortense Powdermaker's entry provides a classic definition of fieldwork which draws upon her *Stranger and Friend: The Way of an Anthropologist* (1966), probably the most widely read book-length study of anthropological fieldwork which discusses the mistakes, successes, and complex emotions of conducting research in Melanesia, Northern Rhodesia, and the United States:

> Fieldwork is the study of people and of their culture in their natural habitat. Anthropological fieldwork has been characterized by the prolonged residence of the investigator, his participation in and observation of the society, and his attempt to understand the inside view of the native peoples and to achieve the holistic view of a social scientist... The publication of Malinowski's *Argonauts of the Western Pacific* in 1922 revealed the great potentialities of fieldwork. This study of Trobriand Islanders, among whom Malinowski had lived for almost three years, set new standards for fieldworkers which continue to operate. Fieldwork came to mean immersion in a tribal society – learning, as far as possible, to speak, think, see, feel, and act as a member of its culture and, at the same time, as a trained anthropologist from a different culture. (Powdermaker 1969:418)

Powdermaker went on to discuss preparation for and starting fieldwork; the advantages and disadvantages of solo, family, and team research; approval and cooperation of authorities; gaining the good will and consent of research participants; the first steps of the fieldwork process; participation in the culture; objectivity; and the influence of the theoretical orientation of the fieldworker.

Powdermaker concluded by identifying an emerging interest in "the fieldworker as an individual": "A quite new development, not yet strong enough to be called a trend, is the recognition that the fieldworker is himself an inherent part of the situation studied and that his personal as well as his scientific reactions are an important part of the research process" (Powdermaker 1969:422). She argued that anthropology has failed to grasp the scientific importance of the ethnographer's social role and personal characteristics, while giving short shrift to chance, mistakes, fieldwork's trial-and-error approach, and the interpersonal dynamic with the people under study. In the following decade, this "emerging interest" would evolve into the full-fledged "reflexive trend" – one of the most significant modern developments in the anthropological approach to fieldwork (see Béteille and Madan 1975, Rabinow 1977, Ruby 1982).

## *"Fieldwork," from* Cultural Anthropology: A Contemporary Perspective *(1998)*

In the third edition of their popular introductory cultural anthropology textbook, Roger Keesing and Andrew Strathern define fieldwork as follows:

> For most anthropologists, the immediate problems of understanding and the sources of data come from what has come to be known as fieldwork: intimate participation in a community and observation of modes of behavior and the organization of social life. The process of recording and interpreting another people's way of life is called **ethnography**...

> Whether the setting is city, town, village, or jungle hamlet, the mode of anthropological research is in many important respects the same. Most essentially, it entails a deep immersion into the life of a people. Instead of studying large samples of people, the anthropologist enters as fully as possible into the everyday life of a community, neighborhood, or group... One learns by participant observation, by living as well as viewing the new patterns of life. Successful fieldwork is seldom possible in a period much shorter than a year, especially where a new language and culture must be learned. Ideally, the researcher stays a good deal longer, sometimes on several successive field trips. The value of fieldwork sustained over a long period is beginning to come clearly into view. Sustained and deep research yields insights into a culture, and into the processes of continuity and change, scarcely attainable any other way. (Keesing and Strathern 1998:7–9)

In their definition, Keesing and Strathern stress that fieldwork is a two-sided encounter between research participants and ethical researchers:

> Consider the encounter from the other side. A people's lives are interrupted by a strange foreigner, often with a family, who moves into the community... This person seldom fits the types of foreigners the people have learned to deal with previously – missionaries, traders, government officers, politicians, or whatever. The newcomer is insatiably curious about things private, sacred, and personal, for reasons and motives that are incomprehensible. The person must be accorded a role of some sort; clumsy efforts to speak, bad manners, and intrusions into daily life must be tolerated. All this attention may be flattering, but it may breed suspicion, hostility, and jealousy. In a less isolated and more sophisticated community, "being studied" may smack of condescension and may offend pride, not arouse it.
>
> On the anthropologist's side, ethical problems loom large. Should one try to protect the identity of the community and its people by disguising names and places? Can one intervene in matters of custom and health? Can one betray the confidence of one's informants in some grave violation of the law? (Keesing and Strathern 1998:8–9)

Finally, Keesing and Strathern draw attention to the gap between the data described in field notes and the lived experiences, sounds, smells, and scenes that cannot be captured in writing but are sedimented in the unconscious. Their use of the term "unconscious" is somewhat misleading because it suggests that the fieldworker is unaware of these impressions, but Keesing and Strathern are right in arguing that the anthropologist draws upon an unpronounced background understanding that is larger than the field notes, and gives the ethnography a richness which could not have been obtained any other way than through an extended immersion in the field.

Keesing and Strathern conclude their brief introductory discussion of fieldwork by observing that anthropology has retained its "humanistic vision" because it remains based on the fundamental intimacy of face-to-face research (1998:10).

## "Fieldwork," *from the* Dictionary of Anthropology *(1986)*

Charlotte Seymour-Smith defines fieldwork in this way:

> **fieldwork.** Research undertaken by the anthropologist or ethnologist in a given ethnographic area or community. Such an ethnographic area in modern anthropology is not

> necessarily limited to the traditional tribal or peasant community, and may embrace studies of urban, industrial or other settings which the anthropologist selects for the purposes of intensive research. The anthropological perspective has similarly been employed in the study of subcultures and in institutional research within modern industrial society. So, while it was once true to say that anthropology was the study of people considered to be primitive, of exotic and little-known tribal cultures, and of peasant communities, modern anthropological research can no longer be defined by this criterion and must be defined instead by the application of its distinctive methods of fieldwork and analysis. In many cases, however, disciplinary boundaries become blurred in the study of modern industrial and urban society, due to the emergence of novel theoretical and methodological syntheses resulting from interdisciplinary collaboration and interchange. (Seymour-Smith 1986a:117)

Seymour-Smith then focuses on ethical and political issues in fieldwork, and suggests that anthropologists might offer "to perform some useful or valued service in return for the collaboration he or she requires" (1986a:117). This call for *reciprocity* as an ethical requirement of fieldwork is a modern one, and Seymour-Smith notes that anthropologists are increasingly asked to legitimate their research before the communities under study, national politicians, administrators, and intellectuals, while being summoned to share the results and produce tangible benefits.

Seymour-Smith's discussion incorporates the emergence of the "reflexive trend" in cultural anthropology, and recognizes the beginnings of new forms of fieldwork with built-in reciprocity then emerging as a response to the ethical issues and criticisms of anthropology raised by research participants – the ethnographic "other" – that exploded in the discipline during the 1960s and 1970s. Reflexive anthropology turns the fieldworker's ongoing negotiation of his or her professional role into an object of study, analyzes the power relations involved, and questions the nature of participant observation.

Seymour-Smith also discusses the issue of neutrality, observing that "he or she may not always be able to conserve neutrality or may under certain circumstances feel that neutrality is not an ethically acceptable position" (1986a:118), and addresses the emerging politics of modern fieldwork:

> In many Third World nations, intellectuals and representatives of indigenous peoples and other oppressed or subordinate groups share the general view of western anthropology either as a form of espionage or an account of "folklore" and exotic customs perpetuating a totally false image of their national reality and the real problems of their minority groups. Anthropologists also enjoy a poor reputation for ethical behavior, and have been criticized principally for their lack of commitment to the welfare of the people they study, and the failure to share the results of their research . . . It is natural enough for people who observe the anthropologist, comparatively wealthy by local standards and apparently free to pursue the line of research he or she chooses, to resent what they regard as the exploitation of the local community for the purposes of advancing his or her own career at home, placing the goals of his or her individual research project over and above any commitment to the aspirations and basic needs of the local population. And it is only to be expected that communities and peoples of the Third World will come more and more to reject this type of investigation and demand that the anthropologist contribute something in return for his or her presence. (Seymour-Smith 1986a:118)

Thus, by the time this article was written in the mid-1980s, the trends in concern for ethics, criticism by research participants, neutrality, and reflexivity in cultural anthropology, which emerged in the 1960s and 1970s, were continuing, and there were new concerns with collaboration, reciprocity, and the politics of fieldwork.

## An Historical Outline of Fieldwork

> The way to do fieldwork is never to come up for air until it is all over.
>
> – Margaret Mead (1901–78; source unknown)

In *Doing Fieldwork: Warnings and Advice* (1971:21–41), Rosalie Wax provided an excellent thumbnail sketch of the history of fieldwork. The following historical outline of the evolution of fieldwork is an elaborated and updated synopsis of Wax's history (see Urry 1984 for another useful history of fieldwork).

Wax began by observing that "Descriptive reporting of the customs, inclinations, and accomplishments of foreign peoples is almost as old as writing itself" (1971:21). Herodotus wrote accounts of the Persians and Scythians for the Greeks, and the Romans continued this practice. With the rise of the Islamic empires, they too began to write descriptions of the foreign peoples they encountered. The first Europeans to collect and record ethnographic data about foreign peoples were Catholic missionaries and frontier merchants. During the last quarter of the nineteenth century, the rapid growth and spread of European and American colonialism and imperialism produced an explosion of ethnographic "fieldwork" and descriptive accounts of the new peoples and cultures encountered during this process. This resulted from the fact that "a good many literate and reasonably well-educated men – governmental officials, administrators, missionaries, and political exiles – were obliged to spend many years and sometimes most of their lives living and working with an alien or 'backward' people" (Wax 1971:23). Many of them learned to speak the native languages and produced excellent field descriptions. In the latter part of the nineteenth century, social research involving the direct observation of groups of people in the researcher's *own* society began to be practiced in Britain and France (the roots of "sociological" fieldwork as opposed to "anthropological" fieldwork with other cultures or peoples).

Wax (1971:28) notes that among the eighteenth- and nineteenth-century philosophers identified as the "pioneers" or "fathers" of anthropology, sociology, and ethnography – such as Saint-Simon, de Tocqueville, Hume, Veniaminov, Smith, Maine, Bachofen, McLennan, Tylor, and Morgan – only Morgan did what could be called genuine fieldwork. Instead, the others relied on secondhand reports and sources. When, in the late 1890s and early 1900s, some British anthropologists – Haddon, Seligman, Rivers, and Radcliffe-Brown – broke with the earlier academic tradition of working only from derived sources and went into the field to obtain data at first hand, they seem to have carried on their investigations more like natural scientists than the fieldworking officials or missionaries from whom the historical theorists had obtained their data. Their funds were meager, their time short, and they were in a hurry. Consequently, they made surveys, looked for and collected interesting or relevant specimens, observed native ceremonies, and questioned native informants as best they could. They were unable to reside near or with an alien people for any length of time to observe or participate in the ongoing cultural and social events, even had this method of doing fieldwork occurred to them. And when Boas and his first students and research assistants began their fieldwork among the Indians of North America, they usually proceeded in much the same fashion (Wax 1971:30).

### *Boas' influence and the American fieldwork tradition*

Franz Boas is renowned as a fieldworker, and considered the "father" of fieldwork in American cultural anthropology. However:

> The fact is that Boas and his students did not customarily spend a great deal of their time in the field learning the native language and observing and taking part in native life. The kind of data they desired did not require this, and, besides, their funds rarely permitted them a field trip of more than a few weeks or, at most, a few [usually the summer] months. What most of them did was try to locate as rapidly as possible a competent informant who was fluent in English and the native language, knew the old stories and customs, and was willing to dictate and translate texts... It is not surprising that the two hardships about which they complain most frequently in their letters are (1) wasting valuable time trying to find or keep a good informant or interpreter and (2) writer's cramp. They were, of course, interested in observing and recording traditional ceremonies and in watching traditional craftsmen at their work. But most of them did not really try to do fieldwork in the style in which it was subsequently done by the British social anthropologists, and they did not, except for rare exceptions, become even moderately fluent in the native language. (Wax 1971:32)

Wax mentions further that Boas made 13 field trips during his lifetime, many of them less than two months long, to 40 different sites along the Northwest Coast. Boas complained in his letters home about the discomforts of fieldwork and the reluctant cooperation of the Indians, yet kept returning to the field because he believed in the utmost importance of collecting firsthand data. Only a few of his students or those who worked in his tradition wrote anything about how they did their fieldwork. Lowie's 1959 autobiography, written just before but published after his death, disclosed the serious difficulties of his first field trip, while a posthumously published collection of Ruth Benedict's articles and letters contains some material on her fieldwork (Mead 1959). They and their generation were reluctant to report openly on any fieldwork problems, afraid to tarnish the reputation of this young science.

### *Malinowski's influence and the British fieldwork tradition*

Bronislaw Malinowski was the first professional anthropologist to describe what intensive fieldwork was really like, and how he obtained his data by living among the Trobriand people to observe their daily lives. His description, published in 1922 as the Introduction to his classic ethnography *Argonauts of the Western Pacific*, is one of the earliest, most widely read, and most influential accounts of fieldwork in anthropology, and is included in this reader. Malinowski suggested that all ethnographic studies should include an account of the research methods and conditions "so that at a glance the reader could estimate with precision the degree of the writer's personal acquaintance with the facts which he describes, and form an idea under what conditions information had been obtained from the natives" (Malinowski 1922:3).

Malinowski is renowned for identifying what he termed "the proper conditions" or "secrets" of effective ethnographic fieldwork. He advocated living among the people under study and remaining far from Westerners. He emphasized the need for clear scientific objectives, a thorough methodology to obtain results, and the use of special data-gathering methods. Finally, he stressed that a fieldworker should stick his nose into all ongoing affairs, even at the risk of offending local etiquette, to discover how the people thought, behaved, and saw the world. Malinowski hoped thus to "grasp the native's point of view" as complementary to the more objective observer's perspective (Malinowski 1922:25).

Inspired by Malinowski's enthusiasm for this "new method," some of his and Radcliffe-Brown's students went into the field and followed this procedure. They and the students they trained produced many of the most valuable and influential ethnographies that emerged in the first half of the twentieth century. However, once again, only a few of these highly trained and experienced fieldworkers wrote anything about their experiences or methods. The most notable are the writings of Evans-Pritchard, Bohannan, and Powdermaker (see Appendix 5 on early and classic accounts of fieldwork). In 1940, Evans-Pritchard published a short but excellent description of the difficulties he experienced doing fieldwork among the Nuer in the early 1930s. In 1954, Laura Bohannan, under the pseudonym Elenore Smith Bowen, published a fictionalized account of fieldwork in Africa which was a pastiche composed of the stories of several people and numerous field trips. And in 1966, Hortense Powdermaker, who had studied with Malinowski in the 1920s, published her detailed account of her fieldwork in four locations. In assessing what she had learned, Powdermaker (1966:287) concluded that the most important conditions for good communication between research participants and fieldworkers are physical proximity of the fieldworker to the people studied, sound knowledge of their language, and a high degree of psychological and emotional involvement. Powdermaker identified the ability to become totally involved and yet to observe with complete detachment, continually to "step in and out" of other cultures, as the key to successful fieldwork. She achieved this ideal to varying degrees in the societies in which she worked (Ellen 1984:97).

## *The influence of the "Chicago School"*

During the 1920s and 1930s, sociologists at the University of Chicago, primarily influenced by the work of social psychologist George Herbert Mead, exerted a considerable influence on the development of sociology and anthropology. Led by Herbert Blumer, William Thomas, Robert Park, Albion Small, Charles Horton Cooley, Florian Znaniecki, and Louis Wirth, they came to be known as the "Chicago School" and represented the first major attempt to conduct systematic ethnographic fieldwork – particularly "community studies" – in urban environments, beginning with Chicago and then spreading to other cities. The sociologists of the Chicago School produced ethnographic monographs about things like Jewish ghettos, taxi-dance halls, professional thieves, hobos, boys' gangs, and the like. They encouraged their students to do fieldwork, and reconceptualized their methodology around "participant observation." By focusing on participant observation, the Chicago sociologists "emphasized their linkage to the tradition of ethnographic fieldwork from Malinowski onward . . . Like Malinowski, they wish[ed] to pitch their tents among the dwellings of the natives (even if these be drug addicts, mental patients, medical students, or rabbis)" (Wax 1971:40).

## Common Subjective Aspects of Fieldwork

In its broader conception, fieldwork in cultural anthropology is characterized by a number of typical subjective or personal experiences. These are usually elided in formal methods texts, but are explored more fully in many reflexive accounts of fieldwork, and frequently arise as topics of discussion in anthropology methods courses. As several of the "pithy quotes" which open this introduction suggest, fieldwork is frequently described as "the professional ethnographer's necessary initiation – variously referred to as a puberty rite, ritual ordeal, or *rite de passage*" (Tedlock 1991:70).

### *Going native*

Barbara Tedlock has observed that the "gone native" fieldworker is one of the archetypes of "the mythic history of anthropology" (1991:69), and Katherine Ewing has noted that:

> The idea of "GOING NATIVE" is one of the few taboos remaining within anthropology. Considered antithetical to the social scientist's stance of objectivity and standing as a professional, *going native* has traditionally been a term of derision among anthropologists. Despite an array of challenges to notions of objectivity, the taboo remains in place and continues to carry a heavy affective load. (Ewing 1994:571)

Powdermaker included a section on "Going Native" in *Stranger and Friend* (1966:115–19), and Barfield states:

> the lore about "going native," that quintessentially anthropological occupational hazard (albeit almost apocryphal), illustrates the advantages and perils of fieldwork. Participating too much results in one's going native; participating too little turns one into a superficial, ethnocentric, survey-wielding, number-crunching social scientist with, some say, zero insight into the people studied. (Barfield 1997:190)

"Going native" is such a central idea in the anthropological tradition of fieldwork that it is not surprising that writer Susanna Kaysen includes it in her novel about an anthropology graduate student doing PhD fieldwork in the Faroe Islands:

> Going bush. Like many anthropological ideas, this one had been born in jungles and savannahs and gave off a tropical scent. Some old-timers called it "troppo," an occupational hazard that began with lassitude and ended by destroying your objectivity. Stories that went, "when we got there, two years later, he was living in a hut with three wives." The trouble was, anthropologists were supposed to be living in huts. The three wives were optional but not exceptional. The line between observer and participant is so fine as to defy detection much of the time. It started easily enough. When members

> of "your" tribe are going on a journey or planning to get married, they slaughter a chicken and read the blood for omens. They offer to read omens for you before your trip back down the river for supplies. What harm can it do? It will certainly give you a better sense of their worldview, and it might give you a little useful information. Pretty soon you have recourse to chicken slaughter every time you have to make a decision. (Kaysen 1990:192–3)

While only a very small number of anthropologists have actually "gone native" in the sense of giving up anthropology altogether to fully join the people they study, the *idea* has a much larger significance in the culture of fieldwork. Perhaps the most widely read example of an anthropologist accused of being at least a borderline case of going native is the experience of Kenneth Good, the author of *Into the Heart: An Amazonian Love Story* (1991). The subtitle of the American edition – *One Man's Pursuit of Love and Knowledge Among the Yanomama* – gives even more emphasis to the impression that there is more to fieldwork than just the fact that it is a scientific method of gathering data. Good first contacted the Yanomama in 1975 while doing fieldwork for his doctorate. He was supposed to stay with them for 15 months, but ended up living with them for more than 6 years, spread over a 15-year period. Eventually, the tribe offered him a wife, Yarima, whom he married. He later took her back with him to New Jersey, where she lived as a "housewife" for 4 years and had three children. But, eventually, Yarima found she could not live in New Jersey; she divorced Good, and left her children there to return to the forest, in what has been described by reviewers (see Amazon.com) as "a fantastic love story."

Good reflected that "in my wildest dreams it had never occurred to me to marry an Indian woman in the Amazon jungle. I was from suburban Philadelphia. I had no intention of going native" (1991:122). Nonetheless:

> Down deep, all I really did want was to find some way to make a living and to get back into the jungle. Not only to study the Indians – I already had enough data for three books – but to live with them. More especially, to live with Yarima. That was what I had come to, after all these years of struggling to fit into the Yanomama world, to speak their language fluently, to grasp their way of life from the inside. My original purpose – to observe and analyze this people as an anthropological researcher – had slowly merged with something far more personal. (Good 1991:145)

In the postmodern era, reflection on the meaning of "going native" has evolved and generated new debates and insights. Where in the past it was a criticism to say an anthropologist had "gone native," today an increasing number of anthropologists, having taken on board the postmodern critique of empiricism and objectivity, advocate rather than criticize the breaking down of the traditional barriers that separate the observer from the observed, and many – including "native anthropologists" – do fieldwork "at home" in their own culture (see Pink 2000). Tedlock suggests that:

> What seems to lie behind the belief that "going native" poses a serious danger to the fieldworker is the logical construction of the relationship between objectivity and subjectivity, between scientist and native, between Self and Other, as an unbridgeable opposition. The implication is that a subject's way of knowing is incompatible with the scientist's way of knowing and that the domain of objectivity is the sole property of

> the outsider. Several fieldworkers have rejected this sharp analytical distinction between Self and Other. (Tedlock 1991:71)

This gives a new and interesting twist: Are anthropologists who are "natives" of the culture they study immune from or even more at risk of the danger of "going native," or do they have an advantage because of their deeper level of participation? Ewing suggests that the taboo against going native is a result of the adoption of the scientific model's commitment to objectivity, but also represents a hegemonic Western practice aimed at control or domination, which has a deleterious effect, because in order to preserve a stance of what they imagine to be scientific objectivity and detachment, ethnographers "may place the act of observing and recording between himself or herself and others" (Ewing 1994:571).

While participation in the culture under study is encouraged, researchers in the field are usually considered to have gone too far – "gone native" – if it comes to constitute a threat to their professional identity. Ewing has explored her experience with this anthropological taboo, describing her deep participation during fieldwork to the point of "belief" or "going native." She recounts how, during her fieldwork in Pakistan, she was the recipient of a dream sent by a Sufi saint, who had earlier told her that he would come to her while she was sleeping. Her initial reaction was rejection of the veracity of the experience, which she attributed to her "internalization of the anthropological taboo against going native, a reaction that I suspect is shared by many anthropologists under similar circumstances and that shapes the ethnographic project" (Ewing 1994:574). She "marveled aloud about the power of suggestion," but finally, before the end of her fieldwork, she chose to visit a Sufi saint "and, for once, to approach the whole encounter as a personal experience rather than as anthropological research" (Ewing 1994:574–5).

Other anthropologists have also pushed the limits of "going native." For example:

> Ethnographers who have learned not only the language but also appropriate behavior (including nonverbal communication codes) have been transformed, sometimes quite radically, by their fieldwork experience . . . David Hayano . . . became so immersed in the subculture of California poker players that "within several years I had virtually become one of the people I wanted to study!" Liza Crihfield Dalby not only took on the social role of a geisha during her fieldwork in Japan, but she also claims to have *become* one in both body and spirit. Her assertion that she learned to think and behave as a geisha suggests the "gone native" archetype of the anthropological imagination. (Tedlock 1991:70)

Similarly, Edie Turner (1999) has recounted her extraordinary experience while observing a ritual practiced by the Ndembu in Zambia. She reported that a "medicine man" named Singleton and his supporters were making a collective effort to draw out an afflicting spirit or *ihamba* from a woman named Meru:

> I felt the spiritual motion, a tangible feeling of breakthrough going through the whole group. Then it was that Meru fell – the spirit event first and the action afterwards. I was clapping and singing with the others like one possessed, while the drums bellowed, and Singleton pressed Meru's back . . . Meru's face in a grin of tranced passion, her back quivering rapidly. Suddenly Meru raised her arm, stretched it in liberation, and I saw with my own eyes a large thing emerging out of the flesh of her back. This thing was a big gray blob about 6 inches across, a gray opaque plasma-like object appear-

> ing as a sphere. I was amazed – delighted. I still laugh with glee at the realization of having seen it, the *ihamba*, and so big! We were all one in triumph. The gray thing was actually out there, visible and you could see Singleton's hands working and scrubbing on Meru's back – and then the thing was there no more. Singleton had it in his pouch, pressing it in his other hand as well. The receiving can was ready; he transferred whatever it was into the can and capped a castor oil leaf and bark lid over it. It was done. (Turner 1999:46)

Did she "go native," hallucinate, or actually experience what she believes she did? These anthropologists did not really "go native" because they never abandoned their anthropological identities. But they were prepared to break the traditional taboo against "going native," and in doing so pushed the boundaries of the self/other dichotomy in anthropological fieldwork.

## *Culture shock*

The common experience of culture shock as a consequence of fieldwork in a foreign culture or context is one of the oldest themes in cultural anthropology. Virtually all fieldworkers have experienced culture shock in varying degrees. This shared subjective experience provides one of the most fundamental bonds that unite anthropologists and the discipline, and it is probably the emotional experience of culture shock that in the main leads to the characterization of fieldwork as a rite of passage. Somewhat surprisingly, in her entry for "fieldwork" in the *International Encyclopedia of the Social Sciences*, Powdermaker dismissed culture shock on entering the field but warned of it on *returning home* from the field:

> Despite its reputation, the so-called culture shock is not often one of the problems of a trained anthropologist [because] he arrives with a general knowledge of the people and their culture through having immersed himself in the literature . . . Culture shock is more likely to be experienced when he returns to his own modern urban society after an extended period in a tribal one. (Powdermaker 1969:420–1)

But in the entry for "fieldwork" in her *Dictionary of Anthropology*, Seymour-Smith identifies culture shock as a characteristic difficulty of fieldwork, commenting that "This state of disorientation is perhaps necessary, and is in the long term a productive one, since like a rite of passage it prepares the ethnographer for the imaginative leap involved in coming to terms with an alien culture or way of life" (Seymour-Smith 1986a:117). In many reflexive accounts of fieldwork, cultural anthropologists have commented on their experience of culture shock and adjustment, and it emerges that one very common or shared response to culture shock in the early phase of fieldwork is withdrawal from contact with the locals and escapism into reading novels, and the like.

In the late 1950s, culture shock was first defined by anthropologist Kalervo Oberg as "the anxiety that results from losing familiar signs and symbols of social intercourse" (1960:177). In a comprehensive analysis, he argued that culture shock was a natural part of the process of adjustment to a new culture which leads eventually to cultural awareness. Oberg identified the signs of culture shock as homesickness, withdrawal, chauvinistic excesses, the stereotyping of locals, the need for excessive amounts of sleep, marital stress, the loss of ability to work effectively, compulsive eating or drinking, unexplainable fits of weeping, irritability, psychosomatic illness, boredom, exaggerated cleanliness, and family tension and conflict. From these observations, Oberg (1974) developed a normative six-month model of the process of cultural adjustment, and made practical recommendations for dealing with culture shock, including learning more about the place and its people, refraining from criticizing the host culture, finding a friend, keeping a sense of humor, avoiding self-pity, getting enough rest, maintaining a healthy diet, getting enough exercise, keeping a sense of adventure, using friends and family for support, asking for help when needed, keeping involved with others and avoiding withdrawal, developing new interests and skills, and recording experiences, insights, and frustrations in a journal.

## Postmodernism and Fieldwork

> All variants of fieldwork have been thoroughly scrutinized and criticized since the mid-1970s. Hyper-positivist fieldwork methods are under fire from postmodernism... Another source of critique is postcolonial studies, which deconstruct how a hegemonic Western social science such as anthropology fashions its particular alterity... *Some anthropologists have found traditional anthropological fieldwork so problematic that they advocate cultural history approaches.* (Barfield 1997:190; emphasis added)

The 1970s and 1980s saw the emergence of postmodern perspectives and increasing debate and eclecticism in cultural anthropology. A heightened awareness of the relationship between power and the construction of knowledge produced a new concern with reflexivity – a "reflexive trend" – and new forms of fieldwork relations and ethnographic writing emerged. There was an increasing trend toward doing fieldwork "at home"; the emergence of interpretive or hermeneutic approaches; the development of feminist anthropology; a growing engagement with indigenous scholars; the emergence of autoethnography, narrative ethnography, indigenous anthropology, and new life-history approaches such as "*testimonio*"; and new practices of writing, communicating, and reading ethnographic accounts as "texts." This last development included a literary turn:

> Declared to be fictions "in the sense of something made or fashioned" (Clifford 1986:6), ethnographies, according to those who advocate this position, are partial or selective truths. They should be approached as texts whose style, rhetoric, and narrative structure can be subjected to the same kind of criticisms to which other works of literature are subjected. Writing about culture raises questions about modes of representation, about objectivity and accountability, relativism and ethnocentrism, science and truth. Textualists urge ethnographers to experiment with new forms of writing that are dialectical, dialogic, or polyphonic rather than analytic, authoritative, and univocal. Indeed, the omniscient "I know because I was there" voice of the post-Malinowskian participant observer is perceived as a trope that is no longer acceptable in a postcolonial world. (Brettell 1993:2)

At a more general level, these trends reinvigorated debate and controversy between protagonists of the universal and relativist, objectivist and subjectivist, scientific and humanistic positions, and were mainly a response to two key developments in the 1960s – the theoretical critique of "neutrality," "objectivity," "truth," and "reality" in empiricism, and the political critique of the discipline's historical relationship with Western imperialism and colonialism. These developments included processes of "decolonization" (Harrison 1991) and the "reinvention" (Hymes 1969) of anthropology, and addressed the politics of the discipline, including such critical issues as the relationship between anthropology and imperialism, academic colonialism, sexism, and issues involved in applied, action, and partisan research (Huizer and Mannheim 1979). One aspect of reflexive postmodern anthropology was a renewed and invigorated interest in the process by which data are gathered – the fieldwork experience.

## *The postmodern critique of fieldwork*

During the postmodern turn in cultural anthropology, some anthropologists, sensitized by the poststructuralist writings of Michel Foucault on power and knowledge, harshly criticized anthropological fieldwork as a kind of invasive, disciplinary "panopticon" – a type of prison building designed by the philosopher Jeremy Bentham in 1791 to allow a guard to observe all prisoners without them being able to tell if they were being observed or not, thus conveying a "sentiment of an invisible omniscience." The anthropological interview was criticized as being similar to the medieval inquisitional confession through which church examiners extracted "truth" from their native and "heretical" peasant parishioners. Finally, they criticized anthropological observation as a hostile act that reduces our subjects to mere "objects" of our scientific gaze. Consequently, some postmodern anthropologists gave up fieldwork and the practice of descriptive ethnography altogether.

As a result of this at times ruthless critique and autocritique of anthropology from historical, political, gender, and postmodern perspectives, even the basic conceptions of the "field" and "fieldwork" – perhaps the most fundamental bases of the anthropological approach – were and are still now being critiqued and contested. An example of the most radical form of postmodern critique is Roger Berger's article "From Text to (Field)work and Back Again: Theorizing a Post(modern)-Ethnography" (1993), in which he argues that

> The "field" is not just produced by the discipline. The ethnographic "field" is also a cultural construct, a disciplinary epiphenomenon, a discursive effect, part of the discursive formation that is anthropology. Without anthropology there is no "field," no "object" of study, *and* without the power to create "fields" there is no anthropology. Indeed, the "field" is a fundamental part of what Foucault terms "the production of truth through power." It is not just the "theoretical framework" that produces ethnographic facts but disciplinary power that enables the production of a "field" in which a conceptual framework can produce "facts." Our normative sense of the "field" functions to hide the power that produces the "field." To put it another way, the "field" is the hidden configuration of western hegemonic power. (1993:178)

The conclusion is that, since "fieldwork" is fundamentally an act of domination and control, and the "field" represents "the dominated bodies of the colonized" (Berger 1993:176), the decolonization of the discipline requires that these be abandoned and new approaches developed.

## *The postmodern turn*

While few cultural anthropologists have abandoned fieldwork, this autocriticism stimulated an experimental era, which continues today, in which new conceptions and ways of doing fieldwork have developed. For example, some postmodern anthropologists see themselves as *artisans* fashioning ethnographies by forming communities and making conversations (Gudeman and Rivera 1995). In *The Art of Fieldwork* (2005), Harry Wolcott advocates approaching fieldwork as an art method; in *Person to Person: Fieldwork, Dialogue, and the Hermeneutic Method* (1996), Barry Michrina and CherylAnne Richards present a "step-by-step" guide for a "dialogical hermeneutic" approach to fieldwork; and volumes such as *Things as They Are: New Directions in Phenomenological Anthropology* (1996), edited by Michael Jackson, *Shifting Contexts: Transformations in Anthropological Knowledge* (1995), edited by Marilyn Strathern, and *Constructing the Field: Ethnographic Fieldwork in the Contemporary World* (2000), edited by Vered Amit, present a wide range of "experimental" articles exploring new ways of approaching ethnographic research.

The "new ethnography" that has emerged in the last few decades incorporates three interlocking concerns. First, an increased awareness of "multivocality" (multiple voices representing multiple interests or "realities"), which has raised issues of signature, authority, and advocacy. Second, a growing number of works concerned with the ethnographic encounter, with cross-cultural communication, and with making explicit the ways in which fieldwork is conducted and research participants are incorporated into the account. Third, an increased regard for the context and praxis of writing and reading ethnographic "texts." For example, one aspect of the postmodern turn is the "revolution" in readership; today, the ethnographic "other" is nearly always literate and can read and respond to – particularly criticize – what anthropologists write about them. As Driessen has observed, "The traditional dichotomy of the ethnographer who looks, listens, writes and reads, versus the native who talks and tries to figure out what the behavior and questions of the anthropologist mean, has largely collapsed" (1993:3). Thus, the elements of contemporary ethnography include experimental form and method, reflexivity on the part of the ethnographer, collaboration and multiple authorship, and the details of fieldwork dialogue and experience.

## *Narrative ethnography*

Modern ethnographers have sought to increase the voice of the "other" through more active involvement of research participants in the co-production of ethnographic accounts, narratives, or texts. This is done by extensive direct quotations, co-authorship, and collaboration with research participants. As Tedlock has observed, beginning in the 1970s, "there was a shift in emphasis from participant observation to the observation of participation" (1991:78), entailing "a representational transformation" in which both the fieldworker and "other" are "presented together within a single narrative ethnography, focused on the character and process of the ethnographic dialogue" (Tedlock 1991:69). Narrative ethnography became a creative intermingling of lived experiences, field data, methodological reflections, and cultural analysis by a situated and self-conscious narrator. Tedlock argues that the development of this new "narrative ethnography" is driven by

> a new breed of ethnographer who is passionately interested in the co-production of ethnographic knowledge, created and represented in the only way it can be, within an interactive Self/Other [epistemological] dialogue. These new ethnographers – many of whom are themselves subaltern because of their class, gender, or ethnicity – cannot be neatly tucked away or pigeonholed within any of the four historical archetypes [of] the amateur observer, the armchair anthropologist, the professional ethnographer, or the "gone native" fieldworker. Rather they, or we, combine elements from all four of these categories. Thus, for example we embrace the designation "amateur," since it derives from the Latin *amatus*, the past participle of *amare*, "to love," and we are passionately engaged with our endeavor. We accept the "armchair" designation because we have a serious concern with both reading and critiquing the work of other ethnographers in order to try to change past colonialist practices. We insist that we are "professional," because of the seriousness of our field preparation and engagement, and also because of our attention to issues of representation in our own work. Finally, to the extent that fieldwork is not simply a union card but the center of our intellectual and emotional lives, we are, if not "gone native," at least becoming bicultural. (Tedlock 1991:82)

## Testimonio

One of the most important forms of narrative ethnography is the emergence of "*testimonio*" or testimonial narrative – a first-person account of a real situation that involves repression, marginalization, and violence, intended to function as a narrative that bears witness to and denounces human rights abuses. Such narratives, most coming out of Latin America since the mid-1970s, have been intended as a way to give voice to the voiceless. The most widely known example is the autobiographical *I, Rigoberta Menchú: An Indian Woman in Guatemala* (1984), which begins with this paragraph:

> My name is Rigoberta Menchú. I'm twenty-three years old. This is my testimony. I didn't learn it from a book and I didn't learn it alone. I'd like to stress that it's not only *my* life, it's also the testimony of my people. It's hard for me to remember everything that's happened to me in my life since there have been many bad times but, yes, moments of joy as well. The important thing is that what has happened to me has happened to many other people too: My story is the story of all poor Guatemalans. My personal experience is the reality of a whole people. (Menchú, with Burgos-Debray 1984:1)

John Beverley has defined *testimonio* and elaborated on its various approaches:

> By *testimonio* I mean a novel or novella-length narrative in book or pamphlet form, told in the first person by a narrator who is also a real protagonist or witness of the event he or she recounts, and whose unit of narration is usually a "life" or a significant life experience. *Testimonio* may include, but is not subsumed under, any of the following categories, some of which are conventionally considered literature, others not: autobiography, autobiographical novel, oral history, memoir, confession, diary, interview, eyewitness report, life history, *novella-testimonio*, nonfiction novel, or "factographic" literature . . . This situation of narration in *testimonio* has to involve an urgency to communicate, a problem of repression, poverty, subalternity, imprisonment, struggle for survival, and so on. (1996:24–5)

In the context of Latin America, *testimonio* developed as an important medium through which subalterns finally found a literary voice of their own. First and foremost, it is considered to be a tactic for empowerment and survival, in which the subaltern brings the existence of oppressive and repressive conditions to the attention of the world to bring about social and political change. Thus, for the work that has been publicized through her *testimonio*, Rigoberta Menchú was awarded the Nobel Peace Prize in 1992. *Testimonio* represents a (to some) controversial (see Stoll 1999), form of experimental ethnography, based on a new approach to fieldwork and a new relationship between the anthropologist and the "other" (Beverley 2004).

### *Reciprocity, collaboration, and partnership*

Another development in postmodern fieldwork, or another approach to the "new ethnography," is an increased commitment to reciprocity and collaborative research. The traditional purpose of fieldwork was to collect ethnographic data, and in the 1960s this position was summarized by Powdermaker when she asserted that "The anthropologist is not primarily interested in helping his informants, although he may do so inadvertently. His motivation is to secure data" (1966:296). However, as noted above, by the mid-1980s there was an increasing commitment to *reciprocity* – to providing something useful back to research participants for their collaboration – as an ethical requirement of fieldwork. Reciprocity became more than an effective practical strategy to please informants and establish a good rapport, and acquired a moral dimension. This led to the emergence of a new type of applied anthropology, in which serving the local community and collecting ethnographic data are equally important, and "the researchers are concerned with conducting research in a team effort that benefits the local people as well as themselves" (Kuhlmann 1992:275). In particular, this has been evidenced in the development of new approaches to ethnographic fieldwork such as collaborative research and research partnerships (see Clifford 1980 and Kennedy 1995) in work with both groups (e.g., Kuhlmann 1992) and individuals (e.g., Marcus and Mascarenhas 2005).

In an article on collaborative research she conducted with members of the Kickapoo tribe of Oklahoma, Annette Kuhlmann states that "the goals of this approach include community participation, collaboration of the participants on all aspects of the project, reciprocal learning processes, and respect for the community's sense of tribal privacy in regard to publications" (1992:274). Kuhlmann worked with tribal members to develop books on Kickapoo history for classroom use. Traditionally, the Kickapoo related their past by passing on stories about events that happened to individuals and the tribe. Through interviews conducted by trained tribal members, these detailed and sometimes humorous life histories and stories were recorded from elders and put into history texts in a way that reflected this tradition.

Kuhlmann identifies the basic elements of participatory research. One approach is to look to enhance research people may already be doing themselves. The orientation of this collaborative research

> entails an attitude on the part of the outside professionals that their work and their relationships with the local members are governed by a commitment to the people, to work *with* them, i.e., not for them and not about them . . . This emphasis distinguishes collaborative research from other applied approaches in which the academic works *for*

> a specific employer, such as the federal government, another organization, or the tribe. Employer–employee relationships imply a hierarchy that the collaborative researchers strive to replace with reciprocity in learning in the context of mutually designed projects. (1992:277)

In collaborative research, the participants attempt to work together as equals, and this teamwork includes every aspect of the project – planning, implementation, problem solving, and evaluation.

Similarly, new approaches to fieldwork based on reciprocity and collaboration have emerged that emphasize *partnership*. This approach has been particularly developed by feminist anthropologists, and an increasing number of fieldworkers are choosing to enter into explicit research partnerships with individuals and groups. Research partnerships between anthropologists and the people with whom they work have already taken a variety of forms (e.g., see Shostak 1981), but generally it involves "partnerships between trained researchers and community members who are not trained in social research but who are interested in doing research into their own community" (Park 1992:581).

One of the best examples that has revealed some of the basic elements of partnership-based research is a project on the place of alcohol in the lives of New Zealand women, based on partnerships between anthropologists and Samoan, Cook Islands, Maori, and lesbian women (Park 1992:581). Julie Park observes that, as in business, research partnerships are negotiated relationships; the partnership does not necessarily have to be equal, but is one of joint engagement based on negotiation (1992:582). Park argues that in order to be nonexploitative, research relationships not only have to be open to people's experiences and voices, but have to return to the participants appropriate and accessible research-based information as part of an empowering process (1992:583). Skills and resources should be shared, and just as in other forms of participatory research, in partnership-based fieldwork it is essential to work in close cooperation with members of the group, including seeking their active involvement in planning, research, and analysis of the project.

## Contemporary Fieldwork in Cultural Anthropology

> Without the continued grounding in the empirical that scientific aspects of our tradition provide, our interpretive efforts may float off into literary criticism and into particularistic forms of history. Without the interpretive tradition, the scientific tradition that grounds us will never get off the ground. (Rappaport 1994:76)

### *The compassionate turn*

At the beginning of the twenty-first century, after more than three decades of intense postmodernist critique, cultural anthropology is passing through another shift in its relation to fieldwork. In the mid-1990s, there arose a growing opposition to the destructive cynicism and negativism of the most extreme forms of postmodern critique, because few anthropologists accepted the radical proposition that fieldwork is inherently a form of imperialism, oppression, and control, which must be abandoned altogether. If modernism (empiricism) represented anthropology's original thesis characterized by a preoccupation with how to analyze and represent cultures, and the "postmodern turn" was its antithesis with an emphasis on the reflexive ethnographer, then lately we are seeing a synthesis of these perspectives in the reassessment of the relation between anthropologists and the peoples they study. The discipline is in the middle of a period of soul-searching about the morality of fieldwork relations and the ethico-political implications of ethnography. One of the best examples of this critical stance toward a self-absorbed anthropology is the work of Nancy Scheper-Hughes, who has written:

> I am weary of these postmodernist critiques, and, given the perilous times in which we and our subjects live, I am inclined toward compromise, the practice of a "good enough" ethnography. While the anthropologist is always a necessarily flawed and biased instrument of cultural translation, like every other craftsperson we can do the best we can with the limited resources we have at hand: our ability to listen and to observe carefully and with empathy and compassion. (1995:417–18)

Most anthropologists today would agree with Scheper-Hughes' conviction that, rather than a "hostile gaze," anthropological fieldwork can be "an opportunity for self-expression. Seeing, listening, touching, recording can be, if done with care and sensitivity, acts of solidarity. Above all, they are the work of recognition. Not to look, not to touch, not to record can be the hostile act, an act of indifference and of turning away" (1995:418). In the unfolding "compassionate turn," there is a growing acknowledgment that – like all academic disciplines – cultural anthropology was, is, and will always be flawed or imperfect, and that vigorous internal debate and criticism are "normal" and beneficial. While recognizing that at any given time we are limited by the current state of knowledge and theory, we remain committed to fieldwork as our primary method, and still aspire to the production or creation of "good enough" or approximately right ethnography capable of presenting "adequate explanations" of human and social behavior which are "adequate to the times we live in" (Becker 1993).

An awareness of the moral and ethico-political dimensions of fieldwork has drawn anthropologists to the study of violence, genocide, suffering, trauma, resilience, healing, and reconstruction (e.g., Daniel 1996, Das 1995, Farmer 2003, Hinton 2005, Kleinman, Das, and Lock 1997, Mahmood 1996, Nordstrom 1997, Robben 2005, Sanford 2003, Scheper-Hughes 1993, Sluka 2000). The opening phrases of the influential collection *Social Suffering* (1997) address several major dimensions of this "compassionate turn":

> Social suffering . . . brings into a single space an assemblage of human problems that have their origins and consequences in the devastating injuries that social forces can inflict on human experience. Social suffering results from what political, economic, and institutional power does to people and, reciprocally, from how these forms of power themselves influence responses to social problems. Included under the category of social suffering are conditions that are usually divided among separate fields, conditions that simultaneously involve health, welfare, legal, moral, and religious issues. They destabilize established categories. For example, the trauma, pain, and disorders to which atrocity gives rise are health conditions; yet they are also political and cultural matters. (Kleinman, Das, and Lock 1997:ix)

The shift from a solipsistic postmodern anthropology to the study of the real suffering of real people carries multiple consequences for fieldwork. The multilayered and multidimensional complexity of research themes such as genocide, social suffering, cultural trauma, and social reconstruction requires a holistic approach that, after a long detour, reconnects fieldwork to the tradition of Boas and Malinowski. Yet, unlike these founding fathers, contemporary anthropologists have acquired enough academic confidence to draw freely on other disciplines. The "compassionate turn" makes anthropologists also reemphasize the importance of empathy, not empathy as a methodological technique to adopt the "native point of view" but as an epistemological approach. Compassionate empathy makes the fieldworker and the research participant share a subjective space, implicating them in each other's lives and in the production of ethnographic knowledge. This approach carries a political responsibility, which is not a return to the action anthropology of the 1970s but rather a form of social advocacy delivered mostly from the steps of the academy and only rarely engaged directly in the field. Robert Borofsky's agenda for an anthropological outreach to the world is the public face of this compassionate fieldwork:

> Public anthropology demonstrates the ability of anthropology and anthropologists to effectively address problems beyond the discipline – illuminating the larger social issues of our times as well as encouraging broad, public conversations about them with the explicit goal of fostering social change. It affirms our responsibility, as scholars and citizens, to meaningfully contribute to communities beyond the academy – both local and global – that make the study of anthropology possible. (www.publicanthropology.org)

Public anthropology sees ethnographers as witnesses, instead of dispassionate observers or political activists, who connect their readers to the world's trouble spots and sensitize them to the suffering and struggles of underprivileged human beings. It wants to generate public discussion, influence opinion, and engage politicians and policy makers critically to achieve genuine social change without, however, becoming involved in its realization.

## "Being there"

The postmodern critique of fieldwork and its compassionate reaction have stimulated new conceptions of what constitutes "the field" and ethnography. This process continues today. People like James Fernandez (1985:19) argued two decades ago that fieldwork is mainly what anthropology is all about, and that the essential characteristic of fieldwork is "being there." This is particularly reflected in two recent books on fieldwork – *Being There: The Necessity of Fieldwork* (Bradburd 1998) and *Being There: Fieldwork in Anthropology* (Watson 1999). But the very "what, where, and when" of fieldwork has shifted. For example, Deborah D'Amico-Samuels (1991:69) has questioned both the geographical and temporal construction of the traditional division between "home" being here and "the field" being there, between where we write and publish and where we do our fieldwork, and explored "where does the field begin and end, if ever?" Does fieldwork end when we "leave the field," and what does "leave the field" mean in a high-tech, postmodern, and globalized world where even research participants in the remotest areas are accessible by telephone? D'Amico-Samuels argues that the notion of being "back from the field" implies an artificial separation in the ethnographic process, with unfortunate consequences. She concludes that today "the field is everywhere," and there is no division between home and the field because both exist in the same holistic context of globalized power relations (D'Amico-Samuels 1991:83).

Traditionally, fieldwork ended when one returned from the field to analyze and write up the research results. However, in contemporary cultural anthropology, when researchers more frequently conduct *long-term* or diachronic fieldwork in the same location over many years and several field trips, and where the physical distance between "home" and "the field" is largely ameliorated by instantaneous means of electronic mass communication, this simple dichotomy between being "in the field" and then leaving no longer holds sway. Today, many anthropologists consider that, in a sense, they never really leave the field entirely and thus their fieldwork never really ends. For example, in 1994 one of Jeffrey Sluka's key research participants in Belfast, the leader of a nationalist paramilitary group, was killed. Within a few hours of his death he received a telephone call from other research participants, and they spoke for nearly two hours about what had happened. This discussion was predicated on fieldwork-based relations, was the same as a research interview, and the notes he wrote were indistinguishable from "field notes." But there was an immense physical distance between them: he was at "home" and they were "in the field." But while he was on the telephone, was he not essentially "in the field" and was this not a form of attenuated "fieldwork"? (See Sunderland 1999 for a discussion of "fieldwork and the phone.")

Today, cultural anthropologists are increasingly exploring the implications for long-term fieldwork marked by the presence of the ethnographer at discreet periods over their life history and intellectual thought. For example, Anthony Cohen, who conducted fieldwork over 19 years on the island of Whalsay, in Shetland, has observed that during this time there was "continuous change: in the field; in the discipline; to the author himself; and in his view of the discipline, the field, and his earlier analysis of his data" (1992:339). He referred to "that mental notebook which is never closed and which certainly does not recognize the geographical and cultural limits and specificities of our putative fields" (1992:339), called attention to "anthropologists' compulsion to carry 'their' field with them mentally, long after they have left it physically" (1992:344), and introduced the idea of "post-fieldwork fieldwork":

> We bring to the analysis of our fieldnotes [and headnotes] continuously accumulating experience extraneous to the circumstances in which they were written. "In this sense," says Ottenberg, "the field experience does not stop. Things that I once read in my fieldnotes in one way, I now read in another." Hastrup makes a similar point: "The past is not past in anthropology." It is precisely this process of rereading that I refer to by the phrase, "post-fieldwork fieldwork." (1992:345)

Today, a common pattern of fieldwork for cultural anthropologists is one year of PhD fieldwork followed by a series of shorter trips, frequently lasting for three months (over a summer break) or sometimes six months (a semester sabbatical), over the course of an academic career. Some do considerably more fieldwork, but some do considerably less. It is increasingly difficult for graduate students to get funding for a year's fieldwork, and for faculty to take that much time off as research leave.

That is, economics often dictates the kind of fieldwork anthropologists do, and the tendency has been toward the development of more cost-efficient (i.e., cheaper) alternatives to long-term fieldwork. One result is that today much fieldwork resembles more the Boasian/American tradition than the Malinowskian/British one – namely, shorter periods working with one or a few key "research participants," rather than extended participant observation of a whole community for one or two years.

In cultural anthropology, the process of critiquing, refining, and redefining ethnography and fieldwork continues. Today, the "best practice" of fieldwork is *ethically grounded*, with the free and informed consent of research participants. It is *participatory*, shaped with the active *collaboration* of research "participants" rather than "subjects," and conducted with their needs in mind. That is, *reciprocity* – giving something back to the community which they deem to be of use to them – is built into it by design. The new research orientations in fieldwork that emerged in the late 1980s and 1990s, such as *narrative ethnography* and *testimonio*, and the compassionate ones in the process of emerging today, are examples of the application of these themes.

## The Construction and Organization of the Reader

> That charming and intelligent Austrian-American anthropologist Paul Radin has said that no one quite knows how one goes about fieldwork. Perhaps we should leave the question with that sort of answer. But when I was a serious young student in London I thought I would try to get a few tips from experienced fieldworkers before setting out for Central Africa. I first sought advice from Westermarck. All I got from him was "don't converse with an informant for more than twenty minutes because if you aren't bored by that time he will be." Very good advice, even if somewhat inadequate. I sought instruction from Haddon, a man foremost in field-research. He told me that it was really all quite simple; one should always behave as a gentleman. Also very good advice. My teacher, Seligman, told me to take ten grains of quinine every night and to keep off women. The famous Egyptologist, Sir Flinders Petrie, just told me not to bother about drinking dirty water as one soon became immune to it. Finally, I asked Malinowski and was told not to be a bloody fool. So there is no clear answer, much will depend on the man, on the society he is to study, and the conditions in which he is to make it. (Evans-Pritchard 1973:1)

So why this reader about fieldwork rather than methods? Should we not just improve our data-collection techniques and leave all talk of fieldwork to after-dinner conversations or one-liners to reassure anxious students? In fact, our mentor Gerald Berreman told us when we were close to embarking on our doctoral fieldwork that what we had to do was "hang out" in cafés and on street corners. Yet, there is more here than meets the eye. Beneath the seemingly common-sense remarks by Berreman and Evans-Pritchard's teachers, there are hints at the complexities of fieldwork relations and rapport (don't talk too long with an informant; keep off women; hang out), ethics (behave like a gentleman), conflicts, hazards, and dangers in fieldwork (take quinine; don't worry about dirty water), and fieldwork identity (don't be a fool).

We therefore believe that the technicalities of field interviews or organizing field notes are not the problem, to mention two common concerns among students, but what goes on in the relation between ethnographer and research participants, and how to interpret field notes. Insight in the interview experience and the epistemology of fieldnotes, rather than the technique, leads to better interviewers and better data. There are no recipes for gathering ethnographic data, just as there are no formulas for designing a groundbreaking experiment in nuclear physics. Instead, a better understanding of the experience of conducting fieldwork will heighten the anthropologist's sensitivity to the research process and enhance access to rewarding data rather than relying solely on trodden paths led by a singular concern with methods. This is not to say that anthropologists should not train themselves thoroughly in fieldwork methods – we have added an appendix with extensive bibliographic references to ethnographic data-gathering techniques for the interested reader (see Appendix 1) – but we are convinced that a sophisticated understanding of the fieldwork experience will make better fieldworkers and yield more profound and groundbreaking ethnographies.

Given the great many books and articles available on ethnographic fieldwork, several choices had to be made for this reader. One, the selection concentrates on those texts which we believe to have continued relevance in the discipline and should be part of the background knowledge of every undergraduate and graduate student in cultural anthropology. Two, the selection aims to provide readers with a good sense of the breadth, variation, and complexity of the fieldwork experience. We have therefore not restricted this anthology to discussions of data gathering proper, but extended into issues of professional identity, fieldwork relations, ethics, and the practice of ethnographic writing. Three, since much has been written about a great number of topics, we have been forced to choose from several equally superb articles on the same subject. Style, accessibility, imagination, innovation, and personal preference were considered in selecting one text over another.

The reader is divided into ten parts, each representing a major subfield or theme in **ethnographic fieldwork** in cultural anthropology:

I **Beginnings:** Ethnographic fieldwork had been carried out in various guises and forms at the turn of the nineteenth century, but Bronislaw Malinowski is generally considered the "founding father" of extended, first-hand fieldwork in anthropology.

II **Fieldwork Identity:** Self-reflection on research experiences has been more prominent in anthropology than in any other scholarly discipline. The long-term immersion in other societies and cultures, the status as foreigners and "others," the interactive research methods, and the nature of fieldwork as a professional rite of passage with often far-reaching effects on the self, have made fieldwork identity of great concern, and some authors argue that the anthropologist's identity has a decisive influence on the data gathered.

III **Fieldwork Relations and Rapport:** The success of ethnographic fieldwork is in large measure determined by the ability to establish good rapport and develop meaningful relations with research participants. These relations range from friendship to hostility, and may be influenced by ethnicity, religion, class, gender, and age.

IV **The "Other" Talks Back:** Access to schooling and the dissemination of anthropological writings have turned once illiterate "informants" into avid critics of their ethnographers. Such "talking back" has made anthropologists aware of their conduct as researchers, led to soul-searching about their writings, and enhanced the quality of ethnographic research.

V **Fieldwork Conflicts, Hazards, and Dangers:** Fieldwork is not free of tensions, dilemmas, conflicts, hazards, and dangers, and even lethal hostility. Awareness of such risks is of life-saving importance to students embarking on their first fieldwork experience, and research troubles, even failures, are a source of ethnographic knowledge in themselves.

VI **Fieldwork Ethics:** The relational nature of fieldwork addressed in Part III, the conflicts described in Part IV, and the changing relation between anthropologists and their research participants referred to in Part V, have made contemporary fieldwork morally and ethically more complex. A grasp of the ethical implications of fieldwork is indispensable before students can develop their first research design.

VII **Multi-Sited Fieldwork:** Long-term, face-to-face, empirical research in a small community has been the enduring hallmark of anthropological fieldwork. However, in the last decade, there has been an increase in multi-sited fieldwork because of a growing interdisciplinary interest among anthropologists and forces of globalization which impinge on even the remotest community. Multi-sited fieldwork is not only concerned with different locales and migrants but also involves widely varying topics such as bodies, imaginary beings, the internet, and the scientific enterprise itself.

VIII **Sensorial Fieldwork:** Fieldwork is mostly based on verbal exchanges and generally yields a written narrative of research results. However, attention to sight, sound, smell, and taste have also produced fascinating interpretations of culture.

IX **Reflexive Ethnography:** The ethnographic turn of the 1970s heightened anthropology's awareness of the epistemology of the fieldwork encounter. The ethnographer's crucial role in dialogic data collection gave her or him a central place in anthropological writings. Reflexivity has enriched fieldwork by making ethnographers pay much closer attention to the interactional processes through which knowledge is acquired, learned, and transmitted.

X Fictive Fieldwork and Fieldwork Novels: Some anthropologists have been exploring literary styles to transmit their fieldwork experiences, thus blurring the boundaries between experience, impression, and emotion. Others have turned to poetry, experimental writing, and fiction to make readers share in the ethnographic encounter which makes fieldwork such a singular experience.

With this introduction and structure in mind, welcome to the reader, and best wishes for success in fieldwork.

# *Part I*
# Beginnings

*Antonius C. G. M. Robben*

Since antiquity the Western world has been curious about other cultures. The exploits of the Greeks and Romans in Europe, Africa, and Asia yielded descriptions of subjected populations and sparked philosophical reflections about the nature of society and humankind. This interest declined after the fall of the Roman Empire. Europe entered the Dark Ages and was beset by political fragmentation, economic decline, and foreign invasions. The Renaissance brought a new élan with the emergence of strong states, flourishing arts, technological innovations, commercial expansion, and a desire for learning and knowledge. Portuguese explorers and traders, Spanish conquerors and missionaries, Dutch merchants and sailors, European slaves of the Ottoman Empire, English captives of Native Americans, and so forth described the strange customs and mores of the people they met. Their observations emphasized the exotic and were often random.

The study of non-European cultures received tremendous impetus from the rise of a humanist philosophy which exalted the dignity of Man, emphasized that people are natural beings, and propagated the study of nature by scholars. From the sixteenth century onward the more serious European students of foreign cultures had begun to call for the systematic collection of knowledge; a plea which had already been made in the fourteenth century by the Arab historian Ibn Khaldûn (2005). Around the mid-eighteenth century, scientific expeditions which mapped the world's natural riches and legitimated imperialism were organized (see Honigmann 1976, Malefijt 1976, Pratt 1992). Of the numerous instructions that were produced over the centuries, one particularly influential and even visionary example has been selected here. In 1800, the young French philosopher Joseph-Marie Degérando wrote a manual for a scientific expedition to Australia and South Asia led by Captain Baudin, entitled *Considerations on the Various Methods to Follow in the Observation of Savage Peoples*. These instructions served as a basis for the *General Instruction to Travelers*, published in 1840 by the Ethnological Society of Paris. This French guide, in turn, strongly influenced the 1841 British publication *Queries respecting the Human Race to be addressed to travellers and others*. The *Queries* developed in 1874 into the highly influential *Notes and Queries on Anthropology* which guided numerous generations of anthropologists through fieldwork (RAI 1951, Urry 1972).

Joseph-Marie Degérando, in the excerpt from *The Observation of Savage Peoples* (1969), gives a critique of the travel accounts by explorers and proposes a systematic study of simple societies for the edification of humankind. Explorers must become philosophical travelers who use scientific methods to know other people. Degérando wants the Science of Man to follow in the footsteps of natural science by emphasizing observation, analysis, and comparison. Simple societies are ideal study objects, so he argued, because crucial properties can be more easily isolated than in complex societies. Simple societies represent the original human society, and thus reflect our own distant past. Comparison will then yield a detailed cultural evolution through which all societies need to pass from savagery to civilization. Seventy-seven years later, the American anthropologist Lewis Henry Morgan (1985) believed he had discovered the principal stages of this development.

The botanists, zoologists, geographers, and other natural scientists on Captain Baudin's mission may not have used Degérando's manual, but his instructions were to become the principal standards of ethnographic fieldwork one century later. Degérando found eight shortcomings in the descriptions and travelogues of the restless explorers who were keener on discovering than carefully mapping new territories. His eight recommendations are remarkably current and, with some stretch of imagination, will therefore be presented here in today's anthropological terms. One, ethnographers must study all facets of a society in a contextualized way, instead of just focusing on a few individuals or ceremonies. Two, ethnographers must make first-hand observations, obtain representative samples, and not rely on secondary accounts. Three, observations must be made in a systematic, unbiased, and holistic way, and confirmed by informants. Four, conclusions must be based on thorough data collection and drawn inductively, not through deduction or analogy. Five, ethnographic accounts must approximate local meanings and understandings as closely as possible instead of being presented in Western cultural terms. Six, ethnographers should not rely on first impressions but should immerse themselves in the host society. Seven, a thorough knowledge of the native language is indispensable for trustworthy observations and conclusions. Finally, ethnographers should pay close attention to the historical origins and development of simple societies by studying their oral tradition. In sum, Joseph-Marie Degérando recommends that ethnographers should conduct sustained research in the native language, establish rapport, use an emic approach, and see society from the native's point of view.

Unfortunately, Degérando's sound recommendations were neither heeded by Baudin's expedition nor by future travelers, who relied more on brief observations, as botanists would, than on learning the local language, asking local people about their culture, and becoming one of them. At best, they kept item lists which reaped what they sowed, providing nineteenth-century anthropologists with decontextualized answers that allowed comparative analyses which would feed into preconceived evolutionary notions. They lacked the lived experience, cultural knowledge, and local understandings which fascinated later generations of anthropologists, and did not conduct the long-term, systematic ethnographic studies propagated by Joseph-Marie Degérando and implemented to varying degrees by Franz Boas and Bronislaw Malinowski, the two most famous and influential forefathers of modern fieldwork.

The German-born Franz Boas was neither the first American scholar to systematically study cultures, because Henry Rowe Schoolcraft, Ely Parker, Patrick T. L.

Putnam, and Lewis Henry Morgan had preceded him, nor was he America's first ethnographer, because Frank Hamilton Cushing had already conducted more than two years of fieldwork among the Zuñi and Alice Fletcher was studying the Nez Percé (Marks 1989), but Boas was certainly the first American anthropologist to professionalize the awakening discipline, introduce an inductive analysis of culture, and instill ethnographic fieldwork as its principal research method. Boas held the first Chair in Anthropology, founded the first Department of Anthropology at Clark University in Worcester, Massachusetts, awarded the first PhD in anthropology at Clark, and trained the first generation of American anthropologists at Columbia University, including Alfred Kroeber, Margaret Mead, Robert Lowie, Ruth Bunzel, Edward Sapir, Paul Radin, and Ruth Benedict. As an ethnographer Boas is best remembered for his fieldwork among Native Americans in British Columbia, on the northwest coast of America (see Stocking 1974, 2001).

The selected article "The Methods of Ethnology" (Boas 1920) is a frontal attack on the evolutionary and diffusionist approaches that dominated European anthropology, and also influenced American scholars. Boas emphasizes the study of cultural change through meticulous ethnographic fieldwork with great attention to detail, before trying to answer larger questions about the long-term development of cultures and societies. In this sense, he agrees with Degérando's instructions, but not with his assumptions about social evolution. Boas also raises doubts about the older evolutionary notion that culture is determined by psychological needs and the, at the time novel, psychoanalytic explanation of culture as repressing destructive unconscious desires by channeling them into social institutions. The title of the article included here is somewhat deceptive, because Boas does not describe fieldwork methods per se but rather the object and nature of anthropological data collection. He makes three crucial points: (1) social and cultural phenomena are both cause and effect in a dynamic process of continuous change, (2) individuals shape and are shaped by their social environment, and (3) larger anthropological conclusions must be reached through induction.

The lasting contribution of Franz Boas to ethnographic fieldwork is his emphasis on the careful study of societies undergoing rapid change without reducing their cultures to evolutionary laws, psychological processes, or collections of traits acquired through migration and diffusion. His critique is epistemological rather than methodological because he is more concerned with the type of knowledge acceptable to the scientific study of culture than with how to acquire that knowledge. Boas leaves ethnographers in the dark about the practice and process of fieldwork. It was the Polish expatriate Bronislaw Malinowski who advanced the methodology of ethnographic investigation through his research among the Trobrianders of Melanesia. He reconciled Boas's desire for rigorous, detailed, and contextualized data collection with the earlier experience-rich accounts of explorers, missionaries, captives, and other amateur scribes of foreign culture, adding his own attention to local worldviews. Malinowski became thus the great synthesizer of ethnographic fieldwork who deeply influenced twentieth-century anthropological research methods.

The excerpt "Method and Scope of Anthropological Fieldwork," from Malinowski's seminal 1922 ethnography *Argonauts of the Western Pacific: An Account of Native Enterprise and Adventure in the Archipelagoes of Melanesian New Guinea* (Malinowski 1984), is anthropology's most classic and enduring statement about ethnographic fieldwork. Malinowski begins this exposé by stating that even many years of residence among a population leave the untrained eye blind to local culture. Fieldworkers can accomplish much more in just a year, when they live among a population as one of them and pursue clear scientific objectives in a systematic, methodical, and active way. Malinowski hammers on the importance of separating the wheat from the chaff by delineating the laws, regularities, social constitution, and "firm skeleton" of cultural phenomena in a holistic manner. This approach covers all cultural domains, the ordinary as well as the quaint, and technology as much as religion and kinship. The anatomy of rules and regularities is not apparent in nonliterate tribal societies and therefore has to be formulated inductively from observed behavior and actions. Still, the skeleton is bare without the flesh and blood of everyday reality, with its intimate social life and excitement of feasts, rituals, and ceremonies. An ethnographer must not be satisfied with describing the structural features of culture but must also have an eye for the "imponderabilia of actual life" discovered through close presence and participation. Malinowski advocates participant observation as a crucial method to obtain such inside perspective. Flesh, blood, and bones are inanimate without a spirit, so ethnographers must also study people's ideas, opinions, and worldviews. How people feel and think in their own language as members of their community is crucial to complete the picture of the culture under study. According to Malinowski, anthropology's overarching aim is "to grasp the native's point of view," to see their world and their relation to life through the eyes of the local population. Thus, Malinowski sets a high standard for ethnographic fieldwork that relies on in situ research in the native language and from the native's perspective, while gathering data on the structure, practice, and worldview of society in a holistic fashion (see also Firth 1957; Young 1979, 2004).

With Malinowski, fieldwork had come of age and his students, including Raymond Firth, Edward Evans-Pritchard, Meyer Fortes, Audrey Richards, Lucy Mair, Gregory Bateson, Edmund Leach, Max Gluckman, Hortense Powdermaker, and many others fanned out across the world to write their own ethnographies based on long-term research. Malinowski's fieldwork method remained hegemonic in anthropology, a true rite of passage for aspiring anthropologists, until the 1990s, when encompassing processes of globalization raised the call for multi-sited fieldwork (see Part VII).

# 1

# The Observation of Savage Peoples

## *Joseph-Marie Degérando*

### I Advertisement

These considerations are addressed to Captain BAUDIN, correspondent of the society, about to leave for his expedition of discovery, and to the various observers accompanying him; they are addressed also to Citizen LEVAILLANT, who is going to attempt a third expedition in the interior of Africa. Since it is possible that both have occasion to encounter peoples at very different degrees of civilization or barbarity, it seems the right course to provide for any hypothesis, and to make these CONSIDERATIONS so general that they can be applied to any society differing in its moral and political forms from those of Europe. The leading purpose has been to provide a complete framework comprising any point of view from which these societies can be envisaged by the philosopher. It has not been supposed that certain simple questions that can easily be foreseen should be omitted, when they were necessary to the completeness of the whole.

It seems astonishing that, in an age of egoism, it is so difficult to persuade man that of all studies, the most important is that of himself. This is because egoism, like all passions, is blind. The attention of the egoist is directed to the immediate needs of which his senses give notice, and cannot be raised to those reflective needs that reason discloses to us; his aim is satisfaction, not perfection. He considers only his individual self; his species is nothing to him. Perhaps he fears that in penetrating the mysteries of his being he will ensure his own abasement, blush at his discoveries, and meet his conscience.

True philosophy, always at one with moral science, tells a different tale. The source of useful illumination, we are told, like that of lasting content, is in ourselves. Our insight depends above all on the state of our faculties; but how can we bring our faculties to perfection if we do not know their nature and their laws? The elements of happiness are the moral sentiments; but how can we develop these sentiments without considering the principle of our affections, and the means of directing them? We become better by studying ourselves; the man who thoroughly knows himself is the wise man. Such reflection on the nature of his being brings a man to a better awareness of all the bonds that unite us to our fellows, to the re-discovery at the inner root of his existence of that identity of common life actuating us all, to feeling the full force of that fine maxim of the ancients: 'I am a man, and nothing human is alien to me.'

Importance of the study of Man in general.

But what are the means of the proper study of man? Here the history of philosophy, and

Joseph-Marie Degérando, pp. 60–70, 101–4 from *The Observation of Savage Peoples*, trans. F. C. T. Moore (London: Routledge & Kegan Paul, 1969). © F. C. T. Moore 1969. Originally published in French as *Considérations sur les methodes à suivre dans l'observation des Peuples Sauvages* (1800).

the common voice of learned men give reply. The time for systems is past. Weary of its centuries of vain agitation in vain theories, the pursuit of learning has settled at last on the way of observation. It has recognized nature as its true master. All its art is applied in listening carefully to that voice, and sometimes in asking it questions. The Science of Man too is a natural science, a science of observation, the most noble of all. What science does not aspire to be a natural science? Even art, which men sometimes contrast with nature, aims only to imitate her.

The method of observation has a sure procedure; it gathers facts to compare them, and compares them to know them better. The natural sciences are in a way no more than a series of comparisons. As each particular phenomenon is ordinarily the result of the combined action of several causes, it would be only a deep mystery for us if we considered it on its own: but if it is compared with analogous phenomena, they throw light each on the other. The particular action of each cause we see as distinct and independent, and general laws are the result. Good observation requires analysis; now, one carries out analysis in philosophy by comparisons, as in chemistry by the play of chemical affinities.

Man, as he appears to us in the individuals around us, is modified at the same time by a multitude of varying circumstances, by education, climate, political institutions, customs, established opinions, by the effects of imitation, by the influence of the factitious needs that he has created. Among so many diverse causes that unite to produce that great and interesting effect, we can never disentangle the precise action that belongs to each, without finding terms of comparison to isolate man from the particular circumstances in which he is presented to us, and to lift from him those adventitious forms under which, as it were, art has hidden from our eyes the work of nature.

Of the observation of savages in particular.

Now, of all the terms of comparison that we can choose, there is none more fascinating, more fruitful in useful trains of thought than that offered by savage peoples. Here we can remove first the variations pertaining to the climate, the organism, the habits of physical life, and we shall notice that among nations much less developed by the effect of moral institutions, these natural variations are bound to emerge much more prominently: being less distinguished by secondary circumstances, they must chiefly be so by the first and fundamental circumstances belonging to the very principle of existence. Here we shall be able to find the material needed to construct an exact scale of the various degrees of civilization, and to assign to each its characteristic properties; we shall come to know what needs, what ideas, what habits are produced in each era of human society. Here, since the development of passions and of intellectual faculties is much more limited, it will be much easier for us to penetrate their nature, and determine their fundamental laws. Here, since different generations have exercised only the slightest influence on each other, we shall in a way be taken back to the first periods of our own history; we shall be able to set up secure experiments on the origin and generation of ideas, on the formation and development of language, and on the relations between these two processes. The philosophical traveller, sailing to the ends of the earth, is in fact travelling in time; he is exploring the past; every step he makes is the passage of an age. Those unknown islands that he reaches are for him the cradle of human society. Those peoples whom our ignorant vanity scorns are displayed to him as ancient and majestic monuments of the origin of ages: monuments infinitely more worthy of our admiration and respect than those famous pyramids vaunted by the banks of the Nile. They witness only the frivolous ambition and the passing power of some individuals whose names have scarcely come down to us; but the others recreate for us the state of our own ancestors, and the earliest history of the world.

And even should we not see in savage peoples a useful object of instruction for ourselves, would there not be enough high feelings of philanthropy to make us give a high importance to the contact that we can make with them? What more moving plan than that of re-establishing in such a way the august ties of universal society, of finding once more those former kinsmen separated by long exile from the rest of the common family, of offering a hand to them to raise them to a happier state! You who, led by a generous devotion on those far shores, will soon come near their lonely huts, go before them as the representatives of all humanity! Give them in that name the vow of brotherly alliance! Wipe from their minds the memory of cruel adventurers who sought to stay with them only to rob or bring them into slavery; go to them only to offer benefits. Bring them our arts, and not our corruption, the standard of our morality, and not the example of our vices, our sciences, and not our scepticism, the advantages of civilization, and not its abuses; conceal from them that in these countries too, though more enlightened, men destroy each other in combat, and degrade each other by their passions. Sitting near them, amid their lonely forests and on their unknown shores, speak to them only of peace, of unity, of useful work; tell them that, in those empires unknown to them, that you have left to visit them, there are men who pray for their happiness, who greet them as brothers, and who join with all their hearts in the generous intentions which lead you among them.

…ts in …rva- …e up …e …nt.

In expressing here everything that we expect of your careful and laborious work, we are far from wishing to underestimate the many services done to society by the explorers who have gone before you. Had they merely prepared the way, by their brave undertakings, for those who were to follow them, and provided valuable guidance, by that alone they would have earned a great title to our gratitude. But they began to establish some communication with savage societies; they have reported to us various information on the customs and language of these peoples. It is merely that, divided by other concerns, and with a greater impetus to discover new countries than to study them, constantly moving when they should have stayed at rest, biased perhaps by those unjust prejudices that cast a slur in our eyes on savage societies, or at least, witness of our European indifference for them, they did not sufficiently devote themselves to bringing back exact and complete observations; they have met the invariable end of those who observe in a precipitate and superficial manner – their observations have been poor, and the imperfection of their reports has been the penalty of our carelessness.[1] Since man's curiosity is aroused more by the novelties that strike his senses than by any instruction that his reason may gather, it was thought far more worth while to bring back from these countries plants, animals and mineral substances, than observations on the phenomena of thought. So naturalists daily enriched their specimen cases with many genera, while philosophers spent time in vain disputes in their schools about the nature of man, instead of uniting to study him in the arena of the universe.

Let us review the main faults of the observations on savage man made by these explorers, and the gaps that they have left in their accounts. When we realize what they have not done, we shall see better what remains to be done.

First fault.

The first fault that we notice in the observations of explorers on savages is their incompleteness; it was only to be expected, given the shortness of their stay, the division of their attention, and the absence of any regular tabulation of their findings. Sometimes, confining themselves to the study of some isolated individuals, they have given us no information on their social condition, and have thus deprived us of the means of estimating the influence which these social relations might have on individual faculties. Sometimes, pausing on the smallest details of the physical life of the savages, they have given us scarcely any details of their moral customs. Sometimes, describing the customs of grown men, they have failed to find out about the kind of education received in childhood and youth: and above all, preoccupied almost entirely with the external and overt characteristics of a people, of its ceremonies and of its dress, they have generally taken too little care to be initiated in the far more important circumstances of its theoretical life, of its needs, its ideas, its passions, its knowledge, its laws. They have described forms rather than given instructive reports; they have marked certain effects, and explained scarcely any causes.

Second fault.

Further, such insufficient observations have not always been very certain or authentic, whether because they have sometimes been too particular, and explorers have wished to judge a society by a few of its members, a character by a few actions, or because they have sometimes confined themselves to hearsay, to the stories of the Savages whom they met, and who perhaps were not properly understood, perhaps not well-versed in what was asked, and perhaps had no interest in telling the truth, or at least in making it known in its entirety.

Third fault. We should add that these observations have been badly ordered, and even in many cases quite without order. The explorers had not enough understood that there is a natural connection between the various facts that one gathers about the condition and character of societies, that this order is necessary to the precision of the individual facts, and that often some of them should serve as preparation for the others. We should study effects before trying to go back to first principles; observe individuals before trying to judge the society; become acquainted with domestic relations inside families before examining the political relations of society; and above all we should aim at full mutual understanding when we speak to men before basing certain conclusions on the accounts that we claim to have received.

Fourth fault. Often explorers have based the accounts that they bring us on incorrect or at least on dubious hypotheses. For example, they habitually judge the customs of Savages by analogies drawn from our own customs, when in fact they are so little related to each other. Thus, given certain actions, they suppose certain opinions or needs because among us such actions ordinarily result from these needs or opinions. They make the Savage reason as we do, when the Savage does not himself explain to them his reasoning. So they often pronounce excessively severe judgments on a society accused of cruelty, theft, licentiousness, and atheism. It were wiser to gather a large number of facts, before trying to explain them, and to allow hypothesis only after exhausting the light of experience.

Fifth fault. In the case of the accounts of explorers there is another cause of uncertainty, a fault of language rather than of imperfect observation, namely that the terms used to pass on to us the results of their observations are often in our own language of vague and ill-determined meaning. Consequently, we are in danger of taking their accounts in a way which they did not intend. This happens particularly when they try to record the religious, moral, and political beliefs of a people. It happens too when instead of giving a detailed and circumstantial account of what they have actually seen, they limit themselves to summary descriptions of the impressions which they received, and of the general judgments which they inferred on the character of peoples. Yet this drawback could easily have been avoided by making it a policy either to describe things without judging them, or to choose expressions whose sense is more agreed, or to give a precise stipulation of the sense in which one intends their use.

Sixth fault. This is not the place to enumerate the inaccuracies springing from a lack of impartiality in explorers, from prejudices imposed by their particular opinions, from the interests of vanity or the impulse of resentment. The character of the worthy men today devoting themselves to this noble undertaking is a sufficient guarantee that such a stamp will never shape their accounts. But explorers with the purest and most honest intentions have often been led into error about the character of peoples by the behaviour they meet with. They have inferred too lightly from the circumstances of their reception, conclusions about the absolute and ordinary character of the men among whom they have penetrated. They have failed to consider sufficiently that their presence was bound to be a natural source of fear, defiance, and reserve; that reasons of policy might exaggerate this unusual circumspection; that the memory of former attacks might have left dark prejudices in the mind of such peoples; that a community might be gentle and sociable, and yet believe itself in a state of natural war with strangers whose intentions are unknown; and finally that for a just estimate of the character of a tribe, one should first leave time for the reactions of astonishment, terror, and anxiety bound to arise in the beginning to be dispelled, and secondly one should be able to be initiated into the ordinary relations which the members of the community have with each other.

Seventh fault. But of all the regrets left by the accounts of the explorers who went before you, the strongest is their failure to tell us of the language of the peoples visited. In the first place, the scanty information which they do give lacks precision and exactness, whether because they fail to record how they went about questioning the Savages, or because they themselves have often taken little care to pose the questions properly. The demonstrative and natural gestures which they have used to ask the Savages the names of objects were often themselves liable to considerable uncertainty; one cannot know if those who were questioned understood the gestures in the same way as the explorers who were using them, and so whether they were giving proper replies to their questions. Further, to provide us with some useful and positive ideas of the idioms of savage peoples, it was wrong for explorers to limit themselves as they did to taking at random names of various objects with scarcely any relation between them; at least a family of analogous ideas should have been followed up, when it was impossible to make a record of the whole language, so that some judgment could be made on the generation of terms, and on the relations between them; it was not enough to be content with some detached words; but it would have been sensible to record whole sentences to give some idea of the construction of discourse. Further, one should have discovered whether these words were simple, or composite, as their length would often lead us to suppose; whether they were qualified by any articles or particles; and finally whether they were inflected or remained in the absolute, and whether they were liable to any kind of grammatical laws.

Eighth fault.

Failing to acquaint themselves thoroughly with the idiom of savage peoples, explorers have been powerless to draw on perhaps the most interesting ideas that could have been available. They have been unable to pass on the traditions that such peoples may preserve of their origin, of the changes that they have undergone, and of the various details of their history; traditions which perhaps would have thrown great light on the important question of how the world was peopled, and on the various causes of the present state of these societies. They have been unable to explain the significance of a mass of ceremonies and customs which are probably no more than allegorical; they have given us bizarre descriptions which tickle the idle curiosity of the many, but which offer no useful instruction to the philosophically minded. Lacking the means to carry on connected conversation with such peoples, they have been able to form only very hazardous and vague ideas of their opinions and notions; finally, they have been unable to provide us with these data, as revealing as they are abundant, that the language of a society presents on its way of seeing and feeling, and on the most intimate and essential features of its character.

Observations to make. I Signs of the Savages.

The main object, therefore, that should today occupy the attention and zeal of a truly philosophical traveller would be the careful gathering of all means that might assist him to penetrate the thought of the peoples among whom he would be situated, and to account for the order of their actions and relationships. This is not only because such study is in itself the most important of all, it is also because it must stand as a necessary preliminary and introduction to all the others. It is a delusion to suppose that one can properly observe a people whom one cannot understand and with whom one cannot converse. The first means to the proper knowledge of the Savages, is to become after a fashion like one of them; and it is by learning their language that we shall become their fellow citizens.

But if there is a marked lack of good methods even for learning well the languages of neighbouring civilized nations; if this study often requires much time and effort, what position shall we be in for learning the idioms of savage tribes, when there is no dictionary, no spokesman to translate to us, and no shared habits and common associations of ideas as in the case of the former languages, through which explanations can be made? Let us not hesitate to say that the art of properly studying these languages, if it could be reduced to rules, would be one of the master-works of philosophy; it can be the result only of long meditation on the origin of ideas. We shall confine ourselves here to making some general remarks; the reflective thought of the enlightened men to whom we address them will assure their development, and direct their application.

The most important thing to observe in the study of the signs of Savages, is the order of the enquiry.

[. . .]

Conclusion.

We are aware that the totality of problems here posed for the explorer's wisdom calls for a huge amount of work, whether because of the number and the very importance of the questions, or the detailed and painstaking observations that each one demands. We are aware that this work is surrounded by all kinds of difficulties, and that one must expect to meet great obstacles in the first relations that one wishes to establish with the Savages. For these peoples cannot penetrate the real intentions of those who approach them, they cannot easily distinguish their friends from their enemies, and those who bring help, from those who come to invade their territory. But we may rightly expect anything of the patience, the perseverance, and the heroic courage of the travellers to whom today we bid farewell; we are assured of it by their personal character, by the intentions animating them, by the dazzling proofs which they have already vouchsafed. Oh! What have they not already done for science, what noble course have they not already run! It was worthy of them still to defer its term, and to go on to complete so fine a work! Estimable men, as we salute you here on the eve of a departure soon to come, as we see you tear yourselves from your land, your family, and your friends, and leap beyond the limits of the civilized world; as we dwell on the thought of the fatigues, the privations and the dangers which await you, and of that long exile to which you have voluntarily condemned yourselves, our souls cannot resist a deep emotion, and the movement of sensibility in us is joined to the respect which we owe to so noble an undertaking. But our thought is settled in advance on the term of that undertaking; and dwelling on this prospect, all our feelings are mixed in that of admiration and enthusiasm. Illustrious messengers of philosophy, peaceful heroes, the conquests which you are going to add to the domain of science have more brilliance and value in our eyes than victories bought by the blood of men! All generous hearts, all friends of humanity join in your sublime mission; there is in this place more than one heart which envies you, which groans in secret that inflexible duties keep him on these shores, who would put his glory in following your path and your example. Our prayers at least will follow you across the Ocean, or in the lap of the desert; our thoughts will often be with you, when below the equator, or near the pole, you gather in silence precious treasures for enlightenment. We shall say to each other: 'On this day, at this hour, they are landing perhaps on an unknown land, they are perhaps penetrating to the heart of a new people, perhaps they are resting in the shade of antique forests from their long sufferings; perhaps they are beginning to enter into relations with a barbarous people, to eliminate its unsociable suspicions, to inspire in it a curiosity to know our ways and a desire to imitate them, and perhaps they are laying the foundations of a new Europe.' Oh! who will tell in fact all the possible or probable results which may spring one day from these fine undertakings? I speak here not only of our fuller specimen cases, our more accurate and extensive maps, of our increased knowledge of the physical and moral history of the world, of the name of France taken to unknown shores! Think of the other bewitching prospects still offered to the reeling imagination! Trade extended by new relations; the navy brought to perfection by greater experience; journeys made easier by discoveries; our political grandeur increased by new colonies or new alliances! Who can tell? Perhaps whole nations civilized, receiving from civilization the power to multiply themselves, and associate themselves with us by the ties of a vast confederation; perhaps broader and more useful careers open to human ambition, talent and industry; these peoples of Europe, daily contesting at the cost of their blood some narrow strip of land, expanding at pleasure in more beautiful terrain; perhaps a new world forming itself at the extremities of the earth; the whole globe covered with happier and wiser inhabitants, more equally provided for, more closely joined, society raising itself to more rapid progress by greater competition and reaching perhaps by these unexpected changes that perfection on which our prayers call, but to which our enlightenment, our methods, and our books, contribute so little! . . . Vain chimerae perhaps; but chimerae to which our long unhappiness, our sad dissensions, and the sight of our corruption, yet give so much charm! . . . At least it is certain that these brave enterprises, directed to the most obscure parts of the Universe, lay up for posterity a new future, and that it is only for the wisdom of our descendants to gather the abundant fruits of this course that you are going to open. See how much the discoveries of Columbus changed the face of society, and what amazing destinies bore that fragile vessel to which he trusted himself! It is true, this grand revolution has not all been to our advantage, still less to that of the peoples to whom it has given us access. But Columbus put in the New World only greedy conquerors; and you are proceeding towards the peoples of the South only as pacifiers and friends. The cruel adventurers of Spain brought only destruction before them, and you will spread only good deeds. They served but the passions of a few men, and you aspire only to the good of all, to the glory of being of use! This glory, the sweetest, the truest, or rather the only true glory, awaits you, encompasses you already; you will know all its brilliance on that day of triumph and joy on which, returning to your country, welcomed amid our delight, you will arrive in our walls, loaded with the most precious spoils, and bearers of happy tidings of our brothers scattered in the uttermost confines of the Universe.

# 2

# The Methods of Ethnology

*Franz Boas*

During the last ten years the methods of inquiry into the historical development of civilization have undergone remarkable changes. During the second half of the last century evolutionary thought held almost complete sway and investigators like Spencer, Morgan, Tylor, Lubbock, to mention only a few, were under the spell of the idea of a general, uniform evolution of culture in which all parts of mankind participated. The newer development goes back in part to the influence of Ratzel whose geographical training impressed him with the importance of diffusion and migration. The problem of diffusion was taken up in detail particularly in America, but was applied in a much wider sense by Foy and Graebner, and finally seized upon in a still wider application by Elliot Smith and Rivers, so that at the present time, at least among certain groups of investigators in England and also in Germany, ethnological research is based on the concept of migration and dissemination rather than upon that of evolution.

A critical study of these two directions of inquiry shows that each is founded on the application of one fundamental hypothesis. The evolutionary point of view presupposes that the course of historical changes in the cultural life of mankind follows definite laws which are applicable everywhere, and which bring it about that cultural development is, in its main lines, the same among all races and all peoples. This idea is clearly expressed by Tylor in the introductory pages of his classic work "Primitive Culture." As soon as we admit that the hypothesis of a uniform evolution has to be proved before it can be accepted, the whole structure loses its foundation. It is true that there are indications of parallelism of development in different parts of the world, and that similar customs are found in the most diverse and widely separated parts of the globe. The occurrence of these similarities which are distributed so irregularly that they cannot readily be explained on the basis of diffusion, is one of the foundations of the evolutionary hypothesis, as it was the foundation of Bastian's psychologizing treatment of cultural phenomena. On the other hand, it may be recognized that the hypothesis implies the thought that our modern Western European civilization represents the highest cultural development towards which all other more primitive cultural types tend, and that, therefore, retrospectively, we construct an orthogenetic development towards our own modern civilization. It is clear that if we admit that there may be different ultimate and coexisting types of civilization, the hypothesis of one single general line of development cannot be maintained.

Opposed to these assumptions is the modern tendency to deny the existence of a general evolutionary scheme which would represent

Franz Boas, "The Methods of Ethnology," pp. 311–21 from *American Anthropologist* 22:4 (Oct.–Dec., 1920).

the history of the cultural development the world over. The hypothesis that there are inner causes which bring about similarities of development in remote parts of the globe is rejected and in its place it is assumed that identity of development in two different parts of the globe must always be due to migration and diffusion. On this basis historical contact is demanded for enormously large areas. The theory demands a high degree of stability of cultural traits such as is apparently observed in many primitive tribes, and it is furthermore based on the supposed correlation between a number of diverse and mutually independent cultural traits which reappear in the same combinations in distant parts of the world. In this sense, modern investigation takes up anew Gerland's theory of the persistence of a number of cultural traits which were developed in one center and carried by man in his migrations from continent to continent.

It seems to me that if the hypothetical foundations of these two extreme forms of ethnological research are broadly stated as I have tried to do here, it is at once clear that the correctness of the assumptions has not been demonstrated, but that arbitrarily the one or the other has been selected for the purpose of obtaining a consistent picture of cultural development. These methods are essentially forms of classification of the static phenomena of culture according to two distinct principles, and interpretations of these classifications as of historical significance, without, however, any attempt to prove that this interpretation is justifiable. To give an example: It is observed that in most parts of the world there are resemblances between decorative forms that are representative and others that are more or less geometrical. According to the evolutionary point of view, their development is explained in the following manner: the decorative forms are arranged in such order that the most representative forms are placed at the beginning. The other forms are so placed that they show a gradual transition from representative forms to purely conventional geometric forms, and this order is then interpreted as meaning that geometric designs originated from representative designs which gradually degenerated. This method has been pursued, for instance, by Putnam, Stolpe, Balfour, and Haddon, and by Verworn and, in his earlier writings, by von den Steinen. While I do not mean to deny that this development may have occurred, it would be rash to generalize and to claim that in every case the classification which has been made according to a definite principle represents an historical development. The order might as well be reversed and we might begin with a simple geometric element which, by the addition of new traits, might be developed into a representative design, and we might claim that this order represents an historical sequence. Both of these possibilities were considered by Holmes as early as 1885. Neither the one nor the other theory can be established without actual historical proof.

The opposite attitude, namely, origin through diffusion, is exhibited in Heinrich Schurtz's attempt to connect the decorative art of Northwest America with that of Melanesia. The simple fact that in these areas elements occur that may be interpreted as eyes, induced him to assume that both have a common origin, without allowing for the possibility that the pattern in the two areas – each of which shows highly distinctive characteristics – may have developed from independent sources. In this attempt Schurtz followed Ratzel who had already tried to establish connections between Melanesia and Northwest America on the basis of other cultural features.

While ethnographical research based on these two fundamental hypotheses seems to characterize the general tendency of European thought, a different method is at present pursued by the majority of American anthropologists. The difference between the two directions of study may perhaps best be summarized by the statement that American scholars are primarily interested in the dynamic phenomena of cultural change, and try to elucidate cultural history by the application of the results of their studies; and that they relegate the solution of the ultimate question of the relative importance of parallelism of cultural development in distant areas, as against worldwide diffusion, and stability of cultural traits over long periods to a future time when the actual conditions of cultural change are better known. The American ethnological methods are analogous to those of European, particularly of Scandinavian, archaeology, and of the researches into the prehistoric period of the eastern Mediterranean area.

It may seem to the distant observer that American students are engaged in a mass of detailed investigations without much bearing upon the solution of the ultimate problems of a philosophic history of human civilization. I think this interpretation of the American attitude would be unjust because the ultimate questions are as near to our hearts as they are to those of other scholars, only we do not hope to be able to solve an intricate historical problem by a formula.

First of all, the whole problem of cultural history appears to us as a historical problem. In order to understand history it is necessary to know not only how things are, but how they have come to be. In the domain of ethnology, where, for most parts of the world, no historical facts are available except those that may be revealed by archaeological study, all evidence of change can be inferred only by indirect methods. Their character is represented in the researches of students of comparative philology. The method is based on the comparison of static phenomena combined with the study of their distribution. What can be done by this method is well illustrated by Dr Lowie's investigations of the military societies of the Plains Indians, or by the modern investigation of American mythology. It is, of course, true that we can never hope to obtain incontrovertible data relating to the chronological sequence of events, but certain general broad outlines can be ascertained with a high degree of probability, even of certainty.

As soon as these methods are applied, primitive society loses the appearance of absolute stability which is conveyed to the student who sees a certain people only at a certain given time. All cultural forms rather appear in a constant state of flux and subject to fundamental modifications.

It is intelligible why in our studies the problem of dissemination should take a prominent position. It is much easier to prove dissemination than to follow up developments due to inner forces, and the data for such a study are obtained with much greater difficulty. They may, however, be observed in every phenomenon of acculturation in which foreign elements are remodeled according to the patterns prevalent in their new environment, and they may be found in the peculiar local developments of widely spread ideas and activities. The reason why the study of inner development has not been taken up energetically, is not due to the fact that from a theoretical point of view it is unimportant, it is rather due to the inherent methodological difficulties. It may perhaps be recognized that in recent years attention is being drawn to this problem, as is manifested by the investigations on the processes of acculturation and of the interdependence of cultural activities which are attracting the attention of many investigators.

The further pursuit of these inquiries emphasizes the importance of a feature which is common to all historic phenomena. While in natural sciences we are accustomed to consider a given number of causes and to study their effects, in historical happenings we are compelled to consider every phenomenon not only as effect but also as cause. This is true even in the particular application of the laws of physical nature, as, for instance, in the study of astronomy in which the position of certain heavenly bodies at a given moment may be considered as the effect of gravitation, while, at the same time, their particular arrangement in space determines future changes. This relation appears much more clearly in the history of human civilization. To give an example: a surplus of food supply is liable to bring about an increase of population and an increase of leisure, which gives opportunity for occupations that are not absolutely necessary for the needs of every day life. In turn the increase of population and of leisure, which may be applied to new inventions, give rise to a greater food supply and to a further increase in the amount of leisure, so that a cumulative effect results.

Similar considerations may be made in regard to the important problem of the relation of the individual to society, a problem that has to be considered whenever we study the dynamic conditions of change. The activities of the individual are determined to a great extent by his social environment, but in turn his own activities influence the society in which he lives, and may bring about modifications in its form. Obviously, this problem is one of the most important ones to be taken up in a study of cultural changes. It is also beginning to attract the attention of students who are no longer satisfied with the systematic enumeration of standardized beliefs and customs of a tribe, but who begin to be interested in the question of the way in which the individual reacts to his whole social environment, and to the differences of opinion and of mode of action that occur in primitive society and which are the causes of far-reaching changes.

In short then, the method which we try to develop is based on a study of the dynamic changes in society that may be observed at the present time. We refrain from the attempt to solve the fundamental problem of the general development of civilization until we have been able to unravel the processes that are going on under our eyes.

Certain general conclusions may be drawn from this study even now. First of all, the history of human civilization does not appear to us as determined entirely by psychological necessity that leads to a uniform evolution the world over. We rather see that each cultural group has its own unique history, dependent partly upon the peculiar inner development of the social group, and partly upon the foreign influences to which it has been subjected. There have been processes of gradual differentiation as well as processes of leveling down differences between neighboring cultural centers, but it would be quite impossible to understand, on the basis of a single evolutionary scheme, what happened to any particular people. An example of the contrast between the two points of view is clearly indicated by a comparison of the treatment of Zuñi civilization by Frank Hamilton Cushing on the one hand, on the other by modern students, particularly by Elsie Clews Parsons, A. L. Kroeber and Leslie Spier. Cushing believed that it was possible to explain Zuñi culture entirely on the basis of the reaction of the Zuñi mind to its geographical environment, and that the whole of Zuñi culture could be explained as the development which followed necessarily from the position in which the people were placed. Cushing's keen insight into the Indian mind and his thorough knowledge of the most intimate life of the people gave great plausibility to his interpretations. On the other hand, Dr Parsons' studies prove conclusively the deep influence which Spanish ideas have had upon Zuñi culture, and, together with Professor Kroeber's investigations, give us one of the best examples of acculturation that have come to our notice. The psychological explanation is entirely misleading, notwithstanding its plausibility, and the historical study shows us an entirely different picture, in which the unique combination of ancient traits (which in themselves are undoubtedly complex) and of European influences, have brought about the present condition.

Studies of the dynamics of primitive life also show that an assumption of long continued stability such as is demanded by Elliot Smith is without any foundation in fact. Wherever primitive conditions have been studied in detail, they can be proved to be in a state of flux, and it would seem that there is a close parallelism between the history of language and the history of general cultural development. Periods of stability are followed by periods of rapid change. It is exceedingly improbable that any customs of primitive people should be preserved unchanged for thousands of years. Furthermore, the phenomena of acculturation prove that a transfer of customs from one region into another without concomitant changes due to acculturation, are very rare. It is, therefore, very unlikely that ancient Mediterranean customs could be found at the present time practically unchanged in different parts of the globe, as Elliot Smith's theory demands.

While on the whole the unique historical character of cultural growth in each area stands out as a salient element in the history of cultural development, we may recognize at the same time that certain typical parallelisms do occur. We are, however, not so much inclined to look for these similarities in detailed customs but rather in certain dynamic conditions which are due to social or psychological causes that are liable to lead to similar results. The example of the relation between food supply and population to which I referred before may serve as an example. Another type of example is presented in those cases in which a certain problem confronting man may be solved by a limited number of methods only. When we find, for instance, marriage as a universal institution, it may be recognized that marriage is possible only between a number of men and a number of women; a number of men and one woman; a number of women and one man; or one man and one woman. As a matter of fact, all these forms are found the world over and it is, therefore, not surprising that analogous forms should have been adopted quite independently in different parts of the world, and, considering both the general economic conditions of mankind and the character of sexual instinct in the higher animals, it also does not seem surprising that group marriage and polyandrous marriages should be comparatively speaking rare. Similar considerations may also be made in regard to the philosophical views held by mankind. In short, if we look for laws, the laws relate to the effects of physiological, psychological, and social conditions, not to sequences of cultural achievement.

In some cases a regular sequence of these may accompany the development of the psychological or social status. This is illustrated by the sequence of industrial inventions in the Old World and in America, which I consider as independent. A period of food gathering and of the use of stone was followed by the invention of agriculture, of pottery and finally of the use of metals. Obviously, this order is based on the increased amount of time given by mankind to the use of natural products, of tools and utensils, and to the variations that developed with it. Although in this case parallelism seems to exist on the two continents, it would be futile to try to follow out the order in detail. As a matter of fact, it does not apply to other inventions. The domestication of animals, which, in the Old World must have been an early achievement, was very late in the New World, where domesticated animals, except the dog, hardly existed at all at the time of discovery. A slight beginning had been made in Peru with the domestication of the llama, and birds were kept in various parts of the continent.

A similar consideration may be made in regard to the development of rationalism. It seems to be one of the fundamental characteristics of the development of mankind that activities which have developed unconsciously are gradually made the subject of reasoning. We may observe this process everywhere. It appears, perhaps, most clearly in the history of science which has gradually extended the scope of its inquiry over an ever-widening field and which has raised into consciousness human activities that are automatically performed in the life of the individual and of society.

I have not heretofore referred to another aspect of modern ethnology which is connected with the growth of psycho-analysis. Sigmund Freud has attempted to show that primitive thought is in many respects analogous to those forms of individual psychic activity which he has explored by his psychoanalytical methods. In many respects his attempts are similar to the interpretation of mythology by symbolists like Stucken. Rivers has taken hold of Freud's suggestion as well as of the interpretations of Graebner and Elliot Smith, and we find, therefore, in his new writings a peculiar disconnected application of a psychologizing attitude and the application of the theory of ancient transmission.

While I believe some of the ideas underlying Freud's psycho-analytic studies may be fruitfully applied to ethnological problems, it does not seem to me that the one-sided exploitation of this method will advance our understanding of the development of human society. It is certainly true that the influence of impressions received during the first few years of life have been entirely underestimated and that the social behavior of man depends to a great extent upon the earliest habits which are established before the time when connected memory begins, and that many so-called racial or hereditary traits are to be considered rather as a result of early exposure to a certain form of social conditions. Most of these habits do not rise into consciousness and are, therefore, broken with difficulty only. Much of the difference in the behavior of adult male and female may go back to this cause. If, however, we try to apply the whole theory of the influence of suppressed desires to the activities of man living under different social forms, I think we extend beyond their legitimate limits the inferences that may be drawn from the observation of normal and abnormal individual psychology. Many other factors are of greater importance. To give an example: The phenomena of language show clearly that conditions quite different from those to which psycho-analysts direct their attention determine the mental behavior of man. The general concepts underlying language are entirely unknown to most people. They do not rise into consciousness until the scientific study of grammar begins. Nevertheless, the categories of language compel us to see the world arranged in certain definite conceptual groups which, on account of our lack of knowledge of linguistic processes, are taken as objective categories and which, therefore, impose themselves upon the form of our thoughts. It is not known what the origin of these categories may be, but it seems quite certain that they have nothing to do with the phenomena which are the subject of psycho-analytic study.

The applicability of the psycho-analytic theory of symbolism is also open to the greatest doubt. We should remember that symbolic interpretation has occupied a prominent position in the philosophy of all times. It is present not only in primitive life, but the history of philosophy and of theology abounds in examples of a high development of symbolism, the type of which depends upon the general mental attitude of the philosopher who develops it. The theologians who interpreted the Bible on the basis of religious symbolism were no less certain of the correctness of their views, than the psycho-analysts are of their interpretations of thought and conduct based on sexual symbolism. The results of a symbolic interpretation depend primarily upon the subjective attitude of the investigator who arranges phenomena according to his leading concept. In order to prove the applicability of the symbolism of psycho-analysis, it would be necessary to show that a symbolic interpretation from other entirely different points of view would not be equally plausible, and that explanations that leave out symbolic significance or reduce it to a minimum, would not be adequate.

While, therefore, we may welcome the application of every advance in the method of psychological investigation, we cannot accept as an advance in ethnological method the crude transfer of a novel, one-sided method of psychological investigation of the individual to social phenomena the origin of which can be shown to be historically determined and to be subject to influences that are not at all comparable to those that control the psychology of the individual.

# 3

# Method and Scope of Anthropological Fieldwork

*Bronislaw Malinowski*

[. . .]

## III

Imagine yourself suddenly set down surrounded by all your gear, alone on a tropical beach close to a native village, while the launch or dinghy which has brought you sails away out of sight. Since you take up your abode in the compound of some neighbouring white man, trader or missionary, you have nothing to do, but to start at once on your ethnographic work. Imagine further that you are a beginner, without previous experience, with nothing to guide you and no one to help you. For the white man is temporarily absent, or else unable or unwilling to waste any of his time on you. This exactly describes my first initiation into fieldwork on the south coast of New Guinea. I well remember the long visits I paid to the villages during the first weeks; the feeling of hopelessness and despair after many obstinate but futile attempts had entirely failed to bring me into real touch with the natives, or supply me with any material. I had periods of despondency, when I buried myself in the reading of novels, as a man might take to drink in a fit of tropical depression and boredom.

Imagine yourself then, making your first entry into the village, alone or in company with your white cicerone. Some natives flock round you, especially if they smell tobacco. Others, the more dignified and elderly, remain seated where they are. Your white companion has his routine way of treating the natives, and he neither understands, nor is very much concerned with the manner in which you, as an ethnographer, will have to approach them. The first visit leaves you with a hopeful feeling that when you return alone, things will be easier. Such was my hope at least.

I came back duly, and soon gathered an audience around me. A few compliments in pidgin-English on both sides, some tobacco changing hands, induced an atmosphere of mutual amiability. I tried then to proceed to business. First, to begin with subjects which might arouse no suspicion, I started to "do" technology. A few natives were engaged in manufacturing some object or other. It was easy to look at it and obtain the names of the tools, and even some technical expressions about the proceedings, but there the matter ended. It must be borne in mind that pidgin-English is a very imperfect instrument for expressing one's ideas, and that before one gets a good training in framing questions and

Bronislaw Malinowski, pp. 4–25 from *Argonauts of the Western Pacific: An Account of Western Enterprise and Adventure in the Archipelagoes of Melanesian New Guinea* (Prospect Heights, IL: Waveland Press, 1984 [1922]).

understanding answers one has the uncomfortable feeling that free communication in it with the natives will never be attained; and I was quite unable to enter into any more detailed or explicit conversation with them at first. I knew well that the best remedy for this was to collect concrete data, and accordingly I took a village census, wrote down genealogies, drew up plans and collected the terms of kinship. But all this remained dead material, which led no further into the understanding of real native mentality or behaviour, since I could neither procure a good native interpretation of any of these items, nor get what could be called the hang of tribal life. As to obtaining their ideas about religion, and magic, their beliefs in sorcery and spirits, nothing was forthcoming except a few superficial items of folk-lore, mangled by being forced into pidgin English.

Information which I received from some white residents in the district, valuable as it was in itself, was more discouraging than anything else with regard to my own work. Here were men who had lived for years in the place with constant opportunities of observing the natives and communicating with them, and who yet hardly knew one thing about them really well. How could I therefore in a few months or a year, hope to overtake and go beyond them? Moreover, the manner in which my white informants spoke about the natives and put their views was, naturally, that of untrained minds, unaccustomed to formulate their thoughts with any degree of consistency and precision. And they were for the most part, naturally enough, full of the biassed and pre-judged opinions inevitable in the average practical man, whether administrator, missionary, or trader, yet so strongly repulsive to a mind striving after the objective, scientific view of things. The habit of treating with a self-satisfied frivolity what is really serious to the ethnographer; the cheap rating of what to him is a scientific treasure, that is to say, the native's cultural and mental peculiarities and independence – these features, so well known in the inferior amateur's writing, I found in the tone of the majority of white residents.[1]

Indeed, in my first piece of Ethnographic research on the South coast, it was not until I was alone in the district that I began to make some headway; and, at any rate, I found out where lay the secret of effective fieldwork. What is then this ethnographer's magic, by which he is able to evoke the real spirit of the natives, the true picture of tribal life? As usual, success can only be obtained by a patient and systematic application of a number of rules of common sense and well-known scientific principles, and not by the discovery of any marvellous short-cut leading to the desired results without effort or trouble. The principles of method can be grouped under three main headings; first of all, naturally, the student must possess real scientific aims, and know the values and criteria of modern ethnography. Secondly, he ought to put himself in good conditions of work. that is, in the main, to live without other white men, right among the natives. Finally, he has to apply a number of special methods of collecting, manipulating and fixing his evidence. A few words must be said about these foundation stones of fieldwork, beginning with the second as the most elementary.

## IV

### *Proper conditions for ethnographic work*

These, as said, consist mainly in cutting oneself off from the company of other white men, and remaining in as close contact with the natives as possible, which really can only be achieved by camping right in their villages. It is very nice to have a base in a white man's compound for the stores, and to know there is a refuge there in times of sickness and surfeit of native. But it must be far enough away not to become a permanent milieu in which you live and from which you emerge at fixed hours only to "do the village." It should not even be near enough to fly to at any moment for recreation. For the native is not the natural companion for a white man, and after you have been working with him for several hours, seeing how he does his gardens, or letting him tell you items of folklore, or discussing his customs, you will naturally hanker after the company of your own kind. But if you are alone in a village beyond reach of this, you go for a solitary walk for an hour or so, return again and then quite naturally seek out the natives' society, this time as a relief from loneliness, just as you would any other companionship. And by means of this natural intercourse, you learn to know him, and you become familiar with his customs and beliefs far better than when he is a paid, and often bored, informant.

There is all the difference between a sporadic plunging into the company of natives, and being really in contact with them. What does this latter mean? On the Ethnographer's side, it means that his life in the village, which at first is a strange, sometimes unpleasant, sometimes intensely interesting adventure, soon adopts quite a natural course very much in harmony with his surroundings.

Soon after I had established myself in Omarakana (Trobriand Islands), I began to take part, in a way, in the village life, to look forward to the important or festive events, to take personal interest in the gossip and the developments of the small village occurrences; to wake up every morning to a day, presenting itself to me more or less as it does to the native. I would get out from under my mosquito net, to find around me the village life beginning to stir, or the people well advanced in their working day according to the hour and also to the season, for they get up and begin their labours early or late, as work presses. As I went on my morning walk through the village, I could see intimate details of family life, of toilet, cooking, taking of meals; I could see the arrangements for the day's work, people starting on their errands, or groups of men and women busy at some manufacturing tasks. Quarrels, jokes, family scenes, events usually trivial, sometimes dramatic but always significant, formed the atmosphere of my daily life, as well as of theirs. It must be remembered that as the natives saw me constantly every day, they ceased to be interested or alarmed, or made self-conscious by my presence, and I ceased to be a disturbing element in the tribal life which I was to study, altering it by my very approach, as always happens with a newcomer to every savage community. In fact, as they knew that I would thrust my nose into everything, even where a well-mannered native would not dream of intruding, they finished by regarding me as part and parcel of their life, a necessary evil or nuisance, mitigated by donations of tobacco.

Later on in the day, whatever happened was within easy reach, and there was no possibility of its escaping my notice. Alarms about the sorcerer's approach in the evening, one or two big, really important quarrels and rifts within the community, cases of illness, attempted cures and deaths, magical rites which had to be performed, all these I had not to pursue, fearful of missing them, but they took place under my very eyes, at my own doorstep, so to speak. And it must be emphasised whenever anything dramatic or important occurs it is essential to investigate it at the very moment of happening, because the natives cannot but talk about it, are too excited to be reticent, and too interested to be mentally lazy in supplying details. Also, over and over again, I committed breaches of etiquette, which the natives, familiar enough with me, were not slow in pointing out. I had to learn how to behave, and to a certain extent, I acquired "the feeling" for native good and bad manners. With this, and with the capacity of enjoying their company and sharing some of their games and amusements, I began to feel that I was indeed in touch with the natives, and this is certainly the preliminary condition of being able to carry on successful fieldwork.

## V

But the Ethnographer has not only to spread his nets in the right place, and wait for what will fall into them. He must be an active huntsman, and drive his quarry into them and follow it up to its most inaccessible lairs. And that leads us to the more active methods of pursuing ethnographic evidence. It has been mentioned at the end of Division III that the Ethnographer has to be inspired by the knowledge of the most modern results of scientific study, by its principles and aims. I shall not enlarge upon this subject, except by way of one remark, to avoid the possibility of misunderstanding. Good training in theory, and acquaintance with its latest results, is not identical with being burdened with "preconceived ideas." If a man sets out on an expedition, determined to prove certain hypotheses, if he is incapable of changing his views constantly and casting them off ungrudgingly under the pressure of evidence, needless to say his work will be worthless. But the more problems he brings with him into the field, the more he is in the habit of moulding his theories according to facts, and of seeing facts in their bearing upon theory, the better he is equipped for the work. Preconceived ideas are pernicious in any scientific work, but foreshadowed problems are the main endowment of a scientific thinker, and these problems are first revealed to the observer by his theoretical studies.

In Ethnology the early efforts of Bastian, Tylor, Morgan, the German Völkerpsychologen have remoulded the older crude information of travellers, missionaries, etc., and have shown us the importance of applying deeper conceptions and discarding crude and misleading ones.[2]

The concept of animism superseded that of "fetichism" or "devil-worship," both meaningless terms. The understanding of the classificatory systems of relationship paved the way for the brilliant, modern researches on native sociology in the fieldwork of the Cambridge school. The psychological analysis of the German thinkers has brought forth an abundant crop of most valuable information in the results obtained by the recent German expeditions to Africa, South America and the Pacific, while the theoretical works of Frazer, Durkheim and others have already, and will no doubt still for a long time inspire fieldworkers and lead them to new results. The fieldworker relies entirely upon inspiration from theory. Of course he may be also a theoretical thinker and worker, and there he can draw on himself for stimulus. But the two functions are separate, and in actual research they have to be separated both in time and conditions of work.

As always happens when scientific interest turns towards and begins to labour on a field so far only prospected by the curiosity of amateurs, Ethnology has introduced law and order into what seemed chaotic and freakish. It has transformed for us the sensational, wild and unaccountable world of "savages" into a number of well ordered communities, governed by law, behaving and thinking according to consistent principles. The word "savage," whatever association it might have had originally, connotes ideas of boundless liberty, of irregularity, of something extremely and extraordinarily quaint. In popular thinking, we imagine that the natives live on the bosom of Nature, more or less as they can and like, the prey of irregular, phantasmagoric beliefs and apprehensions. Modern science, on the contrary, shows that their social institutions have a very definite organisation, that they are governed by authority, law and order in their public and personal relations, while the latter are, besides, under the control of extremely complex ties of kinship and clanship. Indeed, we see them entangled in a mesh of duties, functions and privileges which correspond to an elaborate tribal, communal and kinship organisation. Their beliefs and practices do not by any means lack consistency of a certain type, and their knowledge of the outer world is sufficient to guide them in many of their strenuous enterprises and activities. Their artistic productions again lack neither meaning nor beauty.

It is a very far cry from the famous answer given long ago by a representative authority who, asked, what are the manners and customs of the natives, answered, "Customs none, manners beastly," to the position of the modern Ethnographer! This latter, with his tables of kinship terms, genealogies, maps, plans and diagrams, proves the existence of an extensive and big organisation, shows the constitution of the tribe, of the clan, of the family; and he gives us a picture of the natives subjected to a strict code of behaviour and good manners, to which in comparison the life at the Court of Versailles or Escurial was free and easy.[3]

Thus the first and basic ideal of ethnographic fieldwork is to give a clear and firm outline of the social constitution, and disentangle the laws and regularities of all cultural phenomena from the irrelevances. The firm skeleton of the tribal life has to be first ascertained. This ideal imposes in the first place the fundamental obligation of giving a complete survey of the phenomena, and not of picking out the sensational, the singular, still less the funny and quaint. The time when we could tolerate accounts presenting us the native as a distorted, childish caricature of a human being are gone. This picture is false, and like many other falsehoods, it has been killed by Science. The field Ethnographer has seriously and soberly to cover the full extent of the phenomena in each aspect of tribal culture studied, making no difference between what is commonplace, or drab, or ordinary, and what strikes him as astonishing and out-of-the-way. At the same time, the whole area of tribal culture *in all its aspects* has to be gone over in research. The consistency, the law and order which obtain within each aspect make also for joining them into one coherent whole.

An Ethnographer who sets out to study only religion, or only technology, or only social organisation cuts out an artificial field for inquiry, and he will be seriously handicapped in his work.

## VI

Having settled this very general rule, let us descend to more detailed consideration of method. The Ethnographer has in the field, according to what has just been said, the duty before him of drawing up all the rules and regularities of tribal life; all that is permanent and fixed; of giving an anatomy of their culture, of depicting the constitution of their society. But these things, though crystallised and set, are nowhere *formulated*. There is no written or explicitly expressed code of laws, and their whole tribal tradition, the whole structure of their society, are embodied in the most elusive of all materials; the human being. But not even in human mind or memory are these laws to be found definitely formulated. The natives obey the forces and commands of the tribal code, but they do not comprehend them; exactly as they obey their instincts and their impulses, but could not lay down a single law of psychology. The regularities in native institutions are an automatic result of the interaction of the mental forces of tradition, and of the material conditions of environment. Exactly as a humble member of any modern institution, whether it be the state, or the church, or the army, is *of* it and *in* it, but has no vision of the resulting integral action of the whole, still less could furnish any account of its organisation, so it would be futile to attempt questioning a native in abstract, sociological terms. The difference is that, in our society, every institution has its intelligent members, its historians, and its archives and documents, whereas in a native society there are none of these. After this is realised an expedient has to be found to overcome this difficulty. This expedient for an Ethnographer consists in collecting concrete data of evidence, and drawing the general inferences for himself. This seems obvious on the face of it, but was not found out or at least practised in Ethnography till fieldwork was taken up by men of science. Moreover, in giving it practical effect, it is neither easy to devise the concrete applications of this method, nor to carry them out systematically and consistently.

Though we cannot ask a native about abstract, general rules, we can always enquire how a given case would be treated. Thus for instance, in asking how they would treat crime, or punish it, it would be vain to put to a native a sweeping question such as, "How do you treat and punish a criminal?" for even words could not be found to express it in native, or in pidgin. But an imaginary case, or still better, a real occurrence, will stimulate a native to express his opinion and to supply plentiful information. A real case indeed will start the natives on a wave of discussion, evoke expressions of indignation, show them taking sides – all of which talk will probably contain a wealth of definite views, of moral censures, as well as reveal the social mechanism set in motion by the crime committed. From there, it will be easy to lead them on to speak of other similar cases, to remember other actual occurrences or to discuss them in all their implications and aspects. From this material, which ought to cover the widest possible range of facts, the inference is obtained by simple induction. The *scientific* treatment differs from that of good common sense, first in that a student will extend the completeness and minuteness of survey much further and in a pedantically systematic and methodical manner; and secondly, in that the scientifically trained mind, will push the inquiry along really relevant lines, and towards aims possessing real importance. Indeed, the object of scientific training is to provide the empirical investigator with a *mental chart*, in accordance with which he can take his bearings and lay his course.

To return to our example, a number of definite cases discussed will reveal to the Ethnographer the social machinery for punishment. This is one part, one aspect of tribal authority. Imagine further that by a similar method of inference from definite data, he arrives at understanding leadership in war, in economic enterprise, in tribal festivities – there he has at once all the data necessary to answer the questions about tribal government and social authority. In actual fieldwork, the comparison of such data, the attempt to piece them together, will often reveal rifts and gaps in the information which lead on to further investigations.

From my own experience, I can say that, very often, a problem seemed settled, everything fixed and clear, till I began to write down a short preliminary sketch of my results. And only then, did I see the enormous deficiencies, which would show me where lay new problems, and lead me on to new work. In fact, I spent a few months between my first and second expeditions, and over a year between that and the subsequent one, in going over all my material, and making parts of it almost ready for publication each time, though each time I knew I would have to re-write it. Such cross-fertilisation of constructive work and observation, I found most valuable, and I do not think I could have made real headway without it. I give this bit of my own history merely to show that what has been said so far is not only an empty programme, but the result of personal experience. In this volume, the description is given of a big institution connected with ever so many associated activities, and presenting many aspects. To anyone who reflects on the subject, it will be clear that the information about a phenomenon of such high complexity and of so many ramifications, could not be obtained with any degree of exactitude and completeness, without a constant interplay of constructive attempts and empirical checking. In fact, I have written up an outline of the Kula institution at least half a dozen times while in the field and in the intervals between my expeditions. Each time, new problems and difficulties presented themselves.

The collecting of concrete data over a wide range of facts is thus one of the main points of field method. The obligation is not to enumerate a few examples only, but to exhaust as far as possible all the cases within reach; and, on this search for cases, the investigator will score most whose mental chart is clearest. But, whenever the material of the search allows it, this mental chart ought to be transformed into a real one; it ought to materialise into a diagram, a plan, an exhaustive, synoptic table of cases. Long since, in all tolerably good modern books on natives, we expect to find a full list or table of kinship terms, which includes all the data relative to it, and does not just pick out a few strange and anomalous relationships or expressions. In the investigation of kinship, the following up of one relation after another in concrete cases leads naturally to the construction of genealogical tables. Practised already by the best early writers, such as Munzinger, and, if I remember rightly, Kubary, this method has been developed to its fullest extent in the works of Dr. Rivers. Again, studying the concrete data of economic transactions, in order to trace the history of a valuable object, and to gauge the nature of its circulation, the principle of completeness and thoroughness would lead to construct tables of transactions, such as we find in the work of Professor Seligman. It is in following Professor Seligman's example in this matter that I was able to settle certain of the more difficult and detailed rules of the Kula. The method of reducing information, if possible, into charts or synoptic tables ought to be extended to the study of practically all aspects of native life. All types of economic transactions may be studied by following up connected, actual cases, and putting them into a synoptic chart; again, a table ought to be drawn up of all the gifts and presents customary in a given society, a table including the sociological, ceremonial, and economic definition of every item. Also, systems of magic, connected series of ceremonies, types of legal acts, all could be charted, allowing each entry to be synoptically defined under a number of headings. Besides this, of course, the genealogical census of every community, studied more in detail, extensive maps, plans and diagrams, illustrating ownership in garden land, hunting and fishing privileges, etc., serve as the more fundamental documents of ethnographic research.

A genealogy is nothing else but a synoptic chart of a number of connected relations of kinship. Its value as an instrument of research consists in that it allows the investigator to put questions which he formulates to himself *in abstracto*, but can put concretely to the native informant. As a document, its value consists in that it gives a number of authenticated data, presented in their natural grouping. A synoptic chart of magic fulfils the same function. As an instrument of research, I have used it in order to ascertain, for instance, the ideas about the nature of magical power. With a chart before me, I could easily and conveniently go over one item after the other, and note down the relevant practices and beliefs contained in each of them. The answer to my abstract problem could then be obtained by drawing a general inference from all the cases . . . I cannot enter further into the discussion of this question, which would need further distinctions, such as between a chart of concrete, actual data, such as is a genealogy, and a chart summarising the outlines of a custom or belief, as a chart of a magical system would be.

Returning once more to the question of methodological candour, . . . I wish to point out here, that the procedure of concrete and tabularised presentation of data ought to be applied first to the Ethnographer's own credentials. That is, an Ethnographer, who wishes to be trusted, must show clearly and concisely, in a tabularised form, which are his own direct observations, and which the indirect information that form the bases of his account. The Table will serve as an example of this procedure and help the reader of this book to form an idea of the trustworthiness of any statement he is specially anxious to check. With the help of this Table and the many references scattered throughout the text, as to how, under what circumstances, and with what degree of accuracy I arrived at a given item of knowledge, there will, I hope remain no obscurity whatever as to the sources of the book.

To summarise the first, cardinal point of method, I may say each phenomenon ought to be studied through the broadest range possible of its concrete manifestations; each studied by an exhaustive survey of detailed examples. If possible, the results ought to be tabulated into some sort of synoptic chart, both to be used as an instrument of study, and to be presented as an ethnological document. With the help of such documents and such study of actualities the clear outline of the framework of the natives' culture in the widest sense of the word, and the constitution of their society, can be presented. This method could be called *the method of statistic documentation by concrete evidence.*

## VII

Needless to add, in this respect, the scientific fieldwork is far above even the best amateur productions. There is, however, one point in which the latter often excel. This is, in the presentation of intimate touches of native life, in bringing home to us these aspects of it with which one is made familiar only through being in close contact with the natives, one way or the other, for a long period of time. In certain results of scientific work – especially that which has been called "survey work" – we are given an excellent skeleton, so to speak, of the tribal constitution, but it lacks flesh and blood. We learn much about the framework of their society, but within it, we cannot perceive or imagine the realities of human life, the even flow of everyday events, the occasional ripples of excitement over a feast, or ceremony, or some singular occurrence. In working out the rules and regularities of native custom, and in obtaining a precise formula for them from the collection of data and native statements, we find that this very precision is foreign to real life, which never adheres rigidly to any rules. It must be supplemented by the observation of the manner in which a given custom is carried out, of the behaviour of the natives in obeying the rules so exactly formulated by the ethnographer, of the very exceptions which in sociological phenomena almost always occur.

If all the conclusions are solely based on the statements of informants, or deduced from objective documents, it is of course impossible to supplement them in actually observed data of real behaviour. And that is the reason why certain works of amateur residents of long standing, such as educated traders and planters, medical men and officials, and last, but not least, the few intelligent and unbiassed missionaries to whom Ethnography owes so much, surpass in plasticity and in vividness most of the purely scientific accounts. But if the specialised fieldworker can adopt the conditions of living described above, he is in a far better position to be really in touch with the natives than any other white resident. For none of them lives right in a native village, except for very short periods, and everyone has his own business, which takes up a considerable part of his time. Moreover, if, like a trader or a missionary or an official he enters into active relations with the native, if he has to transform or influence or make use of him, this makes a real, unbiassed, impartial observation impossible, and precludes all-round sincerity, at least in the case of the missionaries and officials.

Living in the village with no other business but to follow native life, one sees the customs, ceremonies and transactions over and over again, one has examples of their beliefs as they are actually lived through, and the full body and blood of actual native life fills out soon the skeleton of abstract constructions. That is the reason why, working under such conditions as previously described, the Ethnographer is enabled to add something essential to the bare outline of tribal constitution, and to supplement it by all the details of behaviour, setting and small incident. He is able in each case to state whether an act is public or private; how a public assembly behaves, and what it looks like; he can judge whether an event is ordinary or an exciting and singular one; whether natives bring to it a great deal of sincere and earnest spirit, or perform it in fun; whether they do it in a perfunctory manner, or with zeal and deliberation.

In other words, there is a series of phenomena of great importance which cannot possibly be recorded by questioning or computing documents, but have to be observed in their full actuality. Let us call them *the imponderabilia of actual life*. Here belong such things as the routine of a man's working day, the details of his care of the body, of the manner of taking food and preparing it; the tone of conversational and social life around the village fires, the existence of strong friendships or hostilities, and of passing sympathies and dislikes between people; the subtle yet unmistakable manner in which personal vanities and ambitions are reflected in the behaviour of the individual and in the emotional reactions of those who surround him. All these facts can and ought to be scientifically formulated and recorded, but it is necessary that this be done, not by a superficial registration of details, as is usually done by untrained observers, but with an effort at penetrating the mental attitude expressed in them. And that is the reason why the work of scientifically trained observers, once seriously applied to the study of this aspect, will, I believe, yield results of surpassing value. So far, it has been done only by amateurs, and therefore done, on the whole, indifferently.

Indeed, if we remember that these imponderable yet all important facts of actual life are part of the real substance of the social fabric, that in them are spun the innumerable threads which keep together the family, the clan, the village community, the tribe – their significance becomes clear. The more crystallised bonds of social grouping, such as the definite ritual, the economic and legal duties, the obligations, the ceremonial gifts and formal marks of regard, though equally important for the student, are certainly felt less strongly by the individual who has to fulfil them. Applying this to ourselves, we all know that "family life" means for us, first and foremost, the atmosphere of home, all the innumerable small acts and attentions in which are expressed the affection, the mutual interest, the little preferences, and the little antipathies which constitute intimacy. That we may inherit from this person, that we shall have to walk after the hearse of the other, though sociologically these facts belong to the definition of "family" and "family life," in personal perspective of what family truly is to us, they normally stand very much in the background.

Exactly the same applies to a native community, and if the Ethnographer wants to bring their real life home to his readers, he must on no account neglect this. Neither aspect, the intimate, as little as the legal, ought to be glossed over. Yet as a rule in ethnographic accounts we have not both but either the one or the other – and, so far, the intimate one has hardly ever been properly treated. In all social relations besides the family ties, even those between mere tribesmen and, beyond that, between hostile or friendly members of different tribes, meeting on any sort of social business, there is this intimate side, expressed by the typical details of intercourse, the tone of their behaviour in the presence of one another. This side is different from the definite, crystallised legal frame of the relationship, and it has to be studied and stated in its own right.

In the same way, in studying the conspicuous acts of tribal life, such as ceremonies, rites, festivities, etc., the details and tone of behaviour ought to be given, besides the bare outline of events. The importance of this may be exemplified by one instance. Much has been said and written about survival. Yet the survival character of an act is expressed in nothing so well as in the concomitant behaviour, in the way in which it is carried out. Take any example from our own culture, whether it be the pomp and pageantry of a state ceremony, or a picturesque custom kept up by street urchins, its "outline" will not tell you whether the rite flourishes still with full vigour in the hearts of those who perform it or assist at the performance or whether they regard it as almost a dead thing, kept alive for tradition's sake. But observe and fix the data of their behaviour, and at once the degree of vitality of the act will become clear. There is no doubt, from all points of sociological, or psychological analysis, and in any question of theory, the manner and type of behaviour observed in the performance of an act is of the highest importance. Indeed behaviour is a fact, a relevant fact, and one that can be recorded. And foolish indeed and short-sighted would be the man of science who would pass by a whole class of phenomena, ready to be garnered, and leave them to waste, even though he did not see at the moment to what theoretical use they might be put!

As to the actual method of observing and recording in fieldwork these *imponderabilia of actual life and of typical behaviour*, there is no doubt that the personal equation of the observer comes in here more prominently, than in the collection of crystallised, ethnographic data. But here also the main endeavour must be to let facts speak for themselves. If in making a daily round of the village, certain small incidents, characteristic forms of taking food, of conversing, of doing work are found occurring over and over again, they should be noted down at once. It is also important that this work of collecting and fixing impressions should begin early in the course of working out a district.

Because certain subtle peculiarities, which make an impression as long as they are novel, cease to be noticed as soon as they become familiar. Others again can only be perceived with a better knowledge of the local conditions. An ethnographic diary, carried on systematically throughout the course of one's work in a district would be the ideal instrument for this sort of study. And if, side by side with the normal and typical, the ethnographer carefully notes the slight, or the more pronounced deviations from it, he will be able to indicate the two extremes within which the normal moves.

In observing ceremonies or other tribal events . . . it is necessary, not only to note down those occurrences and details which are prescribed by tradition and custom to be the essential course of the act, but also the Ethnographer ought to record carefully and precisely, one after the other, the actions of the actors and of the spectators. Forgetting for a moment that he knows and understands the structure of this ceremony, the main dogmatic ideas underlying it, he might try to find himself only in the midst of an assembly of human-beings, who behave seriously or jocularly, with earnest concentration or with bored frivolity, who are either in the same mood as he finds them every day, or else are screwed up to a high pitch of excitement, and so on and so on. With his attention constantly directed to this aspect of tribal life, with the constant endeavour to fix it, to express it in terms of actual fact, a good deal of reliable and expressive material finds its way into his notes. He will be able to "set" the act into its proper place in tribal life, that is to show whether it is exceptional or commonplace, one in which the natives behave ordinarily, or one in which their whole behaviour is transformed. And he will also be able to bring all this home to his readers in a clear, convincing manner.

Again, in this type of work, it is good for the Ethnographer sometimes to put aside camera, note book and pencil, and to join in himself in what is going on. He can take part in the natives' games, he can follow them on their visits and walks, sit down and listen and share in their conversations. I am not certain if this is equally easy for everyone – perhaps the Slavonic nature is more plastic and more naturally savage than that of Western Europeans – but though the degree of success varies, the attempt is possible for everyone. Out of such plunges into the life of the natives – and I made them frequently not only for study's sake but because everyone needs human company – I have carried away a distinct feeling that their behaviour, their manner of being, in all sorts of tribal transactions, became more transparent and easily understandable than it had been before. All these methodological remarks, the reader will find again illustrated in the following chapters.

## VIII

Finally, let us pass to the third and last aim of scientific fieldwork, to the last type of phenomenon which ought to be recorded in order to give a full and adequate picture of native culture. Besides the firm outline of tribal constitution and crystallised cultural items which form the skeleton, besides the data of daily life and ordinary behaviour, which are, so to speak, its flesh and blood, there is still to be recorded the spirit – the natives' views and opinions and utterances. For, in every act of tribal life, there is, first, the routine prescribed by custom and tradition, then there is the manner in which it is carried out, and lastly there is the commentary to it, contained in the natives' mind. A man who submits to various customary obligations, who follows a traditional course of action, does it impelled by certain motives, to the accompaniment of certain feelings, guided by certain ideas. These ideas, feelings, and impulses are moulded and conditioned by the culture in which we find them, and are therefore an ethnic peculiarity of the given society. An attempt must be made therefore, to study and record them.

But is this possible? Are these subjective states not too elusive and shapeless? And, even granted that people usually do feel or think or experience certain psychological states in association with the performance of customary acts, the majority of them surely are not able to formulate these states, to put them into words. This latter point must certainly be granted, and it is perhaps the real Gordian knot in the study of the facts of social psychology. Without trying to cut or untie this knot, that is to solve the problem theoretically, or to enter further into the field of general methodology, I shall make directly for the question of practical means to overcome some of the difficulties involved.

First of all, it has to be laid down that we have to study here stereotyped manners of thinking and feeling. As sociologists, we are not interested in what A or B may feel *qua* individuals, in the accidental course of their own personal experiences – we are interested only in what they feel and think *qua* members of a given community. Now in this capacity, their mental states receive a certain stamp, become stereotyped by the institutions in which they live, by the influence of tradition and folk-lore, by the very vehicle of thought, that is by language. The social and cultural environment in which they move forces them to think and feel in a definite manner. Thus, a man who lives in a polyandrous community cannot experience the same feelings of jealousy, as a strict monogynist, though he might have the elements of them. A man who lives within the sphere of the Kula cannot become permanently and sentimentally attached to certain of his possessions, in spite of the fact that he values them most of all. These examples are crude, but better ones will be found in the text of this book.

So, the third commandment of fieldwork runs: Find out the typical ways of thinking and feeling, corresponding to the institutions and culture of a given community, and formulate the results in the most convincing manner. What will be the method of procedure? The best ethnographical writers – here again the Cambridge school with Haddon, Rivers, and Seligman rank first among English Ethnographers – have always tried to quote *verbatim* statements of crucial importance.

They also adduce terms of native classification; sociological, psychological and industrial *termini technici*, and have rendered the verbal contour of native thought as precisely as possible. One step further in this line can be made by the Ethnographer, who acquires a knowledge of the native language and can use it as an instrument of inquiry. In working in the Kiriwinian language, I found still some difficulty in writing down the statement directly in translation which at first I used to do in the act of taking notes. The translation often robbed the text of all its significant characteristics – rubbed off all its points – so that gradually I was led to note down certain important phrases just as they were spoken, in the native tongue. As my knowledge of the language progressed, I put down more and more in Kiriwinian, till at last I found myself writing exclusively in that language, rapidly taking notes, word for word, of each statement. No sooner had I arrived at this point, than I recognised that I was thus acquiring at the same time an abundant linguistic material, and a series of ethnographic documents which ought to be reproduced as I had fixed them, besides being utilised in the writing up of my account.[4] This *corpus inscriptionum Kiriwiniensium* can be utilised, not only by myself, but by all those who, through their better penetration and ability of interpreting them, may find points which escape my attention, very much as the other *corpora* form the basis for the various interpretations of ancient and prehistoric cultures; only, these ethnographic inscriptions are all decipherable and clear, have been almost all translated fully and unambiguously, and have been provided with native cross-commentaries or *scholia* obtained from living sources.

No more need be said on this subject here, . . . The *Corpus* will of course be published separately at a later date.

## IX

Our considerations thus indicate that the goal of ethnographic fieldwork must be approached through three avenues:

1 *The organisation of the tribe, and the anatomy of its culture* must be recorded in firm, clear outline. The method of *concrete, statistical documentation* is the means through which such an outline has to be given.

2 Within this frame, the *imponderabilia of actual life*, and the *type of behaviour* have to be filled in. They have to be collected through minute, detailed observations, in the form of some sort of ethnographic diary, made possible by close contact with native life.

3 A collection of ethnographic statements, characteristic narratives, typical utterances, items of folk-lore and magical formulæ has to be given as a *corpus inscriptionum*, as documents of native mentality.

These three lines of approach lead to the final goal, of which an Ethnographer should never lose sight. 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, the values are slightly different; people aspire after different aims, follow different impulses, yearn after a different form of happiness. 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.

These generalities the reader will find illustrated in the following chapters. We shall see there the savage striving to satisfy certain aspirations, to attain his type of value, to follow his line of social ambition. We shall see him led on to perilous and difficult enterprises by a tradition of magical and heroical exploits, shall see him following the lure of his own romance. Perhaps as we read the account of these remote customs there may emerge a feeling of solidarity with the endeavours and ambitions of these natives. Perhaps man's mentality will be revealed to us, and brought near, along some lines which we never have followed before. Perhaps through realising human nature in a shape very distant and foreign to us, we shall have some light shed on our own. In this, and in this case only, we shall be justified in feeling that it has been worth our while to understand these natives, their institutions and customs, and that we have gathered some profit from the Kula.

# *Part II*
# Fieldwork Identity

*Antonius C. G. M. Robben*

Bronislaw Malinowski was sensitive to the social life on Kiriwina Island but he did not critically examine his own role and feelings to the Trobrianders, even though his loneliness filters through the opening sentence of the excerpt from his "Method and Scope of Anthropological Fieldwork" (Chapter 3). He had written an intimate diary about his problems and depressions, and his chagrin at the local population, but these revelations only became available after his death (Malinowski 1967). His student Hortense Powdermaker was among the first to openly reveal the personal hardships, role conflicts, and identity crises of fieldworkers. Laura Bohannan had preceded Powdermaker with *Return to Laughter* (Chapter 36) by more than a decade, but she had published under a pseudonym and written in a popularizing style. Bohannan and Powdermaker initiated a trend of self-reflection on the practice of fieldwork that became more prominent in anthropology than in any other scholarly discipline. The lengthy immersion in foreign cultures, the status of ethnographers as outsiders, the interactive anthropological research methods in general and participant observation in particular, and the nature of fieldwork as a professional rite of passage with often far-reaching effects on the self, made the concern for identity during fieldwork of great and enduring importance from the 1960s onward.

Hortense Powdermaker carried out her first fieldwork in 1929–30 among the Lesu on the island of New Ireland in Melanesia. In the excerpt "A Woman Going Native," taken from her book *Stranger and Friend: The Way of an Anthropologist* (1967), Powdermaker demonstrates how her gender and ethnic status influenced her research. Melanesian societies enforce a rather strict division between men and women into different social domains, and therefore she drifts naturally to those roles which women occupy in Lesu society: working in the taro gardens, preparing food, carrying and delivering babies, and dancing at women's feasts. Her research among men carries more restrictions because of her female status. It consists mostly of ethnographic interviews, even though she participates in fishing, feasts, and mortuary rituals whenever invited. Fortunately, Powdermaker is not fully categorized as a woman by the Lesu and is therefore able to cross gender boundaries with relative ease. Still, this gender neutrality has the cost of not being confided in by either men or women. Only after she throws herself fully into a dance during the incorporation ritual of boys who had been initiated into manhood does Powdermaker become accepted by the women, and is, for the duration of the ceremony, treated by men with the same symbolic gender hostility as Lesu women. Such treatment, however benign, cannot but raise the awareness of anthropologists about the impact of gender on their data collection and research results. Powdermaker concludes that female anthropologists seem to have easier access to both sexes than male anthropologists, whose contact with women is often regarded by men as threatening. Yet, Gregory (1984) has argued the contrary, while Golde (1986) has pointed out that once a woman has been accepted into a society, she is expected to conform to the prevailing gender roles and has less freedom to move around at will than men in comparable situations (see Wolf 1996 for a feminist critique of fieldwork).

After completing her research among the Lesu, Hortense Powdermaker returned to the United States and conducted fieldwork on African Americans in Mississippi between 1932 and 1934. She traced this interest to her student days when she rode a crowded streetcar on a blistering hot day in Baltimore. A white woman complained to her about the foul air around the sweaty African American workers. Powdermaker noticed the same odor, took a few steps towards the white workers, discovered that they smelled just as much of perspiration, and finally realized that in fact she herself reeked equally bad, after spending the day working on her summer job at the playground (Powdermaker 1967:132). This anecdote reveals Powdermaker's anthropological intuition, even before her formal training: she noticed a racial remark, tested its validity, ascertained it to be unfounded, and then reflected on her similar personal state.

Powdermaker concealed her Jewish identity in the Mississippi Bible Belt community and tried to establish a good rapport with black and white. This was a time of strong Southern racial segregation, when lynchings of African Americans were common and the Ku Klux Klan was strong. Understandably, Powdermaker had to walk a fine line between the white and black neighborhoods in the town she called Indianola. Her confrontation with the exploitation of African American fellow-citizens, the racism of the white inhabitants, and the fear between both groups, made her conscious of her own status as a white Jewish woman in a segregated American society. She also began to doubt whether she would have behaved differently toward African Americans had she been born in Indianola, hemmed in by social taboos and racial stereotypes. She became insecure about her own identity as she shuttled back and forth between blacks and whites, adopting as if through osmosis some of the qualities of each group. Just as she had straddled the gender gap among the Lesu, she crossed the color barrier as if belonging to both segregated communities.

Powdermaker went through another serious crisis when a lynching party set out to hunt down an African American accused of raping a white woman. Feeling powerless, she decided to retreat into her role as the neutral observer trying to make sense of her fellow-citizens' behavior. This distancing must have been a form of self-protection against the racial discrimination and blatant social disparity she was seeing in her country. Her ethnographic initiation on New Ireland allowed her to cope with her ambiguous social role in Mississippi, distance herself by assuming a professional attitude, and overcome her depressions, fears, and anxieties.

Powdermaker's slow entry and acceptance into female Lesu society and less than full participation in a strongly segregated Indianola have the characteristics of a rite of passage. Arnold van Gennep has argued that "The life of an individual in any society is a series of passages from one age to another and from one occupation to another" (1960:2–3). Norris Brock Johnson (1984), in his article "Sex, Color, and Rites of Passage in Ethnographic Research," demonstrates that fieldworkers undergo a similar transition. They bid farewell to their country or city, reappear as strangers in their host society or community, and pass a period of time in limbo as they await acceptance. They learn local social rules and cultural practices, become eager apprentices, and will finally be incorporated as full, albeit temporary residents. Johnson draws special attention to the influence of the ethnographer's identity, in terms of skin color and gender, on the form, degree, and length of the liminal period between arrival and admission.

Johnson traveled to the Caribbean island of Bequia as a young, single, bearded African American man interested in studying traditional boatmaking. The recent Marxist revolution on nearby Grenada and the political turmoil on Bequia raised suspicions about him being a revolutionary. People kept their distance and it took nearly a week before he was able to meet two highly esteemed shipwrights. His politics were now perhaps beyond question, but what about his gender identity? Was he really up to the local standards of a "real man" who was worthy of respect, knowledgeable about boatmaking, skillful with his hands, and competent at sea? Johnson passed the test as he sailed a small boat across the bay of Bequia, turning himself from an outsider into an apprentice boatbuilder. It is crucial to realize that this passage reveals something about the fieldworker's various social identities and indicates that acceptance translates into privileged knowledge and a respectable social status that otherwise would not have been forthcoming. Johnson also shared in the banter about women and sex, thus revealing another aspect of Bequian male identity as well as showing its clear gender divisions. In other words, the successful passage and incorporation into the professional world of respected shipwrights had a direct effect on the quality of his fieldwork and revealed crucial aspects of Bequian society.

Johnson's identity as a black American helped his admission to the world of black Caribbean shipwrights but had hindered his previous fieldwork in a primary public school in the American Midwest (see Sudarkasa 1986 for comparable experiences of an African American woman in Africa). Just as in Bequia, he had difficulty entering this small social universe, encountering strong spatial and social divisions between men and women, and a prominent gender stratification among teachers and administration. Johnson's male status raised such suspicion and social distancing from the female teachers that it took several months before he earned their trust. He accomplished this by showing them his respect, displaying his teaching skills and knowledge about school affairs, avoiding male company and, unlike the male staff, showing a proper sexual restraint toward the women. He became incorporated into the school community, even though his contacts were closer with black than with white female teachers. Clearly, skin color and gender roles may be so prescriptive of social interaction in many complex societies that they influence the reception of ethnographers, and limit their access to particular social groups and their knowledge. Competence helps to bridge some gaps, while a genuine affinity with and affection for a culture will open other doors.

Powdermaker and Johnson demonstrated the influence of gender and ethnicity on fieldwork but did not discuss the importance of sexuality. Anthropologists have shown a keen interest in the sex lives of other societies, but they have failed to examine their own erotic subjectivity. Kulick (1995) attributes this neglect to anthropology's objectivist approach to the study of culture and its rejection of personal narratives, at least till the postmodern turn of the 1970s. Furthermore, sexuality continues to be a taboo subject in Western culture while the male heterosexual anthropological community has concealed the sexist, racist, and colonialist biases hiding behind much anthropological discourse about sexuality, argues Kulick. Manda Cesara (1982) was the first anthropologist to address at length her sexual feelings and affairs in the field, albeit under a pseudonym. Several collections on fieldwork and sexuality have appeared since then, especially by gay and lesbian anthropologists (e.g., Kulick and Willson 1995, Lewin and Leap 1996). The issue of sexual identity is of particular relevance to them as members of a group stigmatized in almost every society. The question of whether homosexual anthropologists should reveal their sexual identity in a largely heterosexual world is a matter of debate. Concealment versus openness is of a different relevance when studying neutral or homosexual topics, but the relative advantages of either strategy are highly dependent on the host culture, as some may be hostile while others will be indifferent or sympathetic to the forthright fieldworker.

Ethical concerns about the inequality of intimate relations in the field have made teachers urge students to refrain from sexual engagements with research collaborators. Aside from the fact that such sexual encounters will nevertheless take place, a far more interesting issue is to what extent the fieldworker's erotic subjectivity enters into the gathering and interpretation of research data. Many anthropologists may not act on their sexual desires in the field, but the presence and even interference of erotic feelings and fantasies during fieldwork is beyond question. Do these emotions undermine the gathering of data and take away the fieldworker's critical objectivity, or do they instead enrich the fieldwork experience and open new cultural interpretations? Kate Altork (1995) addresses these issues in the selection "Walking the Fire Line: The Erotic Dimension of the Fieldwork Experience."

Altork conducted fieldwork among 1,100 male and about 30 female firefighters living in a temporary base camp in the forests of Idaho. The overwhelming expansiveness of the natural environment and the danger of forest fires increased Altork's sensory perception and stimulated her sexual and emotional passions. These erotic feelings were not simply aroused by her physical attraction to the male firefighters and the seduction of their innuendoes, gazes, and flirtations, but especially by the human bonding and camaraderie within an isolated community built around the common mission of fighting forest fires. Altork advocates a fieldwork approach that gives due attention to the sensual and sexual as well as the intellectual and analytic dimensions of fieldwork because the two are inextricably linked in the human condition. Such complete immersion in a field setting, and a conscious awareness of emotional and erotic attachments during fieldwork, will heighten the senses and contribute significantly to a deeper emotional understanding and more comprehensive rendition of the people under study and, ultimately, of the fieldworker's self.

Anthony P. Cohen (1992) takes these reflections about social roles and identities one step further in the article "Self-Conscious Anthropology" by examining the ethnographer's self inside and outside the field, and his or her intersubjective relation with research participants. There is a paradox here in the understanding of the fieldworker's self and that of others: whereas ethnographers tend to give more importance to what a community thinks about its members than what the members think about themselves, fieldworkers rely more on their self-knowledge through participant observation as a source of cultural understanding than on the community's views of them (see Basso 1979, Okely and Callaway 1992, and Reed-Danahay 1997 for notable exceptions). Fieldworkers have a tendency to regard people as rather indistinct bearers of culture. They are overwhelmed by the ethnographic task at hand, the time constraints on their research period, and the impossibility to know all members in depth and fully account for their uniqueness. Furthermore, they have been trained in an epistemological tradition that privileges the social over the personal. In other words, anthropologists often see people as society-driven rather than self-driven, and more in terms of personhood than of selfhood. Instead, Cohen proposes to acknowledge the intrinsic complexity of the self, to recognize the self's creative agency, and to examine local concepts of selfhood (see Cohen 1994 for a theoretical development of the concept of self; also Battaglia 1995).

Empathy plays a crucial role in this research agenda because, like everybody else, fieldworkers understand themselves by mentally observing their own social position and selfhood through the perspectives of others. At the same time, Cohen emphasizes that there are discrepancies between how we see ourselves (and by implication how others whom we study see themselves), how others see us, and how we imagine others see us. Cohen questions the opinion of cultural-relativist anthropologists who regard "the self" as a Western cultural construct. He argues that other cultures may have different notions of selfhood but that everywhere the self has an inner state, a certain degree of autonomy with an internal dialogue, and a personal sense of time and space.

What these four contributions show is that fieldwork is not a detached activity carried out by an objective observer but that subjective experiences and selfhood are part and parcel of fieldwork and its results. The ethnographer's multiple social identities and his or her dynamic self may be liabilities but also research assets. Anthropologists may use their gender, sexual orientation, skin color, physical skills, nationality, age, marital status, parenthood, and self to obtain data that are unavailable to those with different personal assets (see Okely and Callaway 1992; Whitehead and Conaway 1986). Fieldwork involves a negotiation between ethnographers and research collaborators into which each of their unique constitutions enters.

# 4

# A Woman Going Native

## *Hortense Powdermaker*

### X A Woman Alone in the Field

In casual daily life I was much more with the women than with the men; the sexes were quite separate in their social and economic life. I often sat with the women of my hamlet and of neighboring ones, watching them scrape the taro, the staple of their diet, and prepare it for baking between hot stones. Notes on the different ways of preparing taro became so voluminous that I sometimes thought of writing a Melanesian cook book. I was apparently compulsive about writing everything down, but I justified the fullness of the cooking notes by saying that they illustrated Melanesian ingenuity and diversity with limited resources. More important than the cooking notes were my observations on the relationships of the women with their daughters and with each other; listening to the good-humored gossip provided many clues for further questions and added subtlety to my understanding.

Pulong, whom I saw daily, was in the middle stage of her pregnancy, and I could observe her customs such as not eating a certain fish believed to "fight" an embryo in the stomach. Full details about pregnancy customs and taboos came later and gradually. When I first asked Pulong if there was any way of preventing pregnancy, she said she did not understand and looked vague. It seemed evident that she did not want to talk about the subject. Somewhat later, she and my adopted "mother" and a clan "sister" brought me leaves that they said would produce an abortion if chewed in the early stages of pregnancy. I put them carefully in a botanical press for identification when I returned home.

It was taken for granted that I should go to all the women's feasts in or near Lesu. The morning of a feast was usually spent in preparing the food; then came the dances, distribution of food, and speeches. At one such feast in a nearby village to celebrate the birth of a baby, the hundred or so women were in a particularly gay mood, which I enjoyed. One woman, a born jester, who was not dancing because her relationship to the newborn baby precluded it, grabbed the drum and began doing amusing antics. It was like an impromptu skit and all the onlookers laughed loudly. Encouraged, she continued. Then came the distribution of bundles of baked taro, most of it to be carried home, and the women's speeches praising the ability of their respective husbands as providers of food. However, one complained that her husband was lazy and did not bring her fish often enough. In the late afternoon, my Lesu friends and I trudged home. A basket of food and sometimes a baby was on each woman's back (except mine), and

Hortense Powdermaker, "A Woman Alone in the Field," pp. 108–14, "Going Native?," pp. 115–19, and "White Society," pp. 183–90, 196–8 from *Stranger and Friend: The Way of an Anthropologist* (London: Secker & Warburg, 1967). Copyright © by Hortense Powdermaker.

although we were all tired after a long day, a pleasant, relaxed feeling pervaded the small group.

As I ate my dinner that night and thought about the day's events, I felt happy to be working in a functioning traditional culture rather than one in which the anthropologist has to get most of his data about tribal life from the reminiscences of the elders. Sometimes I asked for details of a ritual in advance of its performance; when it took place, some differences usually showed up, even though the customary pattern was followed. After one rather important ceremony, I made this point to an informant who had given me particularly full details in advance. He looked at me as if I was either stupid or naïve (or both), and asked if I didn't know that nothing ever took place *exactly* as it was supposed to. Nor can generalizations by Melanesians, or anyone else, include all the specific details of actual happenings.

Although it would not have been considered appropriate for me to be with the men in their casual social life as I was with the women, it was understood that I must observe and learn about men's economic and ritual life, which was apart from the women's. I worked on my veranda with individual men as informants and as teachers of the native language. I did not go to men's feasts and other rites unless I was invited, but I was invited to everything. When the men were having a feast, usually in the cemetery, two elders called for me and escorted me there. I sat on one side, apart from the men, and munched a banana as I recorded what was said or done. Later I went over the notes with one or two of the men who had been present. One morning, although I arose very early – before dawn – to go to a men's feast in a neighboring village, I was still eating my breakfast when the Lesu men were ready to leave. Without saying anything to me, they detailed one man to wait. The sun was just rising when he and I started our walk to Ambwa, about three miles away.

I had to go to all feasts for the same reason that my presence was required at every mortuary rite. But no law of diminishing returns operated for the feasts, since the events they celebrated were so varied: birth, bestowal of name on a newborn baby, appearance of his first tooth, early betrothal, first menstruation, initiation of boys, marriage, completion of any work, death, and practically every other event – small or big – in life. Through these feasts I gained not only a knowledge of the normal round of life, but I saw also the economic system functioning. It was evident from the speeches and from my informants that the exchange of bundles of baked food was by no means casual, but followed a rather rigid system of reciprocal gifts. I heard praise for those who were generous and gossip about those who were slow to make gifts in return for those taken. As I typed my notes and mulled over them, I thought that perhaps I would organize all my data around feasts. An article on them, rather than a book, did eventually emerge after I left the field. There, I found it difficult to think in terms of articles, because I was always trying to relate one aspect of the culture to everything else.

I was more selective in going to other activities. I could not afford the time to go regularly with the women to their gardens. They left in the early morning and did not return until midafternoon. But I did go often enough to observe the various stages of their work. Fortunately, there was no feeling that my presence was necessary at all gardening activities. It was the same situation with the men's fishing. I waded into the shallow water by the beach and watched the different forms of fishing: with spears, nets, traps, and hooks. But I was content with seeing each kind of operation once or twice and discussing details of it afterwards with informants. Later, I wrote down the magical spells connected with gardening and with fishing.

I was lucky that during my stay the initiation rites for eight boys from Lesu and from two neighboring villages took place. (They did not occur every year, but only when the number of boys reaching puberty was sufficient to justify the elaborate rites.) Less than a month after I settled in Lesu, the women began practicing a dance each evening. An elderly man was teaching them, but it was taboo for other men to see the rehearsals, although they would see the dance at the ritual.

I sat watching the women practice. The moon was new and delicate, the sea dark and noisy, and the singing women moved in a circle around a fire with slow dancing steps. They asked me to join them, but I was too self-conscious. I sat watching and held one of the babies. But each night, as the music and formal steps became more and more monotonous, lasting until midnight, I became increasingly bored. I had to force myself to stay awake.

Finally, one evening, I gathered my courage and began dancing. My place in the circle was between Pulong and an important old woman in her clan. The old women danced with more vivacity than the young ones; the gayest woman of the village was a grandmother. Good-natured laughter greeted my mistakes, which were carefully corrected. The steps were not difficult and I soon caught on to them. No longer were the evenings monotonous. Every night, as the moon became fuller, I danced. But, somehow, I did not think ahead to the night of the big rites.

Finally it came. That morning, Pulong and several other women came over and presented me with a shell arm band and a *kepkep*, a tortoise-shell breast ornament, and asked that I wear their favorite dress – a pink and white striped cotton. I gulped, and said I was not going to dance; I would just observe. But why, they asked in astonishment, had I been practicing every night? I could not explain that I had started because I was bored and that now I felt too self-conscious to participate in rites which I knew would be attended by thousands of natives from all over the island and nearby islands. Soon I saw that I had to dance. A refusal now would be a rejection.

All day there was an increasing "before the ball" excitement. Hair was colored with bright dyes and bodies of men were ornamented. The tinsel trimming which I had brought with me was in demand by the women dancers, who wound it around their hair. At sunset my dancing companions assembled at my house and we walked together up to the far end of the village. Like the other women, I had a yellow flower behind my ear; the *kepkep* strung on a piece of vine hung from my neck; shell bracelets were on my left arm just above the elbow. I wore the pink and white striped dress. I was excited and nervous.

When we arrived, about two thousand Melanesians from all over New Ireland and neighboring islands were sitting around the fires. We took our places and watched the dances, which went on continuously all night long in an open clearing before an intently absorbed audience. Most of the men's dances were dramatic, often acting out a story such as the killing of a crocodile. Masks were elaborate; dancing was strenuous and the drums beat vigorously. The women wore no masks and their dances, which alternated with the men's, were far less elaborate and formed abstract patterns of lines, squares, and circles. Young men held burning torches near the dancers, so that all could see them.

I was unable to pay much attention. Consumed with self-consciousness, I imagined my family and friends sitting in the background and muttering in disapproving tone, "Hortense, dancing with the savages!" How could I get up before all these people of the Stone Age and dance with them? I prayed for an earthquake – the island was volcanic. But the earth was still, and all too soon it was our turn to dance. I wondered if I would not collapse on my way to the open clearing which served as the stage. But there I was in my proper place in the circle; the drums began; I danced. Something happened. I forgot myself and was one with the dancers. Under the full moon and for the brief time of the dance, I ceased to be an anthropologist from a modern society. I danced. When it was over I realized that, for this short period, I had been emotionally part of the rite. Then out came my notebook.

In the early morning the boys were circumcised inside the enclosure. I was invited to watch the operation, but decided not to. Nor had I gone into the enclosure during the preceding few weeks when the boys were confined there. The normal social separation between the men and women was intensified during the initiation of the boys. The mothers (real and classificatory) openly expressed their sorrow at losing their sons who would now join the adult men. The women wept (not ritual wailing) when the boys went into the enclosure for the operation, and began a dance which expressed their feelings. Men ran out from the enclosure and engaged in a spirited fight with the women. They threw stones and coconut shells at each other and exchanged jeering talk. Since I had been identified with the women, even to the extent of dancing with them, it seemed unwise in the hostile atmosphere between the sexes to swerve suddenly from the women's group to the men's. Or, perhaps, I was unable to switch my identifications so quickly.

Later in the morning the piles of food (contributed by the clans of boys who had been circumcised) were given to the dancers. A particularly large pile was put in front of me and a speech was made praising my dancing and expressing appreciation. From then on the quality of my relationships with the women was different. I had their confidence as I had not had it before. They came of their own accord to visit me and talked intimately about their lives. I secured eight quite long detailed life histories. My relationships with the men were also subtly strengthened. The formal escort to their feasts continued as before, but there was a greater sense of ease between us and they gave me freely any data I asked for. I was glad for many reasons that I had not given in to my self-consciousness. Thinking about it, I was amused to realize that all the things white people had tried to make me fear – snakes, sharks, crocodiles, rape – had not caused me anxiety. Nor had the expedition taken any particular courage. As one of my friends remarked, I had the courage of a fool who did not know what she was getting into. But to dance with the women at the initiation rites – that had taken courage.

A woman alone in the field has certain advantages. Social separation between the sexes is strict in all tribal societies. Male anthropologists say it is difficult for them to be alone with native women, because the men (and the women, too) suspect their intentions. When traders and other white men have had contact with native peoples, they frequently have had sexual relations with the women, with or without their consent. No precedent existed, at least in Melanesia when I was there, for a white woman to live alone in a native village. We could establish our own patterns, and obviously these large strong Melanesians could not be afraid of me. (The gun which Radcliffe-Brown had insisted I take with me for protection remained hidden in the bottom of a trunk.) My relations with the women were more chummy than with the men, and data from the former were more intimate. But the men completely understood that I had to find out as much as possible about all sides of life and, very definitely, did not want the masculine side omitted. It was the men, not I, who suggested escorting me to all their feasts. My impression is that fieldwork may be a bit easier for a woman anthropologist alone than for a man alone.

Being alone for a male or female anthropologist gives a greater intensity to the whole field experience than living with company, and frequently provides more intimate data because the fieldworker is thrown upon the natives for companionship. On the other hand, it has the disadvantage of loneliness, and, perhaps, getting "fed-up" more often. A mate and children reduce the loneliness, and they may be of help in securing data from their own sex and age groups. Children are often an entering wedge into the study of family life. A team of colleagues, particularly from different disciplines, makes possible a many-sided approach to complex problems and offers the stimulation of exchange of ideas while in the field. A disadvantage of the family and team is that they may make relationships with natives more difficult. One member may be quickly accepted and the other disliked. It is usually easier for people to relate to one stranger than to several. The initial pattern in participant observation was the lone anthropologist. Today the family and team are becoming more common.

## XI "Going Native?"

Although I had enjoyed those brief moments of feeling at one with the women dancers at the initiation rites and although I was fairly involved in this Stone-Age society, I never fooled myself that I had "gone native." I participated rather freely, but remained an anthropologist.

While I did fit, to a considerable degree, within the Melanesian social system, small incidents sometimes brought out a sense of my difference. When I admired the beauty of the night, my friends looked at me as if I were quite strange. They appeared to take the scenery for granted, and I never heard them comment on its beauty. When I was trying to find out about the inheritance of personal property, I naturally asked everyone the same question: who would inherit upon death their *tsera* (native shell currency), their *kepkep* (tortoise-shell breast ornament), and other small personal possessions. My informants apparently compared notes, for after a while one man wanted to know why I asked everyone the same question. Did I not know, he asked, that there was only one custom of inheritance? His question came at the end of a long day's work and I was too tired to explain the concept of a sample. I contented myself (and presumably him) with saying that asking the same question of many persons was one of the strange customs of my people, thinking to myself of social scientists. On the other hand, it was I who thought it a strange custom when an old man told me while I was doing his genealogy that he had strangled his father and mother when both of them were old, ill, and wanting to die. I looked at the man's kindly face and wondered how he could have done it. I realized in a few minutes that his attitude might be called realistic and mine sentimental, and I did not like the latter label.

Sometimes a crisis brought out a latent conflict between the roles of being in and being out of the society. My good friend Pulong was ill and no one knew whether she would live or die. In the early morning, before daybreak, she gave birth prematurely to her baby, born dead, and her own life appeared to be in danger. All day women of her clan were in her mother's house (where she had gone when the birth pains had started), giving her native medicine, performing magic to cure, and tending her. Ongus, her husband, sat on a log outside the house. (Because the illness was connected with childbirth, he could not go into the house.) He sat, with his back to the house, his shoulders bowed – a lonely, tragic figure. His large, beautifully proportioned body and its attitude of tragedy made me think of a Rodin statue and of one of Michaelangelo's figures on the ceiling of the Sistine Chapel.

Psychologically, I was not at ease. I walked in and out of the house where Pulong lay, but I could do nothing. Obviously I knew too little of medicine to administer anything from my kit. Pulong was my best friend among the women and a very good informant; personal sorrow was mingled with fear of scientific loss. My sense of helplessness was difficult to take. Inanely I remarked to Ongus that I hoped Pulong would be better soon; he replied gravely that he did not know. Even before he answered, I knew my remark was silly. I sat around the bed with the women, went back to my house, wrote up everything, wandered back to Pulong's bed again. The fact that I was getting good data did not take away my restlessness. I felt all wrong during this crisis: outside it, though emotionally involved.

Living in a culture not my own suddenly seemed unnatural. It was as if the group had withdrawn into itself, and I was left outside. Pulong recovered; the normal daily life was resumed and I lost this feeling. But during Pulong's illness and in similar emergencies, I knew that no matter how intimate and friendly I was with the natives, I was never truly a part of their lives.

Another crisis occurred when I was in Logagon for *Malanggan* rites. On the afternoon of the second day there, my Lesu friends, a large number of whom were at Logagon, were extremely cool to me; they were polite, but the easy friendliness had disappeared. I was puzzled and concerned. I tried to think of something I might have done to offend them, but could recall nothing. Then the rites began and, as usual, I took notes; but I worried about what had caused the strange unfriendliness. That night, Kuserek told me the reason. The Lesu people felt that I had betrayed their confidence by reporting a case of sickness to a government medical patrol.

Earlier that day, two Lesu men, arriving for the rites, had stopped at my house to chat. Giving me news from Lesu, they mentioned that Mimis, a young girl, had been afflicted with the "big sickness." An epidemic causing the death or paralysis of natives had begun to sweep the island. The people called it the "big sickness."[1] Seven people had died of it in a village six miles from Lesu. Before I left for Logagon there were no cases in Lesu. Naturally, I was worried about its spread to Lesu. While I wondered what to do, a medical patrol from Kavieng drove down the road in a lorry. I dashed out of the house, yelling at them to stop. Finally they heard me and I told them about the sick girl in Lesu. They said they were going all the way to the southern end of the island, and would stop in Lesu on their way back and take the girl to the Kavieng hospital, if she were still alive. I walked back to the house, relieved that I had been able to report the sick girl to a medical patrol.

But my Lesu friends had a very different impression of this incident, which they had witnessed. For them, the Kavieng hospital was only a place in which death occurred, and if they were going to die, they preferred to do so in their own home. (Ill people were so far advanced in their sickness when they entered the hospital that they usually did die there.) By reporting the sick girl to the patrol, I was sending her to die in Kavieng, far from relatives.

Stunned by this version of the affair, I immediately went over to where a group of Lesu men, many of them trusted friends and informants, were sitting, and began to explain. They listened politely, said nothing, and turned the conversation to a different topic. Feeling helpless, I decided to let the matter drop for a few days. Everyone was busy with the rites, and I went about taking notes, wondering when I should try to reopen the subject. Then one evening when I was with Ongus and a few people whom I thought had trusted me, I began again. I gave the facts simply and clearly: the horrors of the "big sickness" and the seven deaths in a nearby village, all of which they knew. I said that I was concerned that the people of Lesu did not die in the same manner; that I did not know whether Mimis would live or die in the Kavieng hospital, but her absence from the village would at least prevent other Lesu people from catching the "big sickness" from her. I explained as well as I could the meaning of contagion. All this I repeated several times. The men listened and apparently understood the general point. The former friendliness returned.

The climax came several months later. Mimis returned from the Kavieng hospital, well, except for a slight limp. At a small feast to honor her homecoming I smiled inwardly when speeches were made stressing that I was responsible for her being alive.

An incident which might have been interpreted by outsiders to mean that the Lesu people thought I was one of them occurred when I was feeling a bit indisposed and tired, although not sick. I decided to take the day off. Kuserek dragged my cot out to the veranda and I lay on it, alternating reading with sleeping. In the late afternoon, Ongus came over and said that he was very sorry to hear of my illness and my approaching death. Startled, I hastily told him that I was not going to die. But he insisted the end was near. The cry of a certain bird, believed to be an omen of death, had been heard in the village, and the meaning was evident. Ongus then assured me that I did not have to worry. Since my own relatives were far away, "my" clan would take the responsibility for providing the proper mortuary rites. I got up immediately and walked about the village so that everyone could see that I was decidedly alive, and not begin preparation for my mortuary ritual. The attitudes of Ongus and of "my" clan did not mean that they regarded me as one of them, but rather that to them it was unthinkable for any human being not to have the proper rites after death. My new friends liked me, knew that my real relatives were far away, and therefore planned to do the "honors."

It never occurred to me or to the Melanesians that I was going "native." Understanding between people is, usually, a two-way process. I had told my Melanesian friends something about my family and explained my customs as those of my society; and they knew the function of my living in Lesu – to understand their society. Beyond our differences in culture, they seemed to perceive and understand me as a human being in much the same way that I perceived them as human beings. Preliterate people are usually able to size up an anthropologist as a person, understand his role, and become his friends if they like him, though his skin is of a different color and he comes from a strange culture. Likewise, they quickly spot the phoniness of an anthropologist who thinks or pretends that he is really one of them.

Long after I left Lesu, I was told about a couple – both anthropologists – who had worked among an Indian tribe in South America. The wife decided she wanted to "go native" and left her husband for a short time to live with an Indian family. She slept in their house, dressed as they did, ate the same food, and, in general, tried to live the native life. At the end of an agreed-upon period, her husband

and his colleague called for her. As she climbed into their truck with a sigh of relief, her Indian host winked broadly at her husband. He and his family had been humoring her play-acting.

[. . .]

## XVIII White Society

My life was not segregated, but it was compartmentalized into Negro and white spheres. Living in a white boarding house gave me a base in the white community. My fellow boarders and I ate three meals a day together, and in addition to good food, I absorbed local news and gossip and was continuously aware of the behavior and attitudes of a half-dozen white people. On the other hand, I paid the price of being unable to get away from the research.

The boarders and my landlady seemed to accept me even though they knew I broke the social title taboo. Mrs Wilson phoned occasionally, usually at meal time, since she knew I was apt to be home then. The phone was in the hall and conversations could be heard anywhere on the first floor. Whoever answered the phone would call out to me, "Annie's on the phone." I knew Mrs Wilson had heard the "Annie" and I began, "Good morning, Mrs Wilson." I could feel the strained silence at the table in the next room. Our phone conversations were brief, making an appointment or changing the time for one. My fellow boarders had all told me that "Annie knew her place" and this belief probably kept them from being more disturbed by my calling her "Mrs."

Knowledge of the white people I obtained in different ways: systematic interviewing, considerable informal participation, and an attitude questionnaire administered toward the end of the study. My original plan had called for studying only the behavior and attitudes of white people in their relations with Negroes. But I soon realized that it was not possible to understand these relations without knowing more about the class system and the traditions and history of white society. It never occurred to me not to expand the study, within the limits of the available time.

Planters were systematically interviewed. The sample included those with "mean" and "good" reputations, solvent and insolvent; many were owners of the plantations on which I had interviewed sharecroppers and renters. Interviews with the planters were almost always by appointment; I did not write during them, but always immediately afterwards, as was my custom with Negroes. The frankness of the "mean" planters surprised me. One opened his books and, boastingly, showed how he cheated his sharecroppers. He admired his own cleverness, and was apparently so much a part of his social milieu that he was unaware of, or unconcerned about, other values. However, other planters rationalized their behavior with the commonly-held belief that a "nigger" had to be kept in debt or on a subsistence level in order to get him to work. Whatever the planter said, I maintained the "poker face" of an interviewer, and expressed only interest which was real, and an assumed naïveté.

Before coming south I had known that Negroes had no political or legal rights. But the total absence of economic rights was a minor shock. I had not before known of such a society. Tribal societies had their well-defined rules and customs of reciprocity. The Middle Ages, of which the South reminded me in some ways, had an established system of duties and responsibilities between lords and serfs. But the rules on which the sharecropping system was based were broken more often than followed.

Much of my data and understanding of the white people came from a seemingly casual social participation with the middle-aged and older people in Indianola. I did not participate in the parties of young unmarried people, because I could not take the heavy consumption of corn liquor, and going to them would also have necessitated a boy-friend, which, as already indicated, would have been disastrous to working with Negroes.

I was invited to white people's homes, and used the occasions to direct the conversation along the lines of my sociological interests. Sometimes useful information came up spontaneously. Each social visit or any encounter giving data was written up as soon as I was back in my room. Women invited me to afternoon bridge parties; since I did not know how to play bridge, I dropped in on a party now and then for sociability in the late afternoon when refreshments were being served. More rewarding for data were an afternoon visit or lunch with a woman alone, and dinner or supper with a family. When I dined at a white person's home, I usually knew the servant who waited on the table. We exchanged only a quick knowing look. I may have interviewed her, had refreshments in her home, or been with her on some other social occasion. I could trust her and she could trust me not to reveal our relationship. I was often amazed at the freedom with which my white host and hostess talked in front of their Negro servant. It was as if she did not really exist. The Negroes' awareness of the realities of white society was no accident.

White people talked freely about themselves, including their feelings and attitudes towards Negroes. Revealing also were their frank opinions about white neighbors and friends. Planters were described as "mean" or "good" to their sharecroppers; these evaluations usually agreed with those given by Negroes. The "mean," as well as the "good," planter might be an elder in the church, and this situation was regarded as normal. My host and hostess did not seem to notice, or did not care, that I said little and never expressed an opinion. They were pleased – and, perhaps, flattered – that I was genuinely interested in what they had to say.

Conversation about family background was always a source of data. Almost everyone boasted about having an ancestor who had been a Colonel in the Confederate Army, and tried to give an impression of being descended from men who had big plantations with a large number of slaves. Common sense told me that the Confederate Army must have had more privates than Colonels; reading had informed me that in the whole pre-Civil War South, not more than 25 per cent of the people had owned any slaves and the majority of these had owned but one or two. Only about 6 per cent had as many as twenty slaves. Since Mississippi was settled late, the percentages were probably even smaller in that state.

The pretenses to an aristocratic background, which could also be called fantasies or lies, were significant data on cultural values in the community. Lies frequently reveal much about the values of a society, even when the fieldworker can not check on them. The situation in Mississippi, in which it happened to be possible to distinguish between the equally important lie and fact disclosed two related points: the absence of a middle-class tradition, and the white peoples' burden in carrying a tradition that did not belong to them.

Having perceived these points in listening to fantasies about family background, I gained evidence of them in other places. For instance, the whites talked condescendingly about a planter from Ohio who was openly a middle-class farmer and without pretense to a higher social background. His late father, he said, had been a dirt farmer in Ohio. When I interviewed this midwestern immigrant to Mississippi, he was in overalls, doing manual work. This was completely atypical for a white planter. The man from Ohio owned one of the few solvent plantations in the county; he had a good reputation among the Negroes and his sharecroppers were busy all year long, rather than only during the short planting and picking seasons. He planted other crops besides cotton, and had an extensive and profitable peach orchard. As I had a cup of coffee with him and his wife at the end of the interview, he mentioned that he had little in common with his fellow planters and his wife said she had almost no contact with their wives.

White women as well as men thought all manual work beneath them. Some who were hard up during the depression and were even on Federal relief employed a Negro laundress. The traditional American middle-class tradition of the virtue of hard work and its function in "getting ahead" was absent from this Mississippi community in the mid-thirties. However, it was not beneath a white woman's dignity to run her household and be a skilled cook. Women of the pre-Civil War aristocracy had run large plantations and taught house slaves their skills. The Negroes' term "strainers," for most middle-class whites, was sociologically apt. While it is not difficult to understand why people want to pretend to a background to which culture gives a higher prestige than their own, it is almost axiomatic that this situation produces anxieties, as in cases of Jews denying their backgrounds, Negroes passing for whites, and so forth.

Conversation and behavior revealed that the white people in this middle-class community also had a heavy burden of anxiety or guilt, or both, in reference to the Negroes. No household was without its gun. The whites seemed to live in fear of the tables being turned. I was advised to carry a gun in my car as I drove alone at night "across the tracks." When I laughingly mentioned my total ignorance of firearms and said that I was more afraid of them than of people, I was considered courageous. Some women, particularly those who took their religion seriously, expressed in hesitant manner the contradictions they felt between their Christian beliefs and the accepted code of behavior towards Negroes. Their obvious guilt was, I think, somewhat allayed by my not being interested in assessing blame. I can think of no worse technique for getting data than making an informant feel guilty.

[. . .]

As I mingled with the white people, with values so different from my own, I was surprised at my tolerance and, even more, at my liking some individuals. I remembered that when I was working in the labor movement, I thought that anyone who did not believe in trade unions as the hope of society was beyond the pale of my liking or even socially recognizing. In Mississippi, I wondered if I would have been different from the other whites, if I had been born in this community and had never left it. I had learned through breaking the taboo on social titles for Negroes how difficult it was to go beyond the group uniformity and consensus. As I tried to understand the historical and social situation which had produced these white people, I occasionally wondered about my new self; anthropology had become part of my personality. But my values remained as strong as ever and the effort to understand people with an opposing code did not mean that I condoned it.

Instead, I felt compassionate. The whites seemed to be worse off psychologically in many ways than the Negroes, and I sometimes felt that if, Heaven forbid, I had to live in Mississippi, I would prefer to be a Negro. The oppressed group were sure God was on their side and sure of their eventual victory, and had a sense of satisfaction in successfully disguising their real attitudes and fooling the whites. Many of the dominant group felt either guilt or hypocrisy, or both, and fear. The initial resistance of the leading white citizens in Indianola to my study clearly indicated fear of what I might find out, and was in sharp contrast to the attitudes of Negroes who wanted me to learn the truth. Then, too, the area for reality thinking was larger among the Negroes than for the whites. The former knew well their own group and the whites, both of whom they regarded as ordinary human beings – good and bad. The white man's knowledge of Negroes was limited to opposing stereotypes: a child-like person always enjoying life or a potentially dangerous sub-human type.

However, my compassionate attitude towards whites was severely jolted late one afternoon when I ran into a crowd of about twenty-five rough-looking white men with dogs on a country road. They separated to let me drive by. I stopped for a couple of minutes and found out what I had immediately suspected. They were out to "get a nigger" who, they said, had raped a white woman in a neighboring county. He was supposed to have fled into Sunflower County. Shaken, I drove on. I knew the Negro would be lynched if caught. The would-be lynchers belonged to the poor-white group so easily distinguishable by their clothes and their red-tanned necks. Their faces, now transformed with brutality and hate, were frightening.

I could make only a pretense at eating supper when I returned to the boarding house. My fellow boarders had heard of the alleged crime, and were unconcerned about the possibility of lynching. It was too bad, they said, but after all, it was in the next county, and Negroes had to be taught a lesson once in a while, otherwise no white woman would be safe. That night I could not sleep. I felt I had to do something to prevent a possible lynching. I saw myself as a kind of Joan of Arc on a white horse. But what could I do? I knew that an appeal to police or other authorities was useless. The Negro and the white men hunting him lived in a county where I knew no one. By morning I had not thought of anything I could do to help the Negro. At breakfast I saw the local paper with a headlined story about the "sex crime."

But at least I could function as an anthropologist. The Negroes in Indianola stayed indoors and there was no need to study their attitudes. But I could study the middle-class whites who would not have participated in a lynching, although they condoned it. During the next couple of days I walked around the white section of town, sat in drug stores drinking Cokes with men and women, visited people I knew, listening all the time to what they had to say about the impending lynching. The story was almost always the same: no one believed in lynching, but what could one do with these "sex-crazed niggers." Only one person – a woman – wondered if the Negro was really guilty. She was a deeply religious middle-aged housewife whose family background was a bit higher in terms of education and wealth than the norm in Indianola.

The Negro man escaped to another state, and was not caught. The excitement died down. Whispers and rumors circulated that he had committed no crime, but had attempted to get back some money owed him by a white man, who happened at the time to be accompanied by a girl friend. The story of the Negro's attempted rape of her had been spread by this man.

I felt I had won my spurs as a fieldworker. I had interviewed, observed, and gotten data in a situation which had deeply disturbed me.

[...]

The constant participation and observation among Negroes and whites had its costs, too. Occasionally I wondered who I was, as I passed back and forth between the two groups. When I inadvertently "passed" for Negro, I would return to the boarding house and look in the mirror, wondering if the color of my skin had changed. There was always some tension in the situation for me. I never was sure that something terrible, or at least disastrous for my fieldwork, might not happen as Mr Smith had predicted. But I evidently managed fairly well; I do not remember having any even minor illness when I was in Mississippi.

[...]

Occasionally in Indianola, I was tired and depressed and wondered why I had come there. I remember one hot August night when the damp heat seemed to close in on me. It was more oppressive than the nights in Lesu, where cool sea breezes filtered through the thatched roof. I lay bathed in perspiration, although hardly making any movement. I asked myself, "Why do I have to suffer this heat?" The next day as I was getting some interesting data, I had the answer: I enjoyed fieldwork more than any other. It was a form of experiencing life, of stepping beyond the boundaries of my background and society and of making the latter more intelligible.

In the discussion of the Mississippi fieldwork, I have occasionally touched on some of the differences and similarities with the Lesu experience, in terms of method and techniques. Here, I would like to emphasize a few major points. The culture of Lesu was sufficiently strange and esoteric for me to be really outside of my society. Although the standards of living in Lesu were decidedly lower than in Mississippi, neither Melanesians nor I regarded this or any other aspect of their social organization as social problems – in the colloquial sense of presenting situations for which change was desirable. (Today, after the long contact with Japanese and Americans, as well as Australians, the situation might be quite different.) In Mississippi, it was impossible to escape the inherent social problems. Inevitably, I viewed the mores, behavior, and attitudes of Negroes and whites from the background of the larger American culture in which I had been reared and in terms of my personal values. Yet, I did manage to acquire a considerable social distance from the Mississippi community. Seeing it in historical perspective and as a kind of anachronism was helpful. The previous experience of being involved and detached in Lesu was invaluable. Finally, my identification with both racial groups and with the different classes in each gave me a measure of objectivity.

Of necessity, my roles in the biracial situation and power structure of the deep South were more complex than in Lesu. In the latter place, I occasionally was naïve; in Mississippi, I had sometimes to pretend to a naïveté. In Lesu, I had no anxiety about unknown dangers (once I got over my initial panic). As far as was necessary, I followed the Melanesian taboos, and when I broke them, such as going to a men's ritual feast, it was done at their invitation, with their help and with the approval of the community. In Mississippi, I had to break some taboos of the white power structure openly, and some secretly, never knowing what might happen in either case. "Carrying" two groups mutually hostile and fearful was far more difficult than the clear-cut role in Lesu. My involvement in the two situations was quite different. No matter how high the level of my empathy with the people of Lesu, I never wondered if I was a Melanesian or a Caucasian, a member of the stone-age society or of my own. As already mentioned, in Mississippi, there were times, when passing back and forth between the two groups, being identified with each and occasionally mistaken for a Negro, that I wondered what group I really belonged to. Psychologically, I did belong to both, which in some cultures other than Mississippi would create no problems.

For all the differences between Lesu and Mississippi, many similarities existed. I was not only accepted in each place, but my presence and the study eventually taken for granted. In both the culture seeped into my bones, as I went over and over again to Melanesian feasts and to Mississippi churches, participated in the daily lives of Melanesians and Negroes, interviewed many people, and became close friends with a few.

5

# Sex, Color, and Rites of Passage in Ethnographic Research

*Norris Brock Johnson*

Arnold van Gennep remains an important figure primarily because of his seminal approach to defining the relationship of ritual behavior to the dynamics of both individual and group life.[1] Van Gennep (1960) suggests that the life of any individual or group ought to be conceptualized as a series of transitions, deriving clarity and meaning through their dramatic expression in ritual ceremony. The ritual ceremonies marking off periods of transition from one stage or aspect of life to another are composed of sequenced events and activities collectively termed *rites de passage*. In discussing rites of passage in a variety of cultures, van Gennep delineates the now-familiar typological distinction between *preliminal* rites of separation, *liminal* rites of transition, and *postliminal* rites of incorporation.

Van Gennep's notion of *rite de passage* is useful to our detecting and understanding the latent patterning of the experiences we may encounter during the process of ethnographic research.[2] On one level ethnographic research itself is a series of rite-of-passage transitions. The researcher is first separated, often psychologically as well as geographically, from his or her native culture. Existing in a liminal state, separated from his own culture yet not incorporated into the host culture, the ethnographer becomes what van Gennep, and more recently Agar (1980), would term a stranger. According to van Gennep (1960:28), the way out of liminal stages takes on an invariant pattern and sequence:

> The length and intricacy of each stage through which foreigners and natives move toward each other varies with different people. The basic procedure is always the same, however, for a company or an individual: they must stop, wait, go through a transitional period, enter and be incorporated.

Just being deposited on shore and pitching a tent, as did Malinowski, does not mean that one will gain meaningful access to the group under study. An ethnographer's degree of access to another culture often is associated with his degree of incorporation into the group or subgroup under study. Until being granted appropriate rite-of-passage experiences, the field researcher might remain in that terrible liminal stranger state with which most ethnographers are familiar. And this is of consequence, as there is another level on which to consider ethnographic research as rite of passage.

Norris Brock Johnson, "Sex, Color, and Rites of Passage in Ethnographic Research," pp. 108–20 from *Human Organization* 43:2 (1984). Copyright © by the Society for Applied Anthropology.

In the Boasian tradition, becoming a cultural anthropologist requires successfully "passing" a ritual sequence of research experiences as a precondition of professional status and role. Through these often dramatic experiences, the novitiate is transformed into a new being – a cultural anthropologist.[3] Much has been written, often in anecdotal fashion, about the transformation effect of the experience of ethnographic research. The analogy, though, has been treated superficially. The rite-of-passage *form* of these transformational experiences has been little explored. To be valid, the rite-of-passage analogy ought also to be thought of in terms of the invariate *sequence* of events one experiences in the process of status/role transformation. More attention needs to be directed to the structure and sequence of ethnographic experiences as part of the required passage into anthropologydom.

This article illustrates the useful application of van Gennep's typological concern with sequence and form to our understanding of the nature of ethnographic research. Apart from merely serving to transform one into a cultural anthropologist, what aspects of ethnographic research correspond, or do not correspond, to classic rite-of-passage theory? Conversely, can our understanding of ethnographic research add to rite-of-passage theory?

The color, sex, and host group basis for interpretations of the stranger are important factors not taken into account by van Gennep. Indeed, van Gennep assumes that wayfaring strangers invariably are white males. Van Gennep is not particularly racist or sexist; such assumptions were common in early ethnology.[4] This paper discusses the impact of color, sex, and gender expectations on liminal rites of transition by contrasting two instances of ethnographic research: my research in a rural village in the midwestern United States, and my research on a small island in the British West Indies.[5] In the West Indies I sought access to a strictly male carpentry and boatbuilding group. In the United States, I sought access to a predominately female schoolteacher host group. I found van Gennep useful for understanding the structure and meaning of some seemingly random ethnographic experiences. I came to the conclusion that rite-of-passage events and activities, the kind and degree of transition out of the liminal phase of ethnographic research, vary with respect to the color and sex of the stranger and associated expectations within host groups. I use van Gennep's sequence of ***stopping, waiting, transition,*** and ***entry*** to compare similarities and differences in the rites-of-passage events and activities leading to varying degrees of access to these two host groups.

This little-used typology better depicts the form and sequence of rite-of-passage transitions into ethnographic experiences than does the more familiar separation/transition/incorporation sequence more accurately describing rites of passage into the status and role of professional anthropologist. The stopping/waiting/transition/entry sequence is a subdivision of the liminal transition period within the separation/transition/incorporation sequence.[6]

The color and sex of the stranger, and the judgments about the stranger made by host groups, affect the character of the pattern by which each moves toward, or away from, the other. Given this, some strategies found useful for reducing the time spent in the liminal stranger stage are discussed in the conclusion. Accompanied by these color and sex considerations, van Gennep presents a useful guide to the sequencing of experiences one might expect to encounter during the process of ethnographic research.

## On the Island of Bequia (Beck-We)

The island of Bequia nestles in the northern part of the Grenadines, a lush string of 32 small islands covering the 65 km between Saint Vincent to the north and Grenada to the south, in the windward archipelago of the Lesser Antilles of the British West Indies. With a population of 4,236, Bequia is three km wide and eight km long.

During the midsummer of 1980 I arrived by cargo boat to begin a two-month pilot survey of existing boatbuilding activities on several islands in the West Indies. My large interest is in better understanding the relationship between ideas, values, and feelings, and the manner in which that ideological realm is given existence, and permanence, through material objects and artifacts. We human beings are affected by the environments we create for ourselves. Culture, as a human phenomenon, is something we live through as well as something we bring into being. Through our interactions with them, the things we create, buildings for example, help condition us to the values, ideas, and meanings clustered in the ideological realm of culture (cf. Bourdieu 1973; Csikszentmihalyi and Rochberg-Halton 1981; Oliver 1975). But humans inhabit maritime as well as terrestrial environments, and we have not paid as much attention to the cultural consideration of the things we create in adapting to the water as we have to the things we create in adapting to the land. My interest in boats springs from the observation that, unlike the ethnographic analysis of buildings (cf. Crowhurst-Lennard 1979; Fernandez 1977; Paul 1976), boats are assumed to be purely functional forms. Boats look the way they do, many conclude, because they function to move people and goods through the water. Many assume that, unlike a Gothic cathedral (Panofsky 1957) or a Maori *marae* (Austin 1976), maritime architecture embodies no subtle iconographic form or sophisticated symbolic structure. But boats, especially traditional wooden boats, are material artifacts permitting prolonged interaction with water and air, and anyone who has spent any time sailing has been aware of the powerful (mostly spiritual) archetypal feelings and thoughts engendered through this unique combination of elements (wood/earth; air/water). So my ongoing concern is with researching how traditional wooden boats exhibit noninstrumental form, and how boats embody culture to the extent that interactions with and experiences through these forms orientate one to ideas, values, and meanings at work in other spheres of the societies producing them.

Much research on maritime anthropology has been carried out in the Pacific basin; by comparison, seafaring traditions in the Caribbean have been neglected.[7] Through word of mouth from other sailors and through replies to letters about my project placed in sailing magazines, I found out that "the best boats in the Caribbean are built in the West Indies, and the best boats in the West Indies are built in the Grenadines." Bequia's subsistence strategy and culture have long been tied to the sea. My survey of Bequia, Carriacou, Canouan, and Petit Martinique would reveal an island on which to focus long-term ethnographic research. Further, I came to the Grenadines to find a society where traditional wooden boats are still an integral part of the culture, and to locate shipwrights (to whom I might apprentice myself in a long-term ethnographic study) still using traditional methods of boatbuilding and exhibiting little or no modern European and North American influences on design construction, technique, or symbolic meaning.

## *Stopping*

Van Gennep says that the stranger, seeking access to a host group, initially will be stopped. Upon jumping off the roof of a cargo boat onto Bequia, I found this to be true – literally. As a stranger, I arrived on Bequia at a most inopportune time. The duration of my stopping period, I feel, was influenced by my age, my sex, and my color.

There recently had been a short-lived Marxist revolution on nearby Grenada, and a short-lived armed conflict on Bequia itself. I arrived amid nighttime political rallies illuminated by flickering torchlight, amid men, silent behind their black sunglasses, wearing green fatigues and carrying glistening blue automatic weapons. The guards were from Saint Vincent, the administrative center for the Grenadines. Upon arrival I was not asked for my passport or other papers, but I was watched. At the small guest house in which I initially settled, I soon found it made little difference to introduce myself as a university professor from the United States interested only in learning about traditional boatbuilding. My feeling, though hard to prove, is that I was viewed by a variety of people as a strange *black* male asking questions. My initial attempts to make conversation with the cooks and waitresses, the fishermen on the wharfs, and the owners of the guest house all met with short replies followed by silence, so I stopped trying. About five days passed before people began volunteering information about fishing, sailing, and boats, and about the shipwrights on the island. I became nervous as the first week drew to a close. Did

I look that much like a revolutionary? The mental picture of a "revolutionary," I gathered, did not include white males or women. The armed men with the sunglasses did not appear to scan the few white tourists (cameras, shorts, smiling, always in a group) as closely as they scanned me (cut-off sweatshirt and jeans, sandals, beard, not smiling, alone, black, adult, male). I believe my stopping period would have been shorter if I had been a white male, a white female, or a black female. I was stopped until people made their decisions about my motives and intent. During this period, I remained visible and did not talk about anything except boats and sailing. But one morning at breakfast my hostess casually mentioned that there were two men with whom I ought to speak. Apparently she had decided that I was who and what I said I was, and she was willing to help me proceed with my study. There are many shipwrights on the island, she said, but these two men are "older" (in their seventies) and are "the best." I made a mental note to find out what made them "the best." This week-long stopping phase told me that shipwrights are held in an esteem high enough for them to be either consciously or unconsciously guarded by others. When I initially met with each shipwright I was not surprised that they expected my visit.

## *Waiting*

While on Bequia I divided my time more or less equally between the two shipwrights to whom I had been referred. For illustrative purposes, though, I will narrate the process by which my liminal stranger phase was shortened with only one of them. The man to whom I will refer as Mr Atkins is descended from the Carib Indians aboriginal to these islands. Tall, ruddy, and taut even in his seventies, he has been a shipwright all his life. He learned to build sea vessels from his father. No longer working for himself, for the last several years he has been employed in a small boatyard most recently owned by a young, white male American from California. I had previously seen the old man laboring in the boatyard, but did not realize that he was one of the persons I had traveled over 3,000 km hoping to find.

The phase of my relationship with Mr Atkins I classify as the *waiting* period of my passage lasted almost two weeks. During that time I visited daily the boatyard in which he worked. More precisely, the area in which he worked was a yard with boats being built in it. The work area borders the beach, and is sheltered by palms and other trees. A six-by-nine-meter shed is nestled among the trees. The shed is framed with wood posts and covered with corrugated aluminum panels, leaving the front wall open. Inside is a dry area for storing important woods, a place for tool storage, several large work benches, and small hollows near the walls where the men sit to take their lunch. Boats are built on the sand between the shed and the bay. Work in the boatyard started early in the morning, and I was often up and out at first light. Both directly with words and indirectly with side glances and studied reserve, Mr Atkins told me he wanted to know *why* I had come to see him. I felt him testing me. Could he trust and respect me? Did I trust and respect him? Our conversations kept coming back to boats. It seemed important for him to know how much I knew about boats. I gradually came to feel he was concerned with what kind of "man" I was; with whether or not I was to be respected, by his unfolding criteria, as a "man."

I came to this conclusion indirectly. We did not actually talk about what makes some males "men" and others not. I saw that shipwrights were held in high esteem by other men, as well as by women. Women proudly told me that a male relative "used to build boats." The woman in charge of my guest house had an uncle who was a captain on one of the cargo boats running between the islands. Rather than talking about themselves and their occupations, other men talked to me about the shipwrights they knew. In almost every seafaring culture, the making and sailing of water craft are confined to males in general and to high status males in particular (cf. Gladwin 1970; Lewis 1978; Malinowski 1922; Procope 1955; Wilbert 1976). I found this also to be the case on Bequia. *Males* work with tools to build vessels to carry people and goods across the sea. *Males* manipulate natural elements and bring them under human control. So quite consciously I sought to exhibit my passing acquaintance with knowledge I knew traditionally was privy to high status shipwrights on the island. I felt that making a reputation, as Wilson (1973) terms it, as a man-of-the-sea would make my relationship with Mr Atkins more intimate and aid my entry into this group of people.

For one thing, being a high-status male on Bequia means not only knowing about ships, the sea, and sailing, but it also means being expected to know about hand-and-eye. Those males reading this article who are not familiar with hand-and-eye would not be seen as very masculine by the male shipwright subgroup on the island of Bequia. The analogy here is with being male in the United States and not being employed – one is simply not a "man." I initially came to know about "hand and eye" quite by accident, but it proved to be a key to my gaining deeper access to the shipwright subgroup. I take pains to describe hand-and-eye so as to convey what only a few males on Bequia traditionally came to understand and so in becoming a master shipwright and, so to speak, a man among men.

If you passed by Mr Atkins working in the boatyard on Bequia, you probably would not be aware that his is the most important construction task in the yard. For the last ten years or so, Mr Atkins's work has involved carving a series of curved ribs of various lengths from cedar tree limbs to be used as the framework for boats built in the yard. This work is termed "timbering" and is the most prestigious task in the yard. Only Mr Atkins carves timbers. The other men in the yard told me that if the timbers (the frames) are not "right," the "form" of the boat as a whole will not be "right." I remember them looking at me as a not-very-significant person for not knowing this. Then one day while he was working, I bluntly told Mr Atkins I knew he was building by hand-and-eye. He looked up from his work. I felt he was really looking at me as a person of significance (as a "man") for the first time. I felt respect. The significance, the recognition given by Mr Atkins, stemmed from this stranger knowing about hand-and-eye.

Building by "eye" means that no elaborate plans or drawings are used in the design of a boat. In the contemporary United States, for example, the frames ("timbers") of wooden boats customarily are built from a series of full-size drawings in the case of smaller boats, and scale-reduced drawings in the case of boats larger than about 15 meters. Mr Atkins had to learn to build boats not as they preliminarily existed on paper, but as they preliminarily existed in his mind's "eye." Mr Atkins is both designer and builder. This conceptual feat separated, so to speak, the men from the males. Getting form "right" involves laying out the keel and then putting up a bow (front), beam (middle), and stern (end) frame. Most times, the bow and stern frames are the same. Then long, thin lengths of wood, one to a side, are placed end to end near the gunwales (top of the frames). The shipwright then moves around this five-piece frame, looking at relationships. If not satisfied, an assistant moves one of the frames slightly forward or backward, altering the shape of the stringer representing the shape of the hull. When satisfied, other stringers are added. Sometimes the trial frames are chiseled away, or new ones substituted, to get the desired "form." This goes on for several days, for longer boats, until the "right" frames are selected and their positions marked on the keel. The stringers are removed, secondary frames added, and the boat takes shape. Each and every boat, no matter the size, is built in this fashion. For several months I watched the other shipwright I interviewed building a 25-meter schooner from an initial rough sketch he had made on the sand. These men had to learn to design and build boats in terms of part-to-whole relationships rather than in terms of standardized preset plans – no mean feat. Presumably, not all the males who try can achieve it; those who do are held in high esteem. Further, boats are designed not just in terms of internal form, but also with respect to knowledge of local sea conditions and the intended use of each boat. One must also think of the influence of the environment in which each boat will function.

Building by "hand" means that no electrical power tools or heavy mechanical equipment are used in the construction of boats. At the time of my visit, most of Petit Martinique did not have electricity. The power generating plant on Bequia is a recent acquisition, and was the focus of a sabotage effort by nationalistic guerillas from Grenada. When used in remote areas, electrical power is provided by gasoline generators. Still, fairly large boats are built by only a few men using an adze (or, more recently, a hatchet), several hand planes, a twist drill, a string plumb, and a two-person tree saw. My second informant is not using any power tools to build his 25-meter cargo schooner. I helped him and his two sons lift 4 x 8-meter slabs of imported worm-resistant *silver bali* planking from Guyana up onto cedar frames as thick as my waist.[8] All the hundreds of holes for the planking bolts (wooden in more traditional vessels) are drilled by hand. In part, the physical strength involved accounts for boatbuilding as a primarily male activity. Being able to build boats by hand-and-eye in the West Indies is the achievement of only a few high status males.

During this waiting period I remember feeling quite like a child trying to get a father's attention; one male seeking the approval of a higher ranking male. I knew I was in a waiting period, and so I consciously tried to show the shipwrights and other men in the boatyard that I too knew something about carpentry, tools, boats, and sailing. I too knew about hand-and-eye, a traditional system that has all but died out. I wanted Mr Atkins to see that, though a stranger, in more vital respects I was much like him. I wanted him to know I loved and respected the knowledge and skills he knew, loved, and respected. So over the ensuing weeks I sought to convey that I had come to Bequia not just to know these things, but to know *more* about these things. I knew about building by eye, but I did not understand how one could just look at something as complicated as a boat's hull and see that it was "right." What was "right" and what was "wrong" about a hull's form? How does one come to know and remember these things? I felt I was worthy to know more because of the effort I had made, on my own, to acquire partial knowledge. I wanted Mr Atkins to see me, by local standards, as a man among men. Otherwise, I could not proceed, as the knowledge I sought was too intricately connected with gender expectations.

But all of this is quite circumstantial. In another setting, if the information I sought were linked to expectations emphasizing males efficiently consuming large amounts of alcohol, I would most definitely flunk *any* rites of passage extended to me! But on Bequia my initial display of knowledge about, skill at, and interest in boatbuilding and sailing were instrumental in commanding trust and respect from among the shipwrights. I feel it was for these reasons alone that, over the ensuing weeks of the waiting period, I was subtly invited to enter into a more intimate relationship with Mr Atkins.

## *Transition*

I clearly recall the key event terminating the liminal stranger phase of my passage. One morning I went down to the small boatyard at the end of Port Elizabeth, the main village on Bequia, to watch Mr Atkins work on timbering. Day in and day out, his main task was to use a hatchet and adze to shape tree limbs into timbers. I had been interviewing him about traditional boatbuilding techniques. On the beach in front of the yard, tied to a palm tree gracefully bent out over the water, were two very lovely 5-meter open-deck sailing boats. These craft are made in the yard and are used by men to hunt whales, by boys to deliver goods to yachts in the harbor, and by other males as a way to get around the island. The yard owner also makes these boats to sell to tourists. I never saw a female sailing one of these craft. Mr Atkins used these boats to point out to me traditional building materials and techniques. I made a series of drawings of the craft and spent a considerable amount of time trying to understand the intricacies of their hand-and-eye method of construction. The boats are constructed like fine furniture. I could find no nails anywhere in the boat; water pins, what we call dowel rods, are used to hold the craft together. The cedar and spruce pine decking is sealed with varnish and hand polished to a mirror shine. My admiration of these small craft, shaped and painted green like open pea-pods, is never ending.

That morning Mr Atkins asked if I could sail them. I immediately understood the test, and understood the compliment in being asked if I wanted to be tested. I was being invited to make a transition. Not all the males on Bequia are so invited. I was scared! These small boats were like nothing I had previously seen or sailed. What if I swamped the boat and failed? And what did "fail" mean? I knew that if I did not sail the boat I would be as one of those hollow men of which T. S. Eliot spoke; "all show and no go," so to speak. "Men" sailed boats as well as built them. If I failed to sail the boat, in their eyes I would be seen as knowing something about carpentry, but not as being a more fully competent "man of the sea" like them. But I wasn't trying to be one of them![9] I was just trying to get them to respect me enough to share deeper levels of knowledge about boatbuilding. But the rules I was learning said that this is a "man's" world. One does not cast pearls before swine. If I failed I knew I would go back to the waiting stage, or I might be stopped again. I felt excitement at the thought of sailing a craft I admired greatly. I also knew that Mr Atkins knew this about me. These men, these shipwrights, deeply love boats. Perhaps this was the other reason I had been invited to sail. This last thought nurtured confidence. I knew Mr Atkins was giving me, in Mauss' (1967) sense of the term, a gift – an invitation to share knowledge and experience and to form a deeper bond. Van Gennep (1960:29) mentions offerings and gift-giving as acts whereby host groups extend themselves to strangers. Among this male shipwright group, a valued offering and gift was the invitation to sail a boat they had made.

I could not ask how to sail the boat. During my stay on Bequia I never saw one man ask another man anything about boats. Men discussed boats as equals. But I did ask for help in pushing the 275 kgm boat into the water! I headed upwind . . . and immediately almost capsized the boat. The boat is comparatively light, and does not have a long, deep, stabilizing keel. The slightest breeze wildly tipped us as I wobbled out to Admiralty Bay. I felt I could hear the men in the yard laughing, but laughing with me rather than at me. I used my weight to counter the heeling of the boat.

It is important to note that every part of the boat is organic and made by men. The sheets (ropes used to control the cotton duck sails) are woven (by men) on the island. Sails are made (by men) down the street from the guest house in which I stayed. The mast is polished bamboo gathered (by men) from upland parts of the island. I knew the men who built this boat think of themselves, and are thought of, as a class apart. I was initially not on Bequia long enough to note the actual hierarchy in which these men rank themselves, but I do know that their male status hierarchy corresponds to the part of the boat each built. The keel and frames are considered the most important parts of the boat; they are made by the most important shipwright. Sails are important and sailmakers are high status males. The making of shrouds, the making and polishing of deck parts, and painting the hull are lower status activities, as these pieces are not considered central to what a boat is. The "boat" is considered to be the keel, frame, and hull, "finished off" by these secondary activities. In a subtle manner the physical form of a boat concretizes male status relationships, and links material culture to ideology and social organization.

But I was getting the feel of the boat. Growing confident and wanting to show off for the men in the yard (I later learned they had gone back to work and were not watching me), I wove in and out of the tourist yachts parked in the water like fiberglass space ships. Easing out the sails, I turned away from the wind and let it blow me back to the yard. I let down the sails and coasted onto the beach. Seeing me, several of the yard men stopped work to help pull me in. They told me I had been out for about 20 minutes. It seemed like half a day. Everybody laughed and kidded me about the way I had wobbled out to sea. Several men patted me on the back; I was making contact, and I felt great! Mr Atkins did not stop his work, but just looked up at me and gently smiled. The smile said I had passed. I would take the boat out again at various times, but that first time was a definite transition.[10] After that event, I noticed several important changes in the behavior of Mr Atkins and the other men toward me.

## *Entry*

Mr Atkins began asking me to help him around the yard. I was not permitted to work on the boats themselves, but I was periodically asked to help rough out some of the cedar limbs and tree trunks used in making the frames. In carrying and fetching things, I was becoming an apprentice. Mr Atkins told me that these are initial tasks traditionally done by "boys" seeking to learn boatbuilding. Mr Atkins had done these things; he said it was not something of which I ought to be ashamed. The other men knew that it was only something one had to do in the beginning. More importantly, I was permitted to handle tools such as the adze. Not the personal tools owned by each man and carefully wrapped in cloth and taken home at the end of each day, but the more crude yard tools kept in the storage shed. We did not talk much about tools, but I observed that the possession of one's own tools is respected among these men. The gender expectation is definitely for a "man" to own his own tools and be able to use them. In other seafaring cultures the possession and handling of tools such as these are primarily male activities (cf. Beck 1973:9). Being permitted access to the materials used in building boats, as well as being permitted to handle the tools associated with boatbuilding, I interpreted as further gifts being extended to me. I felt I was beginning to be incorporated as best a stranger could. After initially demonstrating basic knowledge and skill, I was told more about "hand-and-eye" principles. In talking to Mr Atkins about the traditional apprentice system, I was also aware that a more complete process of incorporation would involve being permitted to work on the boats themselves.

Other significant changes signaled the occurrence of a postliminal transition. The men began sharing more of their feelings with me. Sitting under the palm trees eating lunch, we talked not only about boats but about women and sex as well. It seems one must be interested in women as well as in boats. They began asking if I had "found any woman yet." They suggested women I might "call upon." Van Gennep (1960:34–5) considers the offering of women to strangers a prevalent gift (see also Pelto 1970:226–7).

Social relations between males and females are polarized. On Bequia task divisions by sex, for example, are apparent. During my periodic walks to the other side of the island to interview the second shipwright, I continually saw females, both women and young girls, out in the fields. Females are responsible for tending the fruit trees and garden peas grown both for commercial sale and for home use. While taking my meals at the guest house, I saw only females in the kitchen area preparing and serving food. During the day females walk back and forth from open-air markets carrying foodstuffs and household goods. During my stay on Bequia I never saw a male carrying out these activities, and I never saw a female on a boat or in the boatyard except in the evenings when women came by, silently, to gather cedar chips for firewood.

After work, the men in the boatyard often invited me to sit with them. They often talked about the "old days" when they were young men building large schooners by hand-and-eye. They talked about what life was like on the island during the 1940s and 1950s, before the influx of tourists. They do not respect most of the North American Anglo males they encounter who want boats built in a hurry, who are not that concerned about "good" work, who are not able to recognize "good" work in a boat, and do not know, themselves, how to bring it into being. It is for this reason that they continue hiring local builders. I had felt anger about a white male less than half Mr Atkins' age owning the boatyard, although he knew less about boatbuilding than the resident men working for him. In such situations, there is some advantage to being a black male. Though I spent considerable interview time with white residents on Bequia, I spent most of my time with black residents. Though a stranger, I felt more accepted by blacks than by whites. I met and interacted with many white tourists who tended to sometimes behave toward me as they did to the black male residents of the island. No matter how much I dropped references about my schooling, my present position and work, I was not treated as simply another North American. I was not treated as an equal by people from my native culture as much as I was by my host group. Other blacks on Bequia stopped paying that much attention to me after continually seeing me in the company of the shipwrights. Later, I would be told that I began looking, to them, like any other male from any one of the islands (which was the problem in the first place).

No apprentices work with Mr Atkins. Few young men desire to learn boatbuilding. I was told they want to go to Barbados and Trinidad "where the money is." The men shared their anger about the "whites from the North buying up everything around here." Jobs are scarce. Most of the men hold part-time carpentry jobs. The tradition of hand-and-eye boatbuilding in the Grenadines is dying. They told me they were grateful I came to begin recording how it is done, so that people will know.

The lesson from Bequia is that passage into the shipwright subgroup, though exclusively associated with males, involves more than simply being male. The shipwrights on Bequia, as well as the women with whom I spoke, do not see all males as equal. The males connected to boats and the sea are respected more and held in higher esteem than those who are not so connected. For example, my closest friend on Bequia is a young man in his mid-twenties who works at odd jobs for several tourist resorts on the island. He is considerate, thoughtful, and supportive – cbaracteristics I associate with being a "man." My friend, though, continually talks about wanting to be in the (mostly British owned) merchant marine service, presumably because boats and the sea hold considerable power in defining a "real" man. Many of his male friends and relatives hold high prestige jobs on the cargo boats servicing the Caribbean. He often says that so-and-so is "in the service," much as a doting mother brags about the accomplishments of her children – accomplishments she did not achieve. Working in tourist resorts, my friend does not feel as "manly" as his male relatives.

Though linked to males, on Bequia not all the males gain access to the shipwright tradition. Being male is not enough. The gender role expectations for this subgroup stipulate that one must be a certain type of male to gain access. One must love boats, as well as love the water. One must want to do this work for its own sake. One must be good with one's hands, and with tools. I was invited to experience other activities and bodies of knowledge by displaying previous knowledge and skill, as well as through the respect I brought to the work the shipwrights respected. I was seen as a male stranger of some status through an initial display of knowledge and interest in boats and boatbuilding. For both males and females, my status and subsequent access were enhanced simply by being in the company of shipwrights. My liminal stranger phase of ethnographic research was shortened by a display of knowledge of, interest and skill in, and respect for the gender-specific expectations on the island. I feel the waiting period would have been much longer had I not displayed this knowledge and skill. I am certain that a male stranger coming to the island to research syncretist religious traditions prevalent in the area would be accorded a lower initial status by the males of Bequia. This is not the focus of the island's gender expectations. In the "old days" on Bequia when men went out to sea in small boats to hunt large whales, I suspect it must have been very difficult to be a male and be afraid of the water, clumsy with one's hands, sickened by the smell of woodpiles, and unnerved by the howl of the wind.

## In the Village of West Haven

West Haven, comprising 166 sq km and composed of 2,793 people, is a rural village in the midwestern United States. During 1974–75 I carried out ethnographic research in West Haven to study the institutions linking small, local communities to the supralocal social and cultural networks comprising large-scale industrial state-level societies such as the United States. I wanted to develop empirically Julian Steward's (1972:43–63) proposition that, associated with the development of national societies, a group of interrelated institutions cross-cutting local populations and subgroups emerges to bring about functional interdependence among those diverse sectors.[11] I researched public schooling as a mechanism for sociocultural transmission in state-level societies (Johnson 1980; 1982; 1983). It is not by accident that public schooling is presently the only national-level institution in which all are required to participate (cf. Safa 1971: 211). Through microethnographic research on preschool through twelfth-grade classroom culture and society (Johnson 1984), I found that schooling conscripts and conditions children to participate socially and culturally anywhere in the national system. But similar to initiation and rite-of-passage apparatus in other cultures, public schools are limited access institutions quite difficult for strangers to enter.

## Stopping

If you ever tried to visit one unannounced, you know that public schools, especially elementary schools, are shrouded in isolation. People, including ethnographic researchers, are literally stopped at the door. The rituals by which strangers are permitted into public schools, if they are permitted, are both obvious and stringent. In order to enter, one's business must be stated to the authorities. One's reasons for being in the school and one's credentials as parent, researcher, or salesperson (those customarily permitted entry) are verified. Whether male, female, black or white, every stranger is routinely subjected to this entry ritual.

Public schools, particularly the elementary school level on which I will focus, predominantly are female settings (cf. Dreeben 1968; Mayer 1961:3–4, 24–5). Of the 18 elementary school teachers at West Haven, only three are male. The males teach at the fifth and sixth grade level. In these so-called "upper" grades, male teachers are associated with the most prestigious, "advanced" courses. The ranking of the courses reinforces their differential status. Females teach lower status courses. In trying to gain initial access to the classroom, I was struck by the fact that males controlled entry to the school. To gain entry I first presented my petition to the school board, making a formal request to conduct observational research in the school buildings. My role, I stressed, was not evaluative; teachers and administrators, especially, are sensitive to the state evaluations to which they are continually subjected. I represented, I said, no agency other than myself. Many board members were familiar with work I had already completed on the history of the community. My request was accepted. Then I had to present my case to the (white male) superintendent and the building principals (black males). The stopping period comprised the several days it took to present, in the prescribed manner, my petition for entry. But each time I entered or left the school I had to stop by the principal's office, near the outside doors, to report my movements. Permission to enter the classrooms was granted, in part, because I was by then well known in the village and had been ascribed the role of "their" anthropologist. Few academic researchers had studied West Haven, I was told, and those few were white males or females. Remember that on Bequia I had received permission to pass on to a group of males by a female. At West Haven I received permission to pass on to a group of females by a male. In gaining access to women through men and to men through women, I learned to recognize the subtle manner in which differentiated groups are in fact interrelated.

Both on Bequia and at West Haven, I noticed an association between gender role expectations, spatial segregation by sex, and the social distribution of knowledge. On Bequia males are associated with boats and the sea; females are associated with the land and with food. The sexes are also spatially separated. At West Haven, the males in the elementary school building either are in the principal's office or in the custodian's maintenance area. Except for the male "upper grade" teachers, all the classrooms are occupied by females. The knowledge and skills stereotypically associated with either sex, as such, exhibit an obvious spatial distribution. Gaining access to sex- and gender-linked knowledge about boats on the one hand, and classroom life on the other, meant gaining access to the specific spatial areas with which that knowledge is associated.

## Waiting

The phase of my relationship with the West Haven teachers, especially the female teachers, that corresponds to van Gennep's waiting period occupied several months in some cases and several weeks in others. The process of researching classrooms ostensibly controlled by females was marked by subtle cross-sex tensions and feelings of distance I did not notice in my relationships with the male teachers. The female teachers, initially, were reserved toward me. There was little of the free banter I initially experienced with the males, especially with the black male teachers. For quite some time there were no moves by the female teachers to pass me on to more involved levels of relationship.

I was kept in van Gennep's liminal state for what I remember as a considerable amount of time, months in some instances. My feeling was that the female teachers reacted to me as a black male "stranger," just as people on Bequia reacted to me as a black male "stranger." The problem was not merely how to gain physical entry into the classroom. The problem was the significant degree to which female teachers kept me at an interactional distance. Then I noticed the importance of color, sex, and gender expectations in the elementary school itself.

The first clue to the structure of color, sex, and gender relationships within the school system involved the fact that (mostly white) male administrators not only controlled access to the education complex, but controlled access to the classrooms as well. I initially had asked the building principals, not the teachers, if I could conduct classroom observations. Before entering each classroom, I made a formal request to each teacher to do so, and all but two teachers accepted. These teachers remained unconvinced of my purely academic motives, and I believe they felt I might be "spying" for the administration or for some state evaluation agency. Before letting me into a classroom, every teacher asked me where I was from, why I was there, and what I was going to do. Their suspicion, not so much of me but of my role, was obvious. Once in the classroom my pattern was to sit, all day, in the back of the room taking notes on the events and activities I observed. After class there would be brief conversation between us, and the teacher would leave! This went on for some time. I was "in" the classroom yet "out" of more involved contact with the teachers. In comparison with my experience on Bequia, I felt powerless and frustrated. This host group of females was not extending itself to me in any appreciable way.

The defensiveness of these female teachers, both black and white, toward me was attributable, in part, to their structural position in an environment sometimes not so subtly controlled by males (both black and white). Their distance-maintaining effort toward me I interpreted as a result of their own color and sex-role position within the school. Most school building principals are white males. Most classroom teachers are female. I began noticing how the female teachers tried to guard their classroom spaces from intrusions of one sort or another. Indeed, the vast majority of intrusions into their classrooms were by males. Both black and white male custodians had free access, and routinely sauntered unannounced into the classrooms, usually while class was in session. Many times I watched the principal walk into the classroom without knocking, with an announcement, often blithely interrupting the teacher. When this occurred, teachers stood by the door between the visitor and the classroom, but to little avail. Putting myself in the teacher's shoes, I felt angry. The teacher's classroom authority was being continually undermined, her territory continually challenged. In viewing all this, what were students learning about color and sex role relationships? They were learning nonverbally that males have more authority than females. The higher status administrator is male, while the lower status teachers are female. They see, in the school at least, that male roles carry more status than female roles (i.e., the janitor can interrupt the teacher). The role and status of the female classroom teacher carry a gender expectation of subservience, and males have hegemony over females. I never saw a male enter a classroom and apologize for the interruption, or ask permission to enter. Teachers and other personnel, whether male or female, always asked permission to see the principal and to intrude upon his space.

I empathized with the expectations accompanying the role of teacher. I understood some of the subtle structural reasons why most classroom teachers are female while most building administrators are male. Discovering these gender expectations and role relationships, I made it a point not to do anything that by the broadest stretch of the imagination could be interpreted as a challenge to a teacher's authority. I assumed their gender expectation included my trying to usurp their authority just because I was male. I asked permission to enter and leave the classrooms. I asked permission to move around the classroom or to talk with students. I did not unduly argue about the many pedagogical discussions we had. My attitude intentionally was respectful rather than merely deferential.

Another strategy I tried was to disassociate myself from the other males in the elementary building: the principal and the custodians. On Bequia, I disassociated myself from females initially until I found out the gender expectations for males on the island. At West Haven, I stayed with the teachers from the time I entered school until the end of the day. I did not want to be identified-by-association with the other males in the school, but rather wanted to negate the application to me of the teacher's assumptions about these males.

More sexually-orientated gender questions emerged. The gender expectation seemed to be that males are sexually aggressive toward women, irrespective of place or situation. Female teachers were concerned with maintaining a professional role and I surmised that their gender expectation was for males to be disrespectful toward them, or at least guilty of inappropriate sexual conduct. Males were expected *not* to be appropriate with their sexuality, and thus females had to be on guard against sexual conduct potentially negating their professional status. They began asking me if I was married? Did I have children? Why wasn't my wife with me? Where in West Haven was I staying? Did I occasionally go home? They wanted to find out, I felt, if I were "safe." "Safe" meant whether, as a male, I recognized their professional status and would act appropriately. Would the relationship be professional, or would I act like a male and be inappropriate sexually? Part of their initial reserve toward me, I understood, was because of gender-related expectations about my maleness.

To get past this long waiting stage I felt I must not, on the one hand, be sexually inappropriate while, on the other hand, I must act so as to acknowledge their professional status. This might mean not being sexual in school or, especially, in the classroom. I asked myself what I would do if I wanted to be sexually inappropriate, and then did not do those things. I talked about lesson plans and pedagogical techniques and the hardships of being a classroom teacher, rather than about personal matters. Verbally and nonverbally, I tried consciously to act so as to communicate that, as a male, I would act appropriately and not inappropriately display a sexuality potentially undermining their professionalism.

These teachers are prisoners of gender expectations. The traditional stereotypic gender expectations for female teachers, I discovered, have not changed much (cf. Waller 1932:134–59). Female school teachers are expected to be sexually modest if not sexually neutral, discreet, paragons of virtue, exhibiting propriety and sedateness in manner and dress – at least while in school. The unmarried female teachers led dual lives. For example, in school they dressed moderately, yet after school, in larger towns near West Haven, I would see them in tight jeans and body shirts instead of belted, A-line dresses. Many female teachers had male "friends" living outside West Haven and on weekends they traveled to visit them. I suspect that any intimation of sexual behavior in or around the school is a potential prelude to reprimand or dismissal. But these gender expectations only applied to females. I never detected a female teacher flirting with a male student, especially in the upper grades, but I witnessed male teachers continually flirting with female students. In the lunchroom, I saw male teachers flirt with female teachers, but not vice-versa. The gender rules permitted males to be more sexual than females. Understanding the powerful gender expectations in the school made my own temporary adherence to their expected sexual behavior more palatable.

Finally, similar to my experience on Bequia, I tried to display knowledge and skill hopefully decreasing the inevitable waiting period. My strategy involved stressing the fact that I am a former public school teacher and have taught in a variety of settings. Shared knowledge and skill emphasizing these commonalities might function to negate differences of sex and accompanying gender expectations. Indirectly, I tried to emphasize that we were teachers first, male and female second. I was a teacher, not an administrator. I was one of them. I tried to communicate that, as a teacher and researcher, I was more interested in understanding the social and cultural aspects of classroom life than in trying to take advantage of them sexually. As on Bequia, I tried to display the knowledge and skill we had in common. If a teacher casually said that "... the Gates profiles were marginal" (scores on standardized reading tests falling below a statistical norm) to another teacher, I casually asked about the scores for the last series. If a teacher off-handedly mentioned her "rotating periods" I assumed she was talking about the duration of her particular classes, and mentioned that we had twelve-week periods when I was teaching. When a teacher mumbled, talking more to herself than to me, that her "contracts" (state-specified number of days per week the teacher must be in the classroom) are for five days, I spoke up and asked her about holidays and vacation time. As on Bequia, I felt they took me more seriously as I displayed knowledge and skill associated with their profession.

## *Transition*

My relationship with the female teachers changed when my perceived identity and their ascribed gender expectations began to change. Over a period of weeks with some teachers and months with others, I was viewed decreasingly as a threatening black male "stranger" potentially capable of usurping their authority, and increasingly as a former public school teacher and colleague interested only in studying school classrooms. The event I mark as transitional occurred when several teachers asked me to give short talks about anthropology to their classes. As on Bequia, the relatively short transition period in West Haven comprised a specific event or activity. I was offered a gift and made a literal transition from being a liminal stranger isolated in the back of the classroom, to being in the front of the classroom where I was momentarily acknowledged as equal to the teacher. Not all of the female teachers presented me with this gift and with those few teachers who did not, I must admit purposely being kept at the ritual, interactional distance prescribed for a stranger. But most did ask me. The rite of transition here, as on Bequia, involved movement in space. This was the first clear offering of a gift extended to me by the teachers, and I interpreted the offer as a sign of being asked to demonstrate a shared identity. I felt complimented by the fact they felt comfortable enough to turn over their classrooms to me voluntarily. During that short period of time teachers would go to the back or the side of the room, in effect handing me the gift of authority over the class, an authority they guarded stridently from other males.

## *Entry*

Those teachers who offered the gift of their classrooms became more relaxed with me. They joked with me, we laughed more, and they began confiding in me. I felt it especially meaningful when they began talking about the principal in my presence. In effect, they were saying they had made up their minds that I was not a "plant" put in the classrooms by the administration or by the state. My interviewing went more smoothly with those teachers in whose classrooms I had also given the return gift of my knowledge. I felt that by seeing me as a teacher they were able to talk to me as a colleague about the classroom processes in which I was most interested. I found myself spending more out-of-the-classroom time, mostly in the teachers' lounge, with these teachers rather than with the others.

Yet the structure of male/female relationships and gender expectations in the school, rather than in the minds of the teachers, militated against a feeling of substantial incorporation on my part. For example, I never felt comfortable spending time alone with female teachers in the classrooms. One teacher very directly told me that students "might talk." I felt their own color and gender attitudes toward me had changed somewhat, but that they remained mindful of the attitudes and expectations of others in the school. I did not feel quite as awkward when sitting with a group of teachers. Further, about half the female teachers were white. As a black male I was not surprised when my closest informant relationships were with black female teachers. As in the wider society, gender expectations, especially with respect to several of the unmarried white female teachers, monitored relationships by color as well as by sex.

I never felt incorporated into the subgroup and setting I had come to research. I did devise strategies to overcome prevalent sex and color ascriptions to the point where they did not inhibit the gathering of information, and that was comfortable and acceptable to me.

Sex and color differences between the ethnographic stranger and the host group render research difficult, but not impossible. The West Haven case suggests that, especially for black males, cross-color and cross-sex ethnographic research relationships affect the duration and character of the stages in van Gennep's sequence. I suspect that cross-sex and cross-color stranger/host relationships are marked by a long liminal phase in general, especially when the stranger is male. I agree with Powdermaker's (1966:114) observation that a female stranger (and I would add whether black or white) is less perceived as a threat, to both host males and females, than is a male stranger (whether black or white).

## Conclusions

Ethnographic research comprises rite-of-passage transitional experiences, the sequencing of which in other contexts has been put forth by Arnold van Gennep, which are necessary to be permitted access to meaningful sociocultural information. This paper illustrates the manner in which rite-of-passage events and activities, in particular the kind and degree of transition out of the initial liminal waiting stage of ethnographic research, are affected by the color and sex of the stranger as well as the host group. This is an important addendum to van Gennep's typology. Sex and color differences between the host group and the stranger affect the flow of van Gennep's otherwise orderly scheme. In light of this several observations and counter-strategies, as deduced from my research experiences, are suggested.

On Bequia as well as in West Haven the demonstration and display of appropriate knowledge and skill, valued by the host group, proved a particularly effective means of negating the effects of color and sex impediments to the collection of ethnographic information. The strategy heightens a sense of shared identity by emphasizing shared competencies. Shared knowledge and skill mitigate the possible boundaries of color and sex enough to permit effective information gathering. The appropriate display of knowledge and skill effectively reduces the length of time spent in the liminal stranger phase of ethnographic research.

Classic rite-of-passage socialization transitions are non-negotiable with respect to required demonstration of knowledge and skill. In passing from the status of child to adult, youth must succeed in tasks they themselves do not completely understand and customarily have no part in defining (cf. Eliade 1958). In attempting to access another sociocultural system, ethnographic researchers do become as little children. But unlike, say, a Hopi child going through a *Ka-china* initiation in a *kiva*, we know, or at least I am suggesting we can know, that there is a discernible structure and sequence to the sometimes prolonged and often scary ethnographic experiences we encounter. Much like classic initiations, ethnographic research involves non-negotiated tests for greater permitted access to sociocultural information. Host groups possess standards to which strangers must adhere before being permitted access to deep knowledge. The French ethnologist Marcel Griaule spent 23 years among the Dogon of Mali before being permitted 23 days of intimate conversation about Dogon sacred knowledge with the wise hunter Ogotemmeli (Griaule 1965:xi). A long waiting period to be sure, but a waiting period nonetheless. It was Griaule's appropriate display of what he already knew about Dogon metaphysics that gained him access to deeper knowledge. Negotiating the passage into knowledge of another sociocultural system is not merely waiting until people decide if you are who you say you are ("... only a cultural anthropologist"). That might only mean they will decide to talk to you and not kill you. Deep access to another sociocultural system depends on people feeling that you share something with them as a fellow human being. At some point the stranger must demonstrate an interest in, and love for, the culture and people in themselves, rather than as the object of ethnographic study. One must give up some of the observer/researcher role and *do* something well that is valued by the culture. Our presumed "self/other" split must be negotiated. I think this interest/love/demonstration test is non-negotiable, and, like any rite of passage, to the person experiencing it the outcome is uncertain. Passage out of the waiting stage is not guaranteed, but knowing that color and sex have their effects, and knowing that one will be stopped and linger in a waiting stage, forces the creative exploration of ways to make this invariable sequence move smoothly.

A second conclusion is that finding a subgroup cadre of people, or choosing a whole society itself, that reflects one's personal temperament renders one less a stranger by decreasing the liminal waiting stage of ethnographic research. Napoleon Chagnon (1974:162–97) liked the Yanomamo; it is clear, to me at least, that he was drawn to this stereotypically masculine culture for more than professional reasons. I think Chagnon's temperament decreased his liminal waiting period and made possible important transition invitations (such as blowing *ebene*) and other experiences. John Gwaltney's (1980) work on ordinary ("drylongso") black American life is as much informed by his temperamental alignment with this "unit of study" as it is with the acceptance of the ethnographer by this subgroup and their invitations for him to share deep knowledge. Is this subtle factor at work in the conscious/unconscious choices of people and problems we choose for ethnographic research?

Thirdly, research in West Haven and on Bequia reinforced my personal and professional commitment to participant as well as nonparticipant observation as an ethnographic information gathering strategy. Both research experiences offered the opportunity to *do* something as well as study something. Commentaries on such experiences are rarely printed in contemporary accounts of ethnographic research, and my feeling is they are not as prevalent as they once were. The notion of an "objective" observer dispassionately removed from the experiential reality of the sociocultural situation under study is a prevalent ethnographic image (Jules-Rosette 1978; Maruyama 1974). More participant forms of ethnographic research were once quite common. Ruth Bunzel (1929), for example, tells us she intentionally chose to research (female) Pueblo potters by apprenticing herself to a Pueblo potter. Using herself as the primary research instrument, Bunzel felt she could best understand the process of making Pueblo pottery by learning to make pottery acceptable to Pueblo potters. Then there is the well-known ethnographic example of Frank Cushing (Gronewold 1972). Cushing learned so much through being invited to participate in Zuni culture that he was initiated into one of their secret societies. As Cushing decided not to publish much of the deep knowledge into which he was initiated, this remains an extreme case of participant research. Zora Neale Hurston (1970) has not been adequately recognized for her contributions to folklore. A Boasian cultural anthropologist, Hurston was also a trained folklorist. Her accounts of the personal rigors of gathering folklore are as informative as are the folk materials themselves. David Lewis (1976, 1978) mixed participant and nonparticipant observation in his exciting studies of Polynesian navigational systems. Lewis enjoyed a considerable reputation as a sailor and navigator before asking Polynesian navigators to teach him their indigenous system of pathfinding by sailing his 29 ft ketch. To learn more, Lewis served as an apprentice navigator on a 60 ft double-hulled canoe sailed, by wave patterns and the stars, from Hawaii to Tahiti. In all these cases, host groups shared deep knowledge in association with the ethnographer's emphasis on participant forms of observational research.

Ethnographic research is a political act, and political considerations influence the nature of ethnographic research.[12] Traditionally, ethnographic strangers have been white males, and to a lesser extent white females, studying peoples of color, in part due to support by political relationships between ethnographic anthropology and colonial/neocolonial governments (cf. Sontag 1966; Malinowski 1967; Willis 1969). The politics of travel and access make it more likely that David Lewis will go to the Carolina Islands to study wave patterns, rather than a Polynesian to Martha's Vineyard to study Euro-American navigational systems. My strategies for de-emphasizing the effects of color and sex are offered within the context of continuing psychological resistance on the part of whites to being "studied" in the same ethnographic fashion as whites have traditionally studied people of color. Black ethnographers are a comparatively recent phenomenon (cf. Hsu 1973; Hsu et al. 1973; Jones 1970; Remy 1976). Being a black male makes negotiating the ethnographic passage politically more difficult in some situations, and easier in others. My ongoing ethnographic relationships were more satisfying with blacks than with whites. Did I cheat? Would a more culturally-different encounter involve my working more with whites? My relationship with resident Bequia whites, especially white males, was strained because of what I felt was their reluctance to put themselves in the position of one "studied" – especially by a male person of color (cf. Koentjaraningrat 1964: Weller and Luchterhand 1968). How would a black female ethnographer have fared? While the display of skill and use of participant observation strategies brought me close to most of the white males, the gifts inviting further contact were not forthcoming. Unlike Griaule, I was not prepared to wait 23 years for them to come. I have not solved this issue of the politics of color in the ethnographic situation, but I do know that it probably will not be completely solved by displaying competence and using participant observation.

Finally, an underlying premise of van Gennep's typology is that incorporation of the stranger into the host group is in fact desirable. Unlike adolescent to adult rites of passage, rites of passage during ethnographic research do not result in a fixed status and role in the society under study. Even though Griaule was given a Dogon ceremonial ritual to honor his death, his ethnographic rites of passage did not in fact make him a Dogon. The case of Frank Cushing reminds us that complete incorporation into the culture under study logically results in the elimination of the researcher role! Fine for the wayfaring stranger perhaps, but not so fine for the ethnographic stranger and for those who would find information on the Zuni useful to the cross-comparison of cultures. To maintain the researcher role, complete incorporation is *not* desirable. By its very nature then, ethnographic research is a liminal experience. This paper suggests strategies for making sense of the liminality, not strategies ensuring incorporation, I am not suggesting that it is desirable to "go native," though I feel being invited to do so is a high compliment. I only note that the most remembered experiences of ethnographic research are those where, despite the factors of color, sex, and the impossibility of incorporation, one is invited to participate in significant transition events and activities.

We are most familiar with van Gennep's separation/transition/incorporation sequence – a sequence which, I feel, is not as applicable or as useful to understanding host/researcher relationships as is his little-used stopping/waiting/transition/incorporation sequence. The initial stages of the prototypic ethnographic research experience invariably corresponds to this latter sequence. The separation/transition/incorporation sequence is more applicable to ethnographic research as a rite of passage into the professional status and role of cultural anthropologist. One can achieve the former without experiencing the latter.

For ethnographers, though, the latter sequence is itself an important transformational experience. In both cases, the concept of rites of passage, as modified by the implications of sex and color, remains a useful guide to detecting the form of the personal initiation and transformation experiences required by the ongoing practice of ethnographic anthropology.

# 6

# Walking the Fire Line: The Erotic Dimension of the Fieldwork Experience

*Kate Altork*

His hands reach up to remove the camera from around my neck, the tape recorder from my shoulder. Then he solemnly undresses me, as if I were a small and very sweet child. I stand willful before him, and there is no fear. His face is smudged with smoke and dirt, as he reaches out to pull me down into the water. Fighting the forest fire for days has not worn him down but, rather, stimulated him in strange and unexpected ways. His strong hands move like small, pulsing animals as he wraps my legs around his hard body. Looking up, I see pine trees, smell pine trees, taste wildness. The forest seems to be leaping, blurred by smoke from the huge fire many miles away. But we are underwater now, away from the smoke and heat, rolling like smooth river rocks in the stream. And we are suffused in sunlight.

Suddenly, I hear the helicopter moving overhead. It awakens me only seconds before the alarm blasts from the bedside table. I swiftly leap from the bed, my back and belly drenched in sweat. 'I don't believe it!', I mutter to myself. 'This is the third erotic dream like that I've had in a week! My fieldwork seems to be permeating every aspect of my life. I can't get away from it!' Packing up my gear in the hallway – cameras, tape recorder, notebooks, blank tapes – I vacillate between disappointment that it was just a dream and a growing sense of ambivalence. This fieldwork project has a grip on me that I'm unable to break away from even when I want a good night's sleep. It feels like an invasion – and a sweet and sour one, at that.

I was eighteen months into a fieldwork project in a rural, mountain town in Idaho when forest fires began breaking out in record numbers in the summer of 1992. Seven consecutive years of drought had rendered the land dangerously dry and vulnerable to fires started, more often than not, by lightning. Previous records were shattered, and a total of nearly 300,000 acres had succumbed to fire since 1985, more than 10 per cent of the gross acreage of that particular national forest.

As I studied and wrote about this place and the vernacular language utilized by the region's inhabitants to articulate and claim it, huge sections of the landscape were burning. All summer long, I had watched the planes and helicopters come and go, the small, local

Kate Altork, "Walking the Fire Line: The Erotic Dimension of the Fieldwork Experience," pp. 107–39 from Don Kulick and Margaret Willson (eds.), *Taboo: Sex, Identity, and Erotic Subjectivity in Anthropological Fieldwork* (London: Routledge, 1995). © Kate Altork.

better training than going out and trying one's hand at realist tales' (1988: 139). Earlier in the text, he defines the narrator of the realist tale as one who 'poses as an impersonal conduit who . . . passes on more-or-less objective data in a measured intellectual style . . . uncontaminated by personal bias . . . A studied neutrality characterizes the realist tale' (1988: 47). That he finds this 'studied neutrality' feasible, or the notion of 'posing' as 'an impersonal conduit' workable, I find curious in light of his extensive exploration into the ethnographic enterprise. Moreover, I detect in his writing a hint of uncertainty about the true validity of more evocative texts, in spite of the fact that he ostensibly champions such efforts, and perhaps a dash of the old boot-camp mentality. But the subtext says: 'I had to do the cerebral [read linear and grueling] work before I got to write from the heart [the fun part, and I paid my dues to earn the right to have fun], so you should do that, too.'

I would contend that a healthy blend of working from the mind and the heart is in order, and is the logical compromise here for the scholar negotiating the mind/heart dichotomy. The ongoing myth that one can separate the intellectual from the emotional is reminiscent of the mind–body split that the Western medical institution has traditionally embraced philosophically (and is just beginning to question and move beyond, I might add). To compartmentalize is to master, it seems to claim, in the face of compelling evidence to the contrary, evidence that it is the interconnectedness of bodily and emotional functions which shapes the lives of both healthy and unhealthy individuals.

Anthropologist Catherine Lutz speaks eloquently to this issue in her recent (1986) article, which criticizes the Euro-American construction of emotion. The central tenet of this construct, which she flatly labels 'emotion against thought' (1986: 289), centers on the notion that the emotional is antithetical to rationality (1986: 290). As such, Lutz writes, the prevailing view holds that 'To be emotional is to fail to rationally process information and hence to undermine the possibilities for sensible or intelligent action' (1986: 291). The ramifications of this view have been taken to heart by anthropologists concerned with building a base of professional credibility and respect among their academic colleagues. This has strongly influenced many ethnographers, who have traditionally striven to excise the affective from their ethnographies in order that the work be viewed as 'scientific' and, therefore, valid.

The logic behind this choice has become chillingly justified by the recent backlash against so called 'reflexive' writings, in which those who bring their emotions and personal biases into the discourse for analysis and examination have been accused of being 'too personal'.[5] 'Self-indulgent', they sniff when a field account is particularly delicious – more like a chocolate mousse than a piece of dry toast – 'unprofessional', 'inappropriate'. Or, worse yet, some anthropologists are accused of 'going native', as if by immersing themselves in a culture they have been somehow caught with their anthropological pants down doing the wrong thing (getting involved to the point where they are enjoying their work, perhaps?) at the wrong time.

As for those critics, who perhaps have never had the nerve to skinny-dip in their own work (while trusting their brains to function simultaneously), or to write from the hip – or from any other body part below the brain, for that matter – those critics sharpen their No. 2 pencils and produce another barrage of unintelligible, but academically familiar, books and articles. Business as usual, they say. What a relief.

For when certain anthropologists began writing more self-reflexive and passionate ethnographies,[6] a brief reprieve from the mind–body split occurred. The tight corsets were loosened, the mouth was allowed to relax, and a veritable flood of feelings and decidedly subjective materials came pouring out, mixed nicely with objective, rational perceptions and ideas into a fragrant, new intellectual stew. As though a fire hydrant in an inner-city neighborhood had broken on a sweltering August afternoon, some got to run around for a few minutes getting wet and having a ball. They also reached their goal, which was to cool down.

But, as one might imagine, it didn't take long for the word cops to come in with their sirens blaring, and their lights flashing. Before

This medium, on a continuum as she describes it, moves from lively interest to passionate (although not necessarily consummated) erotic attachment (1993a: 11) and facilitates the fieldwork endeavor. It does so, she asserts, by motivating informants to enter into a relationship, rather than feeling intruded upon by the anthropologist's presence or questions. Newton's unconsummated 'crush' on Kay, an elderly woman she interviewed extensively for her ethnohistory of the gay and lesbian community of Cherry Grove (Newton 1993b), appears to have been beneficial to both parties. Of critical importance to the present enquiry, however, is her statement that the relationship significantly inspired her intellectual and creative work. She argues that 'the most intense attractions have generated the most creative energy, as if the work were a form of courting and seduction' (1993a: 15).

This sentiment is reflected in the earlier writing of Manda Cesara.[3] It was while making love with an informant whom she calls Douglas that she experienced an epiphanic moment. It was at that precise moment, she claimed, that she grasped a basic truth about the Lenda[4] culture. 'Body and brain were one, as were mind and flesh, the past and present, life and death', she wrote. 'I experienced the cerebral in the flesh. In Lenda nothing ever is purely cerebral, it is always mingled with flesh' (1982: 55).

Cesara wrote openly about her consummated sexual encounters in the field, claiming that 'it was inevitable that some ethnographers in certain settings should experience such an encounter' (1982: 59). Predating Newton's article by nearly a decade, Cesara's work asserted that the emotion of loving one individual in a culture allows one to lay hold of the culture itself by way of that individual person (1982: 60). Whether the relationship is consummated or, as in Newton's case, is contained as what might be called an affair of the emotions may be secondary to the fact that both anthropologists felt themselves and their work to be enriched by their passionate (and, of critical importance, reciprocated) attachments in the field.

What is it about the fieldwork setting that might foster an opening to erotic possibilities? As Dubisch (1995) notes, it certainly isn't the academic preparation for the field, where the prevailing protocol still seems to involve the twist that one has to be in the field in order to learn about how to be in the field (p. 30). And, as she rightly points out, advice about sex is not a part of the preparatory package for one embarking upon field research. Yet, as many anthropologists know, the most compelling ethnographic writings inevitably contain rhapsodic and sensual descriptions of people and places. What happens, on a subjective level, when an anthropologist and an entirely new environment collide?

It has been my experience that any new locale sends all of my sensory modes into overdrive in the initial days and weeks of my stay. I can recall the particular smell of the earth in the western wilderness as the snow receded from the trees after a long winter. I remember the pungent smell of sargasso seaweed fermenting in the shore break during the height of summer at a sea-lion refuge in California. The mention of a place can often trigger a cacophony of sensory memories: the taste of Greece, the sounds of downtown Washington, DC, the almost unearthly quality of the light surrounding the Sangria de Cristo mountain range near Santa Fe.

Unfortunately, perhaps, the adaptation process dulls the sharp sensory feedback we receive from our surroundings once we've been in a given locale for a time. And for the anthropologist, traditionally charged with the task of scientifically studying 'the Other' while dressed in the straitjacket of so-called objectivity, the insistent feedback of the senses has often been something to be denied, squelched, or, at very least, granted secondary status to the intellect. It's almost as if there was something not quite right about responding and writing from a reflexive stance – a personal place – unless one hastened to link an overarching cerebral tone to the affective to ensure credibility.

Even Van Maanen (1988), who has written eloquently about the writing of 'impressionistic tales' as a means through which to breathe life into the field experience, is reluctant to allow the neophyte to bypass the traditional ethnographic writing route to engage in such passionate writing. When it's all said and done, he states, 'On advice to students of fieldwork, my feelings are traditional. There is, alas, no remarking that walking the line between detachment and involvement is a built-in part of a field relationship. Nader writes, 'The amount of nocturnal dreaming and the ability to remember dreams seems to have multiplied several times over my usual behavior.' Landes (1986: 121) echoed that theme when she noted that one cannot separate 'the sensuousness of life from its abstractions, nor the researcher's personality from his experience.' The fieldwork experience is, in other words, a highly subjective process, affected deeply by bringing to play all of the senses.

When I locked eyes with the fire fighter, I thought of Landes' essay on her fieldwork experience in Brazil. The Brazilians call that gaze '*jogo de olhos*' (the game of eyes), Landes wrote. Her mentor cautioned that this can be dangerous in the field, as it might signal 'love information'. He believed the direct gaze was improper in that particular culture (1986: 135). In my fieldwork experiences, however, it often seems to facilitate matters, enhancing the communication process between myself and others in an already highly charged environment. Opening to the sensual aspects in my field surroundings, I am able to feel the environment in multiple ways. This opening is not restricted to flirtatious interplay (which may or may not occur) but, rather, encompasses an expanded use of all of the senses and a willingness to allow myself to gaze openly at others.

What is it about the senses that make their synthesized integration into the fieldwork process so important for the anthropologist attempting to come to know a place and its people? The definitive writer on the power of the senses may be Diane Ackerman,[7] who has painstakingly explored each sensory mode for its unique qualities and who informs us that, 'Seventy per cent of the body's sense receptors cluster in the eyes, and it is mainly through seeing the world that we appraise and understand it' (1990: 230). She points out that lovers close their eyes when they kiss in order to shut out visual distractions. 'Lovers want to do serious touching and not be disturbed', she writes. 'So they close their eyes.' Such is the power of the gaze, and the explanation helps us to understand why intense encounters with people in the field can have, at times, a seductive quality to them. Fieldwork involves coming into contact with many different people for the first time, a process which typically includes visual appraisal of the Other. Thus, the work itself brings with it possibilities for intimate scrutiny and the resulting need to confront our feelings as we strive to understand the workings of other minds in other places.

Ackerman doesn't stop at vision, in her exploration into the nuances of the senses. About smell, for instance, she writes, 'Smells . . . rouse our dozy senses, pamper and indulge us . . . stir the cauldron of our seductiveness, warn us of danger, lead us into temptation' (1990: 36). We bring our nose to the task, too: 'Odor greatly affects our evaluation of things, and our evaluation of people' (1990: 39). And, as if that weren't enough to contend with, our ears also fight for equal access to the environment. Ackerman writes, 'Sounds thicken the sensory stew of our lives, and we depend on them to help us interpret, communicate with, and express the world around us' (1990: 175). Finally, she reminds us that we have another powerful tool through which to view the world because, 'as sages have long said, the sexiest part of the body and the best aphrodisiac in the world is the imagination' (1990: 131).

As anthropologists have traditionally struggled to maintain an objective, 'scientific' posture in the field, is it any wonder they have perhaps experienced stress and confusion? Instead of blocking out this wealth of sensory (and sensual) input, or relegating it to private field journals, we might consider making room for our sensual responses in our work. The senses that we are equipped with are powerful antennae through which to experience, providing us with full use of what Ackerman calls our synesthetic abilities (1990: 290).

This involves opening to input from all of the senses, which combine to provide us with an enhanced understanding of others, of our surroundings, and of ourselves. In synesthesia, there is an intermingling of the senses so that, for example, one can taste a starlit sky or hear it, rather than only seeing it. Ackerman writes of how Rimbaud understood the power of synesthesia, as evidenced by his remark that the only way one can truly experience life is long the water is turned off. The kids go back to playing desultory and predictable games of stick ball in the streets. The anthropologists go back to writing from the head, hiding their clamoring feelings under the heavy blanket of technical language comprehensible only to their own kind. The party is over. Is this what we want?

Some anthropologists appear to be caught in the trap, wanting to integrate the emotions and the intellect into the language of anthropology, but fearful of criticism, of not being taken seriously. Although we may subjectively know that our senses work together with our intellects to provide us with data in complex and elegant ways, we persist in asking fieldworkers to operate predominantly from their eyes and ears and – most certainly – from the waist up. Repressing or avoiding our own erotic and sensual responses, we work in a haze of sensory anesthesia of our own making.

## Places Penetrating People: Opening to the Senses

I sit between two men, in a crowded roomful of men, listening to fire talk. The room smells of aftershave and leather, and laughter punctuates the space as they take a break from logistics to share an inside joke. Fire experts from Washington, DC, nod in agreement as local experts discuss their strategies for putting out four major blazes which have burned over 27,000 acres of forest in recent days. They talk a fire language that's rough and masculine. 'Most of the heat in the fire is out at the head, and it's running to the north', they say. 'We had another fire pop up down here at Cherry Creek and just bam! Right out of the clear blue sky. We pounced on this one down here with an engine and one crew that picked it up. I knew it would work. That guy's fire savvy. He knows fire and he knows his job.'

Glancing over at the man next to me – he smells like sunlight and oak – I startle when I realize he is studying me. For a moment we lock eyes. A man across the table says, 'Let's hope you have a wet year next year', and another responds, 'Yeah, let's hope it goes the way of the wet year!' As laughter fills the room, I feel my face flush. The man next to me smiles a wry, winning grin and I return the smile, turning my face away, flustered. He's in charge of one of the fires, an Incident Commander, as they call it. I enjoy listening to his husky, earnest voice as he talks about his firefighting strategies. His hands move in swift, confident arcs through the air as he speaks. He says, 'If this thing gets bigger, we may need a few more people, but we try to keep it lean and mean, so . . .' When I am not taking my own notes, I study the bold blond hairs running up and down his tanned arms, the other voices in the room entering and receding from my awareness.

I struggle to integrate the cognitive dissonance I am feeling: the part of me that is attracted to these men who are, as a group, earnest and intelligent, charming and attractive, with the part of me that is unnerved and irritated by their blatant use of macho sexual imagery to discuss forest fires. The privileged sense of entitlement and ownership over a natural force which they appropriate by way of an insider's language seems, at times, to be both insidious and morally incorrect. Yet, as Cohn courageously admits in her essay on the experience of studying nuclear strategic analysts (whom she calls defense intellectuals), there is something thrilling about 'entering the secret kingdom, being someone in the know' in a realm that is both powerful and hidden from the outside world (1987: 704). Even as I struggled to analyze their 'fire language', and to situate it as a language of power and appropriation, I felt myself to be seduced by it, and felt privileged to be privy to it as a temporary 'insider', an experience both uncomfortable and intriguing.

Spontaneously, I ask this man if he will let me interview him about firefighting. When he says he'd like that and gives me the directions to his place for us to meet, I find my mind moving swiftly in two directions. One of them plans a list of fieldwork questions. But the other one . . . the other one fantasizes. And it is those fantasies which find their way into my dreams, causing me to awaken sometimes in the middle of the night as I work the fire project.

Nader (1986: 111) claims that ambivalent feelings are always involved in fieldwork. She recalls anthropologist Hortense Powdermaker feelings, is a kind of human tragedy in the light of his obvious gifts of synesthetic awareness. Moved by the landscape, deeply responsive to both the place and its people, he was unable to utilize his sensual knowing without guilt, to unlock more fully the secrets of the culture he studied. His desire to leave the field as soon as possible can certainly be better understood when one comprehends the incredible tensions he wrestled with. Unable to reconcile his fantasies with his image of himself as a scientist, he suffered. One can only wonder about the ways in which his work suffered, as well.

Mariana Torgovnick, a writer and professor of English, has written compellingly about the Western discourse on the primitive, or 'Other', asserting that it has often been a rhetoric of domination and of control. She offers a penetrating critique of a kind of arrogance which creeps through the thinking of the Western academic, who looks at the world as if the Western way is central, with everything else being considered 'non-Western' or derivative. Torgovnick argues that this rhetoric often hides what lies underneath: our more 'obscure desires: of sexual desires or fears . . . masking the controller's fear of losing control and power' (1990: 192). This, she asserts, allows the Western scholar 'to document the intimate lives of primitive peoples so that we can learn the truth about us – safely, as observers' (1990: 8).

Regarding Malinowski, Torgovnick states that 'the need to forget bodies – his own included – is part and parcel of the kind of scientific objectivity [he] sought' (1990: 230).[10] His disgust with the field (with himself?) emerged blatantly when he wrote, 'The life of the natives is utterly devoid of interest or importance, something as remote from me as the life of a dog' (1989: 167). This statement becomes even more disconcerting when one considers how moved he was by the field earlier in his stay, writing from a sophisticated level of synesthetic awareness. Yet, because he was denied a way to cope with his sexual fantasies, he censored rigorously, corseting himself into a narrow view of the place and its inhabitants. It is my belief that this could not help but weaken both his work, and himself, in the process; a high price to pay for being 'objective'.

Torgovnick vigorously defends Malinowski's passions, boldly stating that 'We should savor his unprofessional desires' (1990: 227), and calls for a 'rhetoric of desire, ultimately more interesting, which implicates "us" in the "them" we try to conceive as the Other' (1990: 245). This progressive viewpoint makes good sense when one considers that we are biologically wired to operate on multiple levels, possessing the capacity to use our bodies, hearts, and minds together to funnel and to interpret massive amounts of stimulus. By contextualizing our erotic responses and channeling them into the ethnographic endeavor, we might better represent those we study and claim to know. We would do this by fleshing out our accounts of people and places, openly bringing our own passions and insights to the task and articulating them in our textual representations.[11]

What is particularly interesting to me about the Malinowski debate is not the fact that he lusted for the women he studied at certain very human moments, but the fact that the revelation that he did so triggered such an uproar in the anthropological community. This speaks clearly to the somewhat unbelievably repressed state the discipline (and its inhabitants) existed in and, I would contend, continue to exist in. What were they thinking? That Malinowski would be far from home, steeped in a foreign and exotic culture for a long period of time, and be dead as a sensual being? The fact that his private journals were seized upon – as if they, too, should have been strictly scholarly and scientific in their contents – attests to the denial that the field operates in, its residents sublimated in the ubiquitous voyeuristic frenzy of uncovering someone else's secrets while affecting an emotional distance from their experience.

## Defining Sex in the Field: What Is It, Anyway?

This brings us to another problem, which involves the defining of sex in the field. The traditional stance in American culture holds that sex has occurred when penetration – preferably leading to male orgasm – has happened. Do we, as anthropologists, want to to be prepared for 'a long immense planned disordering of all the senses' (1990: 291).

Is it any wonder that the social scientist might feel threatened by the rush of sensory input and emotions that floods in upon entry into a new culture? How are we supposed to collect data in the cool and rational fashion we have been taught to affect, when the senses are 'disordered'? It's no wonder that the erotic elements of the field are rarely discussed and often denied. Such a messy business, trying to tabulate data and cope with a barrage of smells and tastes and errant fantasies or desires at the same time! In a culture which tends to deny dissonance (and deify order) this sensory barrage can often be viewed as an unpredictable burden or liability. Yet one can only conjecture how the quality of ethnographic writings might be enhanced and refined if it were culturally sanctioned to write about the field from this vantage point, eliminating the need to repress or compartmentalize certain feelings and thoughts.

One has only to think of the infamous Malinowski *Diary* (1989)[8] to wonder how his view of the fieldwork experience might have been transformed if he had accepted his own passions. The academic community's dogged focus on his sexual fantasies bypasses an equally compelling fact: Malinowski was a true synesthete. Throughout the *Diary* are descriptions such as this one: 'Marvelous sunset. The whole world drenched in brick color – one could *hear* and *feel* that color in the air' (1989: 67; italics in original). His ability to experience and to write in such a sensually descriptive manner allows us to witness the ways in which he was permeated by his environment. Perhaps his sexual stirrings, in part, were simply a byproduct of his sensual awakening in this new and exotic place. 'I went alone to Wawala', he wrote. 'It was sultry, but I was energetic. The wilderness fascinated me . . . Kenoria is pretty and has a wonderful figure. Impulse to "pat her on the belly". I mastered it' (1989: 153). One can follow, in these sentences, the way his mind moved from the sensual to the sexual in an organic, complex way, impacted by his intense emotional reaction to the landscape. To focus selectively on his comments about the woman he watched, by lifting it out of its environmental context, is both unfair and reductionistic.

The *Diary* is fascinating, in part, for what it reveals about Malinowski's extreme struggle to avoid elements of his own sensory input in order to affect the mask of the neutral scientist. This struggle was undoubtedly complicated by his Polish Catholic upbringing, as well as by an academic background in physics and mathematics that is likely to have predisposed him (by training, as well as perhaps by natural predilection) to privilege the rational and logical over the emotional or passionate. Moreover, he was engaged in a meaningful relationship with the woman who would later become his wife. His guilt at harboring what he apparently interpreted to be sinful thoughts about the women he saw in the field (while his beloved was far away) probably further fueled his conflict.[9]

It is highly unlikely that his academic training in anthropology – influenced by the British school of anthropology with which he was associated – gave him any way of understanding what was happening to him emotionally in New Guinea. Thus, having no context in which to house his erotic and sensual reveries, his writings disclose the incredible tensions with which he wrestled: 'I realized once again how materialistic my sense reactions are: my desire for the bottle of ginger beer is acutely tempting', he wrote. Then he confesses: 'Finally I succumb to the temptation of smoking again.' He manages to justify his behavior in the end by claiming, 'There is nothing really bad in all this. Sensual enjoyment of the world is merely a lower form of artistic enjoyment' (1989: 171–2). How sad, it seems to me, that he squelched his abundant nature this way, torturing himself for what could be construed as his natural reactions to his milieu.

Malinowski's efforts to repent for what can be called his 'sensual sinning in the mind' was militaristic in its rigidity. He wrote, 'I must have a system of specific formal prohibitions: I must not smoke, I must not touch a woman with sub-erotic intentions, I must not betray E.R.M. [his beloved back home] mentally, i.e., recall my previous relations with women, or think about future ones . . . my main task now must be: work' (1989: 268). The emotional hardship and pain he experienced, generated by the struggle with his own complex mix of and have been kept, for the most part, in the realm of 'underground' stories or anecdotes, it is striking to consider how long the consideration of such relationships has been avoided and repressed by the discipline.

It may be that the pose of objectivity in the field is no more than another version of the folk tale about the emperor with no clothes. As anthropologists, we would do well to remember that we are all naked at times in the field – if not physically, then certainly emotionally. Therefore, despite our intellectual armor, we are inevitably somewhat open and vulnerable. And if it is true that the emperor really has no clothes on, then perhaps we should just stare openly at him, in the way of children, and see what he looks like. After all, it's just a body. And we might learn something.

## Sexy Business: Immersion in the Field

The Warm Springs fire camp, an instant community constructed to meet the basic needs of 1,100 firefighters and support personnel, existed for sixteen days in August of 1992. Operating smoothly through hot, dry days and cold, sometimes snowy nights, it moved to its own rhythms under glaring sun or dangling stars, wrapped in the smells of pine and smoke. Huge generators provided electricity for light and cooking fuel in the camp, while semi-trucks housing portable showers gave weary firefighters a means to rid themselves of soot and sweat. Kitchens turned out hot, full-course meals three times a day and snack stations were located throughout the camp. The telephone company was brought in to hook up telephones, enabling those on site to connect with the outside world – to phone home. Portable sinks stood near the showers, equipped with mirrors and toiletry stands. While there were only approximately thirty females in the camp (and they were given separate showering and sleeping facilities), male and female firefighters and support personnel mingled socially with one another, moving from the showers to the sinks where the men shaved and men and women alike combed hair and engaged in friendly banter.

Under large, portable canopies, meals were taken and an ambience of warmth and camaraderie prevailed. At the long tables, it was not uncommon to see the sports pages handed from man to man[12] as each finished reading it, a gesture both friendly and surprisingly intimate, infusing the space with a quality of homelike domesticity. Throughout the camp, quonset huts provided sheltered stations for weather forecasting and map making, tool and equipment cleaning and dispersal, first aid assistance, strategy planning meetings, and the filing of disability insurance claims. A portable commissary dispensed razor blades and long johns, toothbrushes and shampoo, and T-shirts which proclaimed, 'Warm Springs Fire Camp – 1992'. At one end of the camp were the sleeping grounds, where large tents housed as many as twenty sleepers and small tents served those who preferred privacy.[13] Paths between tents were lit with ground lights, which gave the area a glowing, dreamlike quality at night. Men and women stood around smudge pots after dark when the temperatures plunged, talking and warming their hands over an electrical, high-tech version of the traditional campfire.

During the day, the sounds of heavy machinery filled the air as helicopters, buses, trucks, and cars brought firefighters on and off the fire lines some fifteen miles away from camp. Competing with that of the generators for airspace, the noise was intense and omnipresent. This was in sharp contrast to the late evenings when, after nightfall, people sat in groups telling stories and laughing, walked the paths in the sleeping areas, or slept noiselessly in their tents after long, grueling days on the line.

My response to this environment was immediate and clear. Intense and complex, it was one of the most engaging environments I had ever worked in and I couldn't get enough of it. My senses were hit with a constant onslaught of sounds and sights, smells and tastes. Sparks flew off shovels being sharpened for use on the line the next day. The smell of chicken and potatoes drifted from the cooking quarters. Machinery and voices, mixed with the rapid pace of motion in a place where the mission is the reason for its very existence – it was an intoxicating little world within the world, far restrict ourselves to that notion? If one becomes infatuated with someone in the field, or has a flirtatious fling with them that stops at deep kissing or intense longing, does that mean that sex didn't occur? Further, does that mean that it did?

The analogy of sex in the 1950s and early 1960s might be useful here. It is a well-known fact that in those years 'good girls' didn't go 'all the way'. While it was commonly understood that many 'good girls' were, in fact, 'doing it', the etiquette of the day called for energetic denial. Thus, the 'good girls' who were 'doing it' were lying about it, which made them apparently morally and socially superior to the 'bad girls' who were both 'doing it' and telling the truth. The irony here is obvious. And for the girls who didn't go 'all the way', but went 'almost all the way', one could argue that by privileging such a small part of the sex act (penetration), we bypass the obvious fact that the ones who went 'all the way' at least weren't pretending that they weren't 'doing anything'.

An anthropologist I met once told me about a 'flirtation' she had with a male informant, hastening to add that 'We never actually had intercourse, but it certainly got intense there for a while!' When pressed, she admitted that the encounter was 'certainly sexual', but pointed out that she had later reminded the man of her role as an anthropologist and his as the 'informant', as if somehow by so doing she could erase what had transpired between them. Her inability to contextualize her own feelings resulted, I would contend, in a messy situation in which she attempted to justify her actions, while struggling to find a logical way out of a mess she hadn't yet been able to acknowledge to herself, a difficult state of affairs to unravel, to say the least. Pressed further, she confessed that the man had been both confused and angry with her, prompting her to blurt out, 'I guess I didn't know what I was doing, did I? I thought I was being a good professional by calling a halt to it.'

This particular anthropologist was concerned that if anyone were to find out she had developed a sexual interest in an informant, her work would be discredited. She felt forced to hide her experience in order to protect herself and, in so doing, became unable to analyze or to understand her own experience. An accomplished professional with a long list of publications to her credit, she will admit privately to 'involvements' with people in the field, while protecting herself publicly by claiming that her work is objective and unaffected by her personal feelings.

What are we afraid of? Do we make the assumption that an anthropologist who becomes sensually or sexually involved in the field can no longer think straight? I would contend that all relationships and events with which we are involved in the field change us in subtle ways and affect the way we perceive, and write about, the field. Does the anthropologist who plays at being 'objective' really create superior work to the one who immerses himself or herself in the field wholeheartedly? I am not advocating random and meaningless sexual encounters here, nor am I talking about situations where issues of colonialism and power imbalance enter into the discourse, which may be, in fact, most of the time.

But anthropologists today increasingly work in field situations where they operate collaboratively with so-called 'informants' (often called collaborators, in recent years) who are not inferior in terms of status or power. To hold on to the shield of 'neutral objectivity' in such situations, protecting oneself from being 'touched' by the field, might be unnecessary in certain circumstances. The validity and sanctioning of relationships which are not abusive and which are mutually desired need to be explored as possibilities, for what they might teach us about ourselves and our ethnographic endeavors. In order to engage in such a dialogue, as a discipline, it is critical that we step out from our hiding places and explore our feelings and beliefs.

The point is not to encourage sensationalistic, *National Enquirer*-type confessionals from the field, replete with descriptive close-ups and minute details about how a given anthropologist had sex in the field. But we might at least acknowledge that we 'did it' if we did (or that we wanted to 'do it', even if we didn't) and be open to the fertile possibilities for dialogue about the ways in which 'it' changed, enhanced, or detracted from what we felt, witnessed, and interpreted in the field. In a discipline where such encounters have taken place, There is also a quality of ownership regarding the forest environment and the fire that is sexy in its expression. Firefighters often refer, almost possessively, to 'my fire', or 'my baby' (in reference to the fire itself), using a language of passionate attachment when they describe their commitment to the work they are doing. For me, this unremitting and focused attention to the task at hand had an extremely erotic quality to it, like a lover whose eyes remain fixed on my face during the act of lovemaking. And this attachment is not just a solitary connection between an individual and the land. There is a collaborative, interdependent quality about the firefighters' activities which is moving to witness. One man articulated it this way:

> There's only us to take care of us here. And so people, being the creatures we are, tend to stick together and while we can be extremely independent, we also need, I think, that other human contact . . . and that reassurance that there are other people there working toward the same thing. So we go to other people for support even though they are relative strangers.

The vulnerability and need to connect with others expressed here add to the elements which come together to create a sense of place that is warm and inviting, yet crackling with excitement and potential danger. While there were occasions when I witnessed what appeared to be active flirting – three men laughingly throwing a cream pie on the shirt of a beautiful, blonde-haired woman (a cohort from the same management team), for instance, and her teasing, easy response[17] – some of the most sensual behavior in the camp took place between the lines. One could witness it in the silent sharing of tasks, the looks of respect and admiration that passed from one person to another when progress was made, the way in which camp inhabitants shared small spaces in the easy manner of those who feel safe and familiar with their companions because they all know what they're doing and share the same goals.

Nor is this dynamic confined to cross-gender interactions. The warmth and connectedness fostered by common purpose in a charged environment operated palpably between men, as well as between women. Back-patting, the tousling of hair, an arm thrown casually around a shoulder – these means of physical contact bespoke camaraderie, mutual respect, and friendliness in a setting where teamwork is the name of the game. Like the butt-slapping of football players in the NFL, these small physical acts which transpire between team mates signify 'insiderness' and connectedness in unique circumstances comprehended fully only by those on the 'inside'.[18]

For an anthropologist to enter such an environment and operate solely by asking questions and recording them on tape would be to miss the point. Some of my clearest insights into camp dynamics came while walking the paths in the sleeping areas at night, listening to the 'feel' of the place. Despite the codified and prescribed external structure of the US Forest Service fire system, there was a sense of expansiveness and softness that seemed to permeate the overall experience. In other words, the subtleties about what makes a given environment tick often go on in the spaces between the work and the words. By opening to the senses, I was able to move closer to the nuances of the place, and to sense its meaning to those who inhabited it. By immersing myself in the field, I began to understand this small subculture from the inside out, rather than from the outside (by way of the intellect) in, 'feeling the field' with my body and heart, as well as with my brain.

Perhaps it was in this frame of mind that anthropologist Paul Rabinow had one of his most sensual encounters during his early fieldwork in Morocco. His account of making love to a woman from another village during a day off from fieldwork has been written about and scrutinized for years by other anthropologists.[19] This account is fascinating to me, not for what it reveals about his sexual behavior, but for his description of what led up to the sexual event itself. For it is here in his account that he describes how he left his fieldsite for a couple of days, let his guard down, and began to respond openly to his surroundings with the sensibility of the synesthete. He wrote, 'It was a beautiful, cloudless day, and we drove joyously away from Sefrou [the fieldwork site] into the mountain areas' (1977: 63). He goes on to say, 'As we left the highway, town, and from the realities of day-to-day life away from fires.

Unlike Malinowski's, my more progressive training for the field allowed me to relish the life of the senses while there. My graduate training, as well as my readings and writings on the anthropology of place, gave me a suitable context in which to house my own experiences.[14] Consequently, I was able to immerse myself sensually in this new environment, feeling my way into a growing understanding.

In the Warm Springs fire camp, I found myself in a very sensual world where the body meets one of the most powerful natural elements known to humankind – fire. In attempting to analyze what it was about this particular place that made it so sensual and intriguing, despite the military-like uniforms and the ubiquitous heat and dust, I turned to some of the fire personnel for answers. Regarding the most direct displays of sexuality, one woman had this to say:

> I'll tell you, after five days men get horny as hell and they will proposition anything they think they can bed. We call them fireline romances. You're very tight with people and shut off from the outside world.

It was this woman's opinion that such encounters were to be expected in situations where people are working closely with one another and are far from home. This is not to say that sexual activity is either condoned or commonplace in the firecamp setting. Indeed, official US Forest Service policy dictates that firefighters and support personnel refrain from engaging in sexual activity with one another. Further, policies are set in place which are designed to deal with accusations of sexual harassment, and the people I spoke with seemed to feel that little, if any (depending upon whom one asked), outright sexual contact took place in the fire camps.

As for the males, there were a number of men with whom I spoke who alluded to the fact that women 'came on' to them after days of working together. If men were 'coming on' to other men (or women propositioning other women) in that environment, such activities remained resolutely hidden in the recesses of an institution which reflects the military's strong homophobic predisposition.[15]

What is critical here, however, is the fact that men and women were working together for long periods of time, engaged in activities which they found meaningful in a way that was defined by their collective goal – in this case, putting out fires – and that these kinds of human engagement are sexy by definition. I often saw small clusters of people laughing or talking together with a kind of intensity and mutual affection that is born out of such settings, where life revolves around a clear purpose and people know that they are equipped to get the job done.[16] By their nature, then, these types of setting are erotically charged. The issue is not one of men and women, but one of human and human, working together in a setting where everyone matters and each action signifies. There are few activities engaged in by people in more ordinary environments which have the kind of stark coherence and clarity of mission that one witnesses in the fire camps.

The similarities between people in a fire camp and anthropologists, who develop bonds with the people they study when immersed in a community far from home, can be teased out here quite readily. Moreover, in the case of the fire camps one can add to the mix the element of danger. One officer put it succinctly when he said, 'It's hazardous country, they're hazardous trees. It's hard to fight fire here. Fire fighters can get hurt. And, in the worst case scenario, they can die.' Therefore, these people work in an almost continual state of heightened physiological arousal. One woman said:

> You're on a constant adrenaline high because you have something important to do and you have to do it right away and then you go back to the camp and you crash. But ir's a nice crash, because it's all encompassing. When I get bored on my regular job, I tell my boss, 'I need a fire to go to!'

Firefighters, who often refer to themselves proudly as 'fire bums' (in reference to their willingness to drop everything to 'go to a fire'), seem to thrive on an adrenaline-induced state of vigilance and energy that the fire situation fosters. This arousal state was palpable and I found myself responding to it by bringing a different quality of intensity to my own work in the camp.

filled with ambivalence. What will my colleagues think if I put sexual fantasies in a chapter like this? I visualize myself reading a professional paper, standing in front of a large room full of anthropologists. Suddenly, someone shouts out, 'So, do you really think it's OK to have sex when you're working?' Another voice rings out aggressively, 'So, you're condoning sex in the field? How unprofessional. How could you?' And the feminists ... all this talk about men and erotic attractions. Where is the evidence of my twenty years of feminist self-identification here, hidden behind talk of macho machinery and locking eyes with male strangers?

If I'm going to write about sex, I tell myself, perhaps I'd better at least protect myself with some scholarly armor. Maybe I should retreat behind the cool, gray wall of academic language, haul out some big words. I could throw in a few of the top-ten favorites of the earnest anthropologist, for instance – problematize ... metacommunication ... de con struct ion.

Margaret Mead (1972) once wrote of how troublesome Reo Fortune's (1963) passionately written account of the Dobuans became over time, fostering suspicion from many of his anthropological colleagues about the validity of his work. His immersion in the world of the sorcerer, and his choice of writing about it in a lively and subjective way, weren't considered appropriate by those who expected work produced solely from the precisely analytical part of his mind (Mead 1972: 184; cf. Cesara 1982: 136). As Cesara accurately points out, 'He [the practicing anthropologist] cannot escape passion, and yet he is not able to claim it as central to his knowledge' (1982: 100). Therefore, one is left with the impression that it's bad enough he had to feel it (passion, the heat of the field), but even worse that he couldn't keep it out of his writing (poor boundaries, bad judgment).

But there are others in recent years who see it differently. Paul Stoller (1989), for instance, speaks movingly about his long-time commitment to one particular place and its people, in one of his writings about the Songhay of Niger. 'In 1969 my senses were tuned to otherness ... my senses of taste, smell, hearing, and sight entered into Nigerian settings', he wrote. 'Now I let the sights, sounds, smells, and tastes of Niger flow into me' (1989: 5). By not restricting himself to the Western 'gaze', which he refers to as the 'privileged sense of the West' (1989: 5), he was able to produce a representation of a people and their lives that is vital, rich, and alive.

Stoller calls on the anthropologist to move beyond the dry and impoverished language of traditional academic writing, in order to give the reader what he calls 'the taste of ethnographic things'. This, he contends, can be done by writing ethnographies which attend to the sensual aspects of the field, ethnographies that 'will render our accounts of others more faithful to the realities of the field – accounts which will then be more, rather than less, scientific' (1989: 9). By allowing ourselves to be penetrated by the field, with all of the dissonance that may elicit, we might 'walk along our solitary paths in the field, exposing our hearts so full of excitement, fear, and doubt' (1989: 54–5).

It is important to note that Stoller is not suggesting that we relinquish the 'objective' data at our disposal, nor that we lapse into experimental forms of representation that render understanding difficult, if not impossible.[20] Rather, he contends that the anthropologist can choose to work from a place of vulnerability and acceptance of the emotional and intellectual dissonance which is part and parcel of the process of trying to come to 'know' a place and its people. It was the understanding of this dissonance which allowed me to integrate and to interpret my sexual fantasies and sensual reveries as a normal part of the fieldwork process, and therefore to set the limits that kept me from acting on them, particularly when tempted by the golden arms of a handsome firefighter.

Working from this place of vulnerability and acceptance can give the reader 'the taste of ethnographic things', Stoller asserts, with 'filmic or narrative images: the smells, the tastes, the sounds, the colors – lyrical and unsettling – of the land' (1989: 156). By so doing, we might enable the reader to sense what it is like to live in a certain place without having to pretend that we have mastered a culture intellectually while maintaining a detachment from it emotionally.

society behind, I felt a mounting excitement, as if personal inhibitions and social conventions were also being left behind.' He admitted then: 'I had never before had this kind of sensual interaction in Morocco' (1977: 65).

The heart of his problem as a self-defined objective 'scientist' is then revealed, as he continues: 'Although it was incredibly welcome it seemed too good to be true. Haunting superego images of my anthropologist persona thickened my consciousness as the air became purer and the play freer . . . I felt wondrously happy – it was the best single day I was to spend in Morocco'(1977: 65). This is a remarkable statement, in the sense that it leads one to question how he was recording his notions about the people and place he studied *before* he opened to the fuller use of his own senses. Shut off from the deeper part of himself, restricted – as Malinowski perceived himself to be – to the role of voyeur and recorder, he deprived himself of aspects of his own sensual awareness as a potent data source.

This is all the more interesting in the light of his confession that the sexual event itself was anticlimactic for him. What really moved him was his ability to let his guard down and open up to the sensuality of the weather and his female companions. 'Here we were, after an absolutely splendid romp through the mountains, sitting down next to some sulphur springs, and they were going swimming', he exults (1977: 67). Of the sex itself, he wrote somberly, 'Aside from the few pillows and charcoal burner for tea, there was only the bed. The warmth and non-verbal communication of the afternoon were fast disappearing. This woman was not impersonal, but she was not that affectionate or open either. The afternoon had left a much deeper impression on me' (1977: 69). In other words, when the visual and sensual elements were altered, and he was face to face with a woman with whom he had no true emotional connection – in a meager environment far removed from the sensual joy he had experienced earlier that day – the act of lovemaking was internalized as a diminishing act by comparison.

This statement is courageous, in my view, in that it not only breaks down the myth that sensual pleasure must culminate in sexual intercourse, but also highlights emphatically the idea that the anthropologists can make a conscious choice about how to conduct themselves if they understand their own sensual experiences and can put them in perspective. Perhaps Rabinow might not have chosen to make love with the woman if he had understood the nature of his own intense experience that day. Having no way to contextualize his feelings and responses, he carried through to the logical – although perhaps unnecessary – conclusion. Having sex while in the field is not something one can decide clearly upon if one is unaware of the sources of desire and alternative ways of handling those desires.

## Words Melting on the Page: Writing the Erotic into the Text

My own fieldnotes from the firecamp days are peppered with sensual and sexual references:

> 8/16/92 The sounds of helicopters and heavy machinery move in me in a rhythmic way. I wonder what it would be like to make love in a helicopter. One man, one woman, and one engine . . . an intriguing triangle.
>
> 8/19/92 These people live in a world so elemental – in the grip of fickle weather patterns, at the mercy of the wind. Everything is about basic needs here: food, water, sleeping place, work. They work on the edge, reduced to focusing on the moment at hand as they confront fire. It seems to give them a directness and the ability to listen cannily and acutely – like forest animals – that is almost primitive. It's very seductive.
>
> 8/22/92 No one here is distracted by furniture or decor, by superficial tasks or by social niceties. It's pared down and lean, a world where words and acts are measured and channeled directly into the work. The quality of silence is intensely erotic, as if anything you say – any sound you might make – carries with it deep meaning. They listen with the rapt attention of the lover who doesn't want to miss anything, who is tuned to the nuanced. It's almost overwhelming at times.

In attempting to translate the field experience onto the virginal, blank, white page, I am about the self, with few expectations or distractions from the 'outside'.

In the fire camp, my very presence as a solitary female from the 'outside' was provocative, inducing specific sensual verbal and nonverbal responses from certain men. Golde (1986: 6) states that the very accessibility of the woman fieldworker can be considered provocative. In my particular situation, I came and went freely in the field, a fact that was widely registered by those whom I studied. Striding around the grounds, eating meals with the firefighters, I moved in and out of their living and sleeping environment in an obvious way. With so few women on site, I was a highly visible presence. As a result, I became more visible to myself – as a female – over time. Having my gender reflected so consistently by those with whom I came in contact brought me ultimately to a point where I became more aware of myself as a gendered being. This may be one of the most distinct and unnoticed advantages of the fieldwork endeavor: the opportunity to know the self better, particularly in terms of gender considerations, by seeing oneself reflected in the eyes of others.

Steeped in one's own senses, the boundary between self and other might slowly blur at times (see Geertz 1983). And while noticing difference, we might be able to feel – from the innermost part of ourselves – the power of similarity. For while the use of the word 'other' serves to separate us from other people, it is also likely that it separates us, in subtle ways, from ourselves. If it is true that our senses work synesthetically, in what Hiss calls 'simultaneous perception' (1990: 4), is it not possible that we are more connected to those we study than we might sometimes like to admit? Perhaps, by repressing our sensual feelings or sexual urges, we maintain a distance that – in the end – does a disservice to the sensitive work we claim to do as cultural recorders and interpreters.

Perhaps by acknowledging our own feelings and desires, we might actually look at other people and places more objectively, by being able to ferret out our own biases and distortions as we do our work. Hiss suggests that those who attempt to come to know a place should utilize simultaneous perception for just that reason. Bringing all of the senses to the task in this way 'helps us experience our surroundings and our reactions to them, and not just our own thoughts and desires' (1990: 4). Unlike Malinowski, who was forced perhaps to dislike those whom he studied in a sturdy effort to propel himself away from his own overwhelming sensual feelings, we might then be open to interpreting from a more centered, stable vantage point. With the energy conserved by not having to fight our hormones or our fantasies, we could perhaps better grasp an inhabited landscape in a sensual, but more fully accurate, way.

To be permeated by a place – truly to feel its heart beat – is both a gift and the primary fringe benefit of the work that we do. To respond to that place and its inhabitants without holding back, and to represent them fully in the spirit of generosity and abundance, is the way we might return the favor.

On the last day of the fire project, I stand under a large ponderosa pine with two firefighters, protected from the direct glare of the midday sun. They express gratitude that I thought enough about what they do for a living to have given over a chunk of my own time to come and 'check it out'. Thanking them for their feedback, I ask if I might take a picture of the two of them under the tree. 'You know, end of fire – exhausted, but victorious firefighters prepare to return home', I intone in the voice of the television newscaster. Laughing, one of the men amiably throws an arm over his companion's shoulder, preparing to pose. But the other one objects, 'Hey, wait! I'm covered with soot, and my clothes are filthy. Wouldn't you rather get your shot after I clean up, so you can see how handsome I really am?' He sticks an elbow into his buddy's ribs for emphasis.

But the other man shakes his head and, turning to his friend, says this: 'What do you mean? This is how we look out there, man! This is what it's all about – men and machines, sweat and grit. Why wash it off?' Turning to me, his face opening into the grin of the heartbreaker, he adds, 'Hey, Katie, this is the way it really is, right? It's hot and heavy work. But, what the heck, it's kind of sexy, don't you think?' And then, looking me full in the face, he winked. And I winked back.

Outside of the academy, it is a well-known fact that there are those who accuse academic writers of 'linguistic constipation'[21] or of being 'out of touch' with the 'real' world. The phrase 'out of touch' is particularly apt here, as it reflects the sentiment that there are those who cannot 'feel' the writing of academics who stay hidden behind a language form that, by its nature, is inaccessible to many.

This is a double-edged issue, it seems. We need to ask, first of all, whether we want to study a culture in a calculating, solely intellectual way, and then – when we turn to the task of bringing the culture to others – whether those whom we study would want to be represented in that way. Finally, if I dared, I would ask: who would want to read such an account? How many papers and articles have anthropologists read or listened to over the years from which every ounce of sensual and emotional content had previously been bled in the name of credibility?[22] Do we really have to avoid lyrical description, subjectivity, and the personal voice in order to hold our place in the line-up of respected social scientists? Cesara, in a letter to a loved one, articulated her view clearly:

> Deep inside of me something tells me that art and science are one... What happens to a man who sits outside his cage of rats and experiments on them? Does the interaction between these men and rats not affect the scientist? I think it does. I think such a scientist's view of the human condition becomes simplified, often deterministic: in some instances he loses all sense of the individual's wholesomeness, freedom, responsibility, and dignity. (1982: 193)

When it's all said and done, it seems to come down to a question of whether we work and write from the head or from the heart. And it is here where the logical compromise emerges. Is it not possible to forge an amalgam of the two, in which we allow ourselves to be immersed as sensual beings in each phase of the ethnographic endeavor, from project planning to final textual representation, while simultaneously employing our capacities to analyze and to reason? I believe that this is not only realistic, but is a highly beneficial way to conduct the anthropological enterprise. By accepting ourselves as sensual – and yes, sexual – beings, we might harness all of our collective intellectual and sensory capacities for use in both the work itself and the written product created from it.

## Erotics and the Field: Making Love to One's Self

So, where does this leave us? Do we make love in the field or don't we? And, if we do, how far do we go? How can we untangle the web of moral and ethical issues involved and explore, as well, the dissonance between the unspoken rules of the academy and our own personal beliefs and actions? When I say, for instance, that some of the best lovemaking I have yet experienced with my husband took place on my return home between forays into the world of the firefighters, does that trivialize or cheapen what we shared together? I would contend that the opening of my senses, in an environment which was passionately compelling, created a heightened level of awareness in me which served to bring me closer to myself. And it is that connection to self which allowed me to blend the images of smoke and helicopters, hard work and attachment to others, and which I then brought most advantageously to the marital bed.

Looking at field photos from that time, I notice that my face looks radiant, as women (and, sometimes, men) often look when they fall in love or become passionate about their work. My powerful attraction to the firefighters and to the world they dwell in excited me in a way that seems to have attached me more firmly not only to them, but to myself. In the way that the act of masturbation is a self-absorbed act which forms a closed circle between the individual and her or his own body, so the field experience can function in similar fashion. When the anthropologist works with an open heart and mind, allowing the senses to operate freely, an erotic place is created between the anthropologist and the place she or he studies. Undisrupted by the complexities of the 'outside world', the anthropologist has a rare opportunity to learn about the so-called 'Other' by learning to know more evidence of his idiosyncratic behaviour. He did not object to any of this – although his own explanations of these anecdotal incidents were notably more prosaic than the versions which circulated throughout Whalsay – nor of my account of the extremely contentious manner in which he had campaigned thirty years previously for a harbour development, an argument which caused considerable strife within the community and which still evokes painful memories. He made only one objection: to my description of his brief fishing career as 'inglorious', the judgement of it which was certainly made by the many people who had commented about it to me. Far from being inglorious, he said, it had been 'da maist glorious' time of his life.

All the stories elicited by mention of his name are of things 'known' about Henry: 'everybody knew' them, 'Oh, aye, we aa' ken wir Hendry'. Yet, what was known 'about' him was clearly not known *by* him, or was known in a quite different way. Indeed, he would not recognise himself in other people's versions of him. Self-knowledge and social knowledge of persons are incongruent. Anthropologists tend to privilege the second over the first, in spite of their personal experience of the fallacies with which the Other constructs the Self.

Many years ago, at a conference in Scotland, I presented a paper which contrasted strategic modes of public identity among political activists in rural Newfoundland. I contrasted them as, respectively, over- and under-communicating the *bayman* archetype of the Newfoundland outporter: as, on the one hand, emphasising, and on the other as masking the stigmas popularly regarded as inhering in Newfoundlander identity within Canada (Cohen 1975).[1] A member of the audience who had known me slightly ten years earlier during my undergraduate days told me later that the paper was only incidentally about Newfoundlanders: it was obviously an autobiographical statement. I was sufficiently shaken by his observation not to want to pursue the discussion, but assumed that he was referring to my Jewishness, lapsed entirely in religious observance, supposedly betrayed in my committed anti-Zionism, but nevertheless evident in my name. I did not think then that he was correct (nor do I now): I was writing about Newfoundlanders, rather than about myself. However, his comment did persuade me that, as an anthropologist, my concern with identity had its source in my personal experience; more than that, that my construction of identity as 'a problem' or dilemma, though hardly original, was a reflection of my own struggles. It would not be contentious to suggest that many anthropologists are motivated by a personal problematic as well as by mere intellectual curiosity (an issue raised in several contributions to the 1989 ASA Conference, *Anthropology and Autobiography*). Fabian tersely remarks that, 'our past is present in us as a project' (1983: 93).

There is nothing very novel in this. If we accept that anthropology is an essentially interpretive exercise, it must be clear that interpretation cannot begin from a *tabula rasa*. Rather, it must use all the resources of sense-making that are available to us. Further, it would be impractical, tedious and a denigration of our expertise to have to provide an autobiography as the interpretive key to our ethnographies. If we are really saying that the only paths to the Yanomamo or to the Whalsay islanders are through the life-histories and self-analyses of their ethnographers, we clearly call into question the scholarly integrity of the entire ethnographic record. Knowing that Leach was a mathematically-inspired engineer (Leach 1961: 6, 1984: 9–10) may enable me to appreciate some nuance in *Political Systems of Highland Burma* (1954) which I had previously missed; but I do not need to know his background in order either to read his account of the Kachin, nor to make a judgement about its authenticity. By the same token, *Nuer Religion* (Evans-Pritchard 1956) and *The Drums of Affliction* (Turner 1968) must both be comprehensible as accounts of, respectively, Nuer and Ndembu, rather than as records of their authors' conversion to Catholicism. But that is not to say that they can or should be regarded as ethnographic and interpretive documents which somehow stand independently of their authors' religious experiences and convictions.

So, what importance should we impute to the anthropologist's self? Where should it fit into the equation, if at all? It is a commonplace

# 7

# Self-Conscious Anthropology

## *Anthony P. Cohen*

. . . an anthropology that makes an ethnographic problem of itself offers pragmatic insight into the social worlds it examines and to which it belongs. (Herzfeld 1987: x)

### Some Biographical Observations

Many anthropologists with fieldwork experience will recall the uncertainty with which they actually, or mentally, answered the question put to them by people whom they were 'in the field' to study: 'Who are you?'. The uncertainty is composed of a number of factors: What *should* I say? (i.e. what would it be politic to say?) What *can* I say? (i.e. what could I say that would be intelligible? Is there an answer which is at once comprehensive and faithful? Do I even know who I am?). We cope with this aggravated sense of self-consciousness by resorting to all sorts of more or less honourable devices. But the problem should be seen as one of self-instruction as well as of strategy. It ought also (but seldom does) put us on our guard when we reciprocate with the same question. So anxious are we for information that we often fail to see just how perplexing the question can be. Evans-Pritchard famously recalled his Nuerosis in regarding the unforthcoming Nuer as bloody-minded, rather than as stumped (1940: 12–13). I was impressed by the disinclination of Whalsay islanders to offer introductions when we first met. I supposed that, since they all know each other and knew of my existence it did not occur to them that I would not know who they were. After all, on their own territory how could anyone *not* know? When I would enquire of a friend about the identity of a third party to whom I had just spoken, the answer would usually be given in terms of genealogical referents. Genealogy is rarely a neutral account in Whalsay, if anywhere, but is perhaps the most neutral, least complicated answer available. In public discourse in Whalsay, who a person is depends upon *who* is being asked and *by* whom.

In the summer of 1986 I took with me to Whalsay the first draft of the book I was then writing about the community, intending to show it to some of the people who appeared in the book (Cohen 1987). One man who looms large in it is a controversial figure, well known for the single-mindedness and vigour with which he pursues his campaigns. His presence locally is such that he might reasonably be described as 'universally known'. Notwithstanding the regard and affection I have for him, I had tried to write about him 'warts and all', reporting his somewhat ambivalent standing in the estimations of the islanders. I made reference to various anecdotes which are invariably offered locally as

Anthony P. Cohen, "Self-Conscious Anthropology," pp. 221–41 from Judith Okely and Helen Callaway (eds.), *Anthropology and Autobiography* (London: Routledge, 1992). © 1992 Association of Social Anthropologists.

> How does one know one is a Jew . . . ? One can only know it, obviously, by showing it in some way; to sit back in your armchair and know gets you nowhere; it is meaningless. So if you want to know who you are, you have got to show it . . . (1983: 395–6)

In the past we blithely referred to our self-consciousness as fieldworkers. But we have barely begun to plumb the depths of that consciousness, nor of its implications, for anthropological research and writing. For years we were instructed to eliminate it from our processed anthropology. We now acknowledge that self-consciousness is a useful learning device through which we test on ourselves our perceptions of the cultures we study. Moreover, many of us would accept that, by its very nature, ethnography is an ethnographer-focused art. 'The magician', says Hastrup, 'is part of the plot' (1989), a sentiment echoed in many of the papers at the 1989 conference. But how do we avoid writing just about the magician, or so positioning her/him that the trick is obscured?

A further complication is our ingrained and correct fear of ethnocentrism which inhibits us from recognising qualitative similarities between the self of the anthropologist and that of the anthropologist's 'subject'. This would be defensible if it was due only to a determination to avoid constructing others in one's own image. But it is not defensible if it results in the axiomatic denial of such similarity *when this has the consequence of denying to others the complexity which we impute to ourselves*. By and large, we do not regard ourselves as generalisable. Yet, the categorical techniques of our discipline, indeed the conventional definition of our task as the discovery of 'culture', implies our generalisation of others. We have reserved the Self as the province of mysticism, of artistic expression or psychology. Anthropologists' concerns with it have been denigrated as self-indulgence (Friedman 1987) or worse (Sangren 1988), or have been confined to the elaboration of putative cultural theories. Perhaps this is because we do not know how to reconcile the notion of the unique self with a generalisation such as culture. We (anthropologists) have 'method'; they (those whom we purport to observe) have culture. We have strings to our bows besides anthropology; that is, beyond our method there lie our selves (which we may confide to our diaries, even to novels or to poetry). What could lie beyond *their* culture, assuming we exclude biology? It has been a peculiarly inhumane approach to ethnography (cf. Okely 1975) and one for whose exposure the 'reflexivists' and experimental ethnographers can claim some credit.

Let us determine to avoid trying to reinvent the wheel. The purpose of this discussion is not to rehearse the weary old truism that the ethnographer's self intrudes upon the ethnography; nor that social theory should address the relationship of the individual and society – for it, and anthropology, have always done so. Indeed, anthropology has long acknowledged its difficulty with the self. Our present object is to so formulate the problem that we might begin to exploit the intrusive self as an ethnographic resource rather than suffer it as a methodological hindrance. I suggest three ways in which this expedient use of the self may help us. We can use our experience of the complexity of our selves to contain the anthropologist's temptation to generalise and simplify others. We may recognise that selfhood, the sense of personal identity, is not merely contingent or relative, but has a certain absoluteness, or a 'self-driven' element. Finally, with these points in mind, we might make sense of concepts in other cultures which approximate to our notion of 'personhood', but without rendering them egocentrically, ethnocentrically or Eurocentrically, as analogues of our concept of self.

## An Historical Sketch of the Anthropology of the Self

I suggest that we will not be much assisted in this exercise by recent musings of experimental psychologists, nor of some philosophers (while Luhmann is elegantly sceptical about the help available from sociology (1986: 313–14)). The philosophers seem strangely absorbed by the problem of whether a person at different moments may be properly spoken of as the 'same' person (Williams 1973; Swinburne 1984), and with distinguishing among the body, the person and the self. Psychologists of fieldwork experience that we learn a good deal about ourselves while struggling to understand others. This self-discovery does not concern only our hitherto unsuspected resourcefulness, durability and ingenuity; it is also that, by struggling to understand other people's complexities, we are brought face-to-face with our own. Thus, Jean Briggs's 'discovery' of the emotional discipline displayed by Utku Inuit prompts her also into frustration with her own emotional self-indulgence (1970). Southwold's doubts about God and Buddha are re-energised by the confrontations of 'theological' and 'village' Buddhism in Sri Lanka (1983). There is here a transposition of self and other. Indeed, in anthropological discourse we are accustomed to making instrumental use of the Other in our self-discovery. But we have been educated to the contrary view: that using the self to discover the other offends the fundamental canons of anthropological science. How curious that we should have succumbed to this rigid discrimination. Needham argues that it arises from anthropologists' assumptions about psychological universality: that we suppose we can recognise others' states of mind because we assume they must be like our own, or can be linguistically constituted as such (1981: 57, 60). He goes on to castigate such assumptions. However, I would suggest that our dogmatic segregation of Self and Other has had the *contrary* consequence of constituting us (self) as qualitatively *different* from the Other, depicting this qualitative difference in terms of our complexity and uniqueness, and *their* simplicity and generalisability. As Wendy James puts it, we impute to ourselves a 'potent ego', but to them a lack of 'moral personhood' (1988: 143; 1987: 57). This is the discrimination which Hannerz satirises as the 'Great Divide' (1983: 350). We do not avoid egocentricity, ethnocentricity (nor Eurocentricity for that matter (Needham op. cit.: 71; Herzfeld 1987: 7) by supposing we can, or should, neutralise the self until the completion of the day's research work. To the contrary. The inevitable conjunction of self and other has been noted by Stein as one of the processes of 'countertransference' characteristic of medical diagnosis (Stein 1985) where self insinuates itself as an 'explanatory model' (Kleinman 1980). As an anthropologist, I cannot escape myself; nor should I try. In studying others I do not regard myself as merely studying my self; but rather, as using my self to study others.

## Another Biographical Observation

Recently, three of us, schoolfriends since the ages of nine or ten, were talking about the efforts of our ex-headmaster to contact former pupils of the school (which no longer exists) all over the world. It had been a very small, predominantly Jewish school which, after the Second World War, had mostly recruited foreign pupils, particularly from the Middle East. My two friends, both lawyers, were reflecting on the disproportionate number of our former associates who seemed to have achieved professional or financial success, or have risen to positions of prominence in their own countries. They surmised that our Jewishness, that is, our membership of an historically disadvantaged minority group, motivated us to impress ourselves on our host societies, and suggested that this applied to each of us.[2] Again, I disagreed. First, it did not explain the absence of such motivation among many – most? – Jews, nor among other minority ethnic groups. Second, it did not address my awareness of my own motivation since, in most circumstances, I am not conscious of myself as a Jew.

I said as much – as, indeed, I have for as long as I can remember, but my friends demurred, figuratively tapping the sides of their noses: 'we know better'. Do they? I read my own conduct and life in one set of terms; they construct them in another. Are they not doing to me, like the Whalsay islanders do to Henry, what we do as anthropologists (a) to those whom we study, when we subordinate their individuality as members of society X or as bearers of culture Y (cf. Needham op. cit.: 56); and (b) when we insist on reading our colleagues' works biographically as well as, or instead of, ethnographically (Geertz 1988)? Incidentally, our disagreement about me is an interesting refutation of Fortes's contention that self-knowledge is only knowledge if it is externalised:

thou art thou. But if I am I because thou art thou, and thou art thou because I am I, then I am not I and thou art not thou' (Epstein 1978: 1).

The reluctance to address the issue of the essential self may have been the product of a tradition of social theory which, since the late nineteenth century, had treated self-consciousness as an aberration. For Marx, in capitalist society it was a false consciousness, a manifestation of the individual's alienation; for Durkheim, it signified the inadequate subordination of the individual by society. Mauss's concern with the person or self took the form of a cross-cultural review of the degrees of licence afforded by cultures (and their legal and religious institutions) to individuals and individualism. His was still, and not surprisingly, a structural rather than an 'experiential' approach to the self (Mauss 1938; Carrithers *et al.* (eds) 1985). Throughout the traditions of structural functionalism and British structuralism, the individual was analytically consigned to membership of groups and categories, and perceived as refracting their collective conditions and characteristics. This was entirely consistent with a theoretical model in which the parts of society were conceptually identified by reference to their unique functions. In this kind of schema, individuals were regarded as significant as structures in themselves or as related to structure in identifiable ways, a view which is still propounded by Dumont (1986).[3] Even in structural*isme*, individuality is subordinated to the uniformities of cognitive structures. In all these approaches, the individual is depicted as determined by culture, society, psychology or environment, or by a combination of any of these. Hence, individuals were regarded as generalisable. Individuality was thus portrayed as a theoretical problem: as a deviation from a putative norm, it was something to be explained – as wilful deviance, a failure of socialisation, or as the breakdown of the normal constraints of life.

Whatever the particular theoretical variety of anthropological positivism, individuals were displayed as almost incidental. This posture did not change appreciably until we began to recognise 'meaning' as a problem; to see social differentiation *within* cultures in every respect as normal; to push the problem of generalisation to the centre of the methodological stage.

The relationship between this general paradigm shift and the conceptualisation of the individual is obvious. We belatedly recognised 'meaning' as a matter of interpretation, rather than of stipulation, and then also had to revise our view of symbolism accordingly. Not only were symbols acknowledged as saying many things in many voices, but as being *heard* in quite different registers. Hence, the idea of culture integrating its individual members by their sharing of its symbols required some qualification. Further, we came to recognise that this variability of meaning might not be susceptible to ethnographic 'documentation', precisely because the very nature of symbolism locates it, at least partly, in a non-observable realm. Symbols are thus perceived by people through their *individual* (rather than culturally-cloned) consciousnesses. We cannot hope to make sense of *their* perception of symbols – that is, of their 'membership' of society – without acknowledging their individuality; and cannot do that without recognising the character of our own. I see no more of myself in Leach's statement of contingency than I do in my friend's version of me as a replicate of *their* experience as Jews, or than Henry does in his friends' accounts of him as laughably eccentric and seasick. Rabbi Mendel tells me who I am not; but neither he, nor anyone else, can tell me who I am.

## Who Am I?

If I am not necessarily the person that others see, and if I am not necessarily the person who I *imagine* that others see, and if I am not merely the persona whom I present to others (for whatever reason), who am I, and how might I discover the answer? As I suggested at the outset, posing the question is instructive because it alerts us to the enormity of the task we assume in describing other people – and to the immensity of our misrepresentations of them when we treat them categorically ('as typical examples of a genus' (Watson 1989)); or when we confuse them with our and others' *perceptions* of them. I do not suggest that pursue their own semantic puzzles over discriminations between person or self, and 'personality' (not to be confused with Mauss's struggle with *personne*, person*age*, and *moi*); and clearly tend towards relativism: to a view of the self as 'other-directed' (Gergen 1977). In this, they resemble early psychological anthropologists like Hallowell, Lee and Florence Kluckhohn. Lee added a Buberesque ideological dimension to her view of the socialness of the self, arguing that its autonomy can only be realised in a person's 'relatedness' with others, the degree to which such relatedness is achieved being an expression of 'cultural value' (e.g. Lee 1976). Hsu pursued the point, with some sophistication, distinguishing among gross cultural types – Chinese, Japanese and western – on the basis of the extent to which the individual's 'psychosocial homeostasis' (the essential self?) is rooted in relationships of her/his own making, the Chinese being at the minimal extreme, the western at the maximal (e.g. 1985). The spectre at this feast, often curiously unacknowledged by British anthropologists (with Fortes an honourable exception (e.g. 1973)), is G.H. Mead. Mead went beyond a recognition that individuals cannot be regarded as cultural automata, to consider in some detail the question of how the individual symbolises her/himself in interaction, a concern from which the tradition of symbolic interactionism sprang.

Mead distinguished between the 'Me' – the unthinking being, the enduring product of experience – and the 'I', the consciousness of being, the being which, through its competence to symbolise, is capable of behavioural control, precisely because it conceptualises the self. The 'I', the active agency of being, has to be continuously creative to keep the person (including the 'Me') viable, a view of the 'self' which has had recent anthropological echoes (e.g. Heelas 1981a: 13–14; Lock 1981: 32). Much of Mead's work elaborated this creative aspect of the individual, dealing, for example, with the human's unique power to 'manipulate', to intercede, through 'mind', between means and ends; to intervene, say through language, in the process between perception and 'consummation'. It is in this mediating phase that individuality inheres in the form of reflective thinking. Mead's individual, both in its reactive and proactive modes, is permeated by the Other; but, to the extent that reflexivity is retained and nurtured, is not determined by it (see, e.g. Mead 1934).

Such influence as Mead had on anthropology was largely through the interactionists and phenomenologists, surfacing later among theorists of strategy, game and transaction. It seems to have missed most British scholars, at least until after the 'discovery of mind' and the demise of the deterministic paradigms. That Mead is still not routinely taught to British undergraduates is an expression of this historical neglect, but the omission has become even more curious with the accession to our reading lists of contemporary 'reflexive' anthropology. For Mead of course, as, earlier, for Cooley, social interaction is at the very foundation of self-conception: both are accomplished by 'taking the role of the other' – viewing oneself and one's behaviour from what is imagined to be the perspective of an Other, anticipating the Other's reaction. The 'I' component of the self is the analyst of this self-'observation' who modifies or plans behaviour on the basis of the analysis. The conceptual material for the analysis is partially derived from 'culture' (which accounts for the similarities among members of a society); but is mediated through the individual consciousness in ways which reflect cultural theories of the relation of individual to society.

This all sounds rather dated in the context of recent symbolic theory and developments in linguistics and semiotics. However, another curiosity of recent anthropology is that our conceptualisation of the self, the symbol*iser*, has not kept pace with our ever more complex and refined approaches to symbol*ism* itself. In his book on ethnic identity, Epstein (1978) quotes the Leach of a decade earlier sounding even more relativistic than Mead himself: '*I* identify myself with a collective *we* which is then contrasted with some other. What *we* are, or what *other* is will depend upon context' (Epstein 1978: 100, quoting Leach 1967: 34). By contrast, Epstein also quotes Rabbi Mendel of Kotsk – and one could be forgiven for wondering who, of Leach and the Sage, is the modern and who the medieval: 'If I am I, simply because I am I, and thou art thou simply because thou art thou, then I am I and 'us'. When 'I' becomes 'we', one does not necessarily contradict self but, rather limits it. One says, in effect, there are aspects of 'I' which are not relevant to 'we' and which must be, or can be left out of consideration for the moment. The self that is taken into 'we' is a particularistic, but not a contradictory, version of self.

I will now attempt to illustrate these various points with something of a Cook's tour of the comparative ethnographic record. Several critics have called this excursion into question. Quite apart from its presupposition of the comparability of extremely diverse cultures, and the generalisability of any one of them (which I have been to some lengths to criticise), it has been suggested that it privileges anthropological over indigenous knowledge (Rapport: personal communication). Further, ethnographic examples cannot, of themselves, do anything to establish the integrity of my claim for a 'self-driven self', as opposed to a 'society-driven self', which can only be axiomatic (Campbell: personal communication).

My critics' reservations would certainly be justified except, I think, that my reasons for introducing these ethnographic accounts are somewhat different. I wish to suggest that a person's consciousness of self and of social membership are not merely reconcilable, or complementary, but that the second may be built on the first (rather than vice versa). Further, it seems to me that this relationship *cannot* be appreciated without the explicit introjection and use of the anthropologist's self – and that, far from this being a weakness of a particular argument or style of anthropology, it is both the limitation and the strength of anthropology as such.

## 'I' and 'We' as Versions of the Self

The first case is drawn from Hastrup's account of the Icelandic freestate, the period between settlement by the Norse in the ninth and tenth centuries until its full colonisation in the thirteenth century. Hastrup suggests that at first concepts of time and space were ego-centred. People lived in isolation. The measurement of time was material only in relation to the routine observed in the conduct of one's own subsistence activities. Moreover, since its reckoning was based on observations of the sun and moon, it would vary according to vantage point. Concepts of space were likewise related to the unique coordinates of ego's location. Space was demarcated by reference to ego conceptualised as occupying its centre.[5] As the population increased and settlements became denser, social relations obviously assumed greater prominence and the mechanisms of the state proliferated, among them absolute, rather than egocentric, standard measures of time and space. But the one did not simply dislodge the other. The two systems of reckoning coexisted, each prevailing in different spheres. So far as the individual's immediate environment was concerned (say, the farmstead), there was retained a model of space 'as a circular, multi-dimensional area with ego in the centre . . .' (1985: 56). However, when the space in question was beyond the personal 'fixed' domain (for example when reference was to an area which implied the individual moving between fixed points outside his own domain) then it was divided by a scheme based on fixed, objective coordinates reflecting the socio-political sub-division of the country into quarters. Rather than being ego-centred, this model of space was society-centred (66). For so long as the two systems coexisted, the imposition of a social system for the reckoning of space did not especially compromise the former individualistic mode.

A structural-functional reading of this history would see the state displacing the self to the degree at which the individual became a mere basket of social roles or repository of social facts, the kind of picture which Wirth depicted as the fate of social actors on the urban stage (1938). A Meadian perspective would sustain the view of the more inclusive self evident in Hastrup's account. For example, Ralph Turner insists that, 'People are not just miniature reproductions of their societies'. Rather a person's experience of his/her articulation with social structure generates a 'self-conception', that is, a consistent symbolisation of self which runs through all the person's activities (Turner 1976: 989–90). This essential self may be *informed* by social engagement, but is not dependent on it: it is an expression of autonomy rather than of ethnographers should become psychoanalysts, searching for the irreducible essence of a person buried so deeply that the person may be unaware of it: simply, that we should allow the intractability of the problem to discipline our pens. Hywel Lewis described the difficulty eloquently, but succinctly, in his Gifford lectures:

> (If I am asked) what is this 'I' that has these thoughts and this pain, how is it in turn to be described over and above describing the thoughts or the pain, or noting them, what is the self or subject over and above there being the pain etc.?' – I am wholly nonplussed. There is nothing I can begin to say in reply, not because it is exceptionally difficult to give a correct description, but just because there is no description that can be offered. My distinctness, my being me, is quite unmistakable to me, there can be nothing like a rare vase or painting where we can indicate the properties that make it unique, but unique in a final sense of just being itself. (1982: 55)

This irreducibility and elusiveness of our own selves should be an invaluable mnemonic. If we do not do descriptive justice to individuals, it is hard to see how we could do it for societies. If the substance of 'self' is indescribable, and if (as I trust) (*pace* Douglas 1983: 43) we accept that there is no more mileage in trying to use the self to discriminate between types of society, how, then, can we use the self ethnographically? A tentative answer is 'experientially', for this is the only way to avoid the fictional abstractions that inevitably emerge from sociological theories of individualism or of the self, and from taxonomies of society and individualism. How is self-hood experienced?

## Ethnography and Self-Hood

Commenting on this chapter at the ASA Conference, my colleague Ladislav Holy remarked that, in practical terms, only *social* knowledge is accessible to us; and asked whether people's self-knowledge should be an issue for anthropology if it is not available to the ethnographer. The ethnographer can only guess at it with the experience of her/his own self-knowledge (which, in itself, is relational in source and nature, especially when the 'discovery' of the anthropologist's self is so heavily influenced by fieldwork and literary encounters with the Other). In this respect, he observed, anthropological accounts are necessarily reflexive and autobiographical.[4] But, he concluded, this suggests the danger, all too manifest in contemporary anthropology, of 'too much self, too little other'.

My response to these cogent arguments is that, although people's self-knowledge is not easily available to the ethnographer, anthropology cannot continue to be written as if it does not exist, or is immaterial, or, even, is less important than 'social' knowledge. People's knowledge of themselves is of *critical* importance to us for without it we misunderstand them. Its availability does certainly present us with profound methodological difficulties, for which we may have only the very imperfect device of our own experience – and here I hasten to distance myself from any suggestion that anthropology should be 'about the anthropologist's self': rather, it must be *informed* by it.

It is with such self-experience in mind that I argue that anthropology may have exhausted the usefulness of contingency and relativism as means of revealing theoretically how people experience self-hood. Relativism has spanned diverse theoretical traditions in anthropology and is based on views of the individual as plastic, as capable of reformulation by society through various processes of socialisation and initiation. Some exponents of such views defend them with the assertion that in the societies with which they are concerned there is no 'asocial' concept of self and, therefore, no experience of self-apart-from-society (e.g. Lienhardt 1985). John Mbiti remarks, 'I am because we are, and because we are therefore I am' (1970: 141). I am not convinced. My sense of self does not only become crucial when I experience contradictory social demands made of me; or social constructions made of me from which I dissent. It is to be found also in my solitary, Cartesian soliloquy; in my experience of personal space; in the increasing proficiency with which I learn the use of the concept which mediates between the absoluteness and the contingency of self:

game and meat, a contradiction resolved by the ritual purification of adults, thus polluted, by uncontaminated children. There is the contrast between the ideal of peace, *ekimi*, and noise or crisis, *akami*, resolved through the mediation of youths bringing the crisis out into the open and music-ing it away with the sacred *molimo-made*.

The metaphor of reflections and reconciliation might itself be metaphorised as weights on either side of a scales. If they are unresolved, one pan will outweigh the other. The ideal of resolution is to bring them into balance. Being out of balance, *waziwazi*, does not imply movement from the individualistic to the collective modes of self. It just means being disoriented. When the Mbuti refers to the 'real self', he/she can have in mind both the individual and the member of the collectivity. In this view, then, the mere fact of sociality does not compromise the idea of self.

## Society, Self, Other

Why might it be that the Mbuti seem to have achieved a reconciliation of self and society which has eluded most of us? The answer might be that they accord greater value to the self than to the social: the individual is not permanently tied to kin or hunting groups, and enjoys a mobility which suggests an avoidance of the social impingement of self (and, incidentally, provides a marked contrast to the individualism which Riviere describes among potentially mobile Guianan peoples (1984: 94 ff)). But this view bears all the hallmarks of that 'society-driven' view of the individual which has dominated anthropology and which still directs us to limit our interpretation of 'inner states' to what can be documented in terms of 'social facts' (cf. Needham op. cit.) or of generalised cultural models of the constitution of personhood (e.g. White and Kirkpatrick 1985; Heelas 1981b). If, instead, we were to allow the possibility of the *self*-driven individual the Mbuti case would not look so discrepant.

The society-driven view sees the self being tugged in different directions by the competing claims and allegiances of the individual's social ties, each of which entails a role with appropriate script. But for too long anthropology has simply tended to accept the social psychologists' axiom that we *are* so subordinated – indeed, that we may even connive at such self-subordination strategically and tactically: that is, we attempt to make ourselves appear as we believe others would wish them to, or in ways in which we would wish others to perceive them. In this tradition, the self confronted by society is merely Performance. What lies beneath the script and the make-up is indiscernible. This kind of analysis threatens to eliminate the self as a real entity altogether. For example, in his book, *The Tactical Uses of Passion*, Bailey analyses displays of emotion, the very antitheses of self-control, in terms of their tactical potency (1983). It is a theory of self with no null-hypothesis – just as, in his earlier exhibition of political masks, he precludes logically the possibility of a person claiming truthfully not to be masked (1977). This may be an especially explicit attack on the saliency of self-motivation, but it is not much more exaggerated than the mainstream social anthropology which persists with the use of general categories from which the self is excluded on the grounds of irrelevancy or methodological inaccessibility or is treated as culturally defined.

The contrary, self-driven view may be less exciting theoretically, but may be closer to our experience. It is certainly true that we suffer from a contemporary idiom which assumes our passive conformity and complicity, which treats us as reflecting, even parroting the social influences which are brought to bear on us. 'The self', says Lock, 'is constituted by culture' (1981: 22). By extension, this would suggest that we wear designer-selves by courtesy of the advertising copy-writers and market manipulators; indeed, even allow the 'rights' which we rhetorically express as inalienable from ourselves, to be defined, and increasingly curtailed by the sophistry of our political leaders.

This view of the self-as-clone should not satisfy us. Our own theoretical approaches to symbolism and meaning contradict it. We do not merely ingest stimuli, whether these are symbols or directives. We *interpret* them; *we* make sense of them, and the stimulus does not dictate what sense we shall make of it. Of course those who direct them at us try to contingency. Consider this lesson in self-hood administered to Turnbull by an Mbuti on the banks of the River Lelo.

## The Reality of Self

> Stand at the edge of the water, I was told, and look at your reflection. Who is it? It looks like you, but its head is down there, looking up at the other you. Is it thinking the same thing, wondering who you are? Then put out your foot, over the water, and gently lower it. The other foot will come up to meet yours, and if you are very careful (not to break the surface of the water), you will feel that other foot touch yours. You are getting to know your other self. Then as you lower your foot further into the water the other foot comes up, passes through your foot, and disappears into your leg. The deeper you go into the water the more of your other self enters into you. Just before you go right down into that other world, look down, and see yourself down there, all but your head. Only your other self's head is there. And then look upward as you go right under the surface, and you see nothing. Your other self has passed into the world you left behind, taking your place. Now walk across the bottom of the river, and slowly come out on the far side. If you look up from under the water you will see nobody, just the forest, but as you emerge into that world something will leave you, passing through your body down into the water. Now who is the real self, and which is the real world? (Turnbull 1983: 122)

It is possible to read into this probing of the apparent a number of themes which are prominent in Mbuti culture. The one to which I would draw attention here is a notion of balance. Not only does this seem to embrace many features of Mbuti life, but also expresses a resolution of the dialectic of self and society. The Mbuti live nomadically within the Ituri forest, speaking a wide variety of languages and revealing a plethora of extraneous cultural influences. They live in and on the forest, have a very fluid kin structure, moving from band to band, and from place to place, as they judge expedient. Around them, on the edge of the forest, live Mbira villagers who contrast with them in most respects: they are tribal, sedentary, non-pygmy, cultivators, who are incompetent in the forest, depend on the Mbuti to supply them with its produce, but play the roles of patron to the Mbuti's client. The villagers are *alter* to the Mbuti *ego*.

Here we have the Mbuti, contemplating his reflection in the river and talking about his two selves. Who or what might they be? They could be a metaphorical statement of this curious opposition between the forest nomad and sedentary villager, but this is improbable: ethnic encounters do not often seem to be conceptualised in terms of mirror images – at least, not unless we think in terms of distorting mirrors. Rather, the very idea of the reflection of two images may be a paradigm for the reconciliation of contradictory themes which are perceived as inherent in the human social condition, among which is the opposition between self and society. So far as the Mbuti are specifically concerned, the existence of oppositions and of their resolution is a constant refrain in the culture (Turnbull 1965). There is the obvious distinction between the Mbuti and the villagers: distinct tribally and ethnically; in terms of modes of subsistence and social organisation; physically and topographically different; believing in different gods and spirits. Yet, they live in a degree of symbiosis, the Mbuti supplying the villagers with forest foods (roots, berries, meat and honey) and building materials: the villagers, probably unwittingly, certainly unwillingly, providing the Mbuti with lootable cultivated foodstuffs. Their 'contra-definition' is reconciled through the great circumcision festival of *Nkumbi*, held approximately every three years, through which many of the locally-dwelling peoples are federated.

There are a host of other oppositions: for example, between biological and classificatory kinship. The latter suggests a pronounced egalitarianism, all the women of one's mother's generation being one's mother; all the children of your child's generation being your children. The equality is ruptured by marriage but this contradiction is itself at least partially resolved, by the licence to resume unrestricted sexual relations during the three years following the birth of a child. There is the contradiction between the ideals of non-aggression, of passivity, of freedom and the sanctity of life; and, the necessity for adults to hunt and kill chology, but in a quite impersonal way: really, they map out explanatory space within which the individual can find a unique niche. They leave intact the individual's essential moral autonomy and self-motivation. The individual is 'singular', possessed of a personal Genius (88): 'The human being is the creature of no ruling god, no inner passion, nor are persons mere puppets of an external order' (91; and cf. Beattie 1980: 313–14). Individuals are mostly free to contrive their own relationships (94). All of this is consistent with the idea of *arum* as the driving force, for *arum* is 'inside' the person (133) and, even having in mind Needham's strictures regarding the translation of psychological states, might reasonably be viewed as an approximation to the concept 'self'.

This requires some qualification, for *arum* has multiple meanings: spirit, or ghost (8) or spiritual power (11), 'timeless things of myth' and pre-civilised people (142); animating personal force (7) or 'vital being' (100). *Arum* is certainly not simply assimilable to the concept of self, not only because of the multi-referential nature of the term, but because one could not properly speak of a person as being in control of *arum* in the way in which we talk about 'self-control'. *Arum* is force, vital essence, and therefore cannot be forced. But we do not need to suggest that *arum* and 'self' are equivalents. The beauty of James's exposition is that, without succumbing to the temptations of translation, she shows how the non-equivalent concepts of *arum* and self nevertheless inform our understanding of each. Our own experience of the difficulty of encapsulating the concept of self, not to mention the confusion which arises from the plethora of theories of the self, helps us to an appreciation of the elusiveness of *arum* – and vice-versa. This is an intellectual exercise, but one built on personal, subjective experience. That is to say, it is a different kind of interpretation than might be involved in the juggling of mathematical formulae, or the documentation of 'social facts'. It is largely the product of introspection, of a scrutiny of the self as a '... touchstone for understanding the world of others ..." (James 1988: 144). Hence her conclusion, and one which I share, that 'Self-knowledge is intimately linked with the possibility of understanding others ..." (156).

What, then, of the difficulty with which we began, of the inaccessibility of 'inner states' or self-knowledge? This is not just a problem of eliciting 'indigenous psychology', but arises whenever we impute a state and product of mind to other people (within or across cultural boundaries). It is evoked by questions of 'symbolism', of 'meaning', of 'interpretation', of intention and so forth. To declare these out of bounds because of the difficulties of conceptual or verbal equivalence would be to paralyse anthropology. It would be the academic 'equivalent' of retreating from society because your closest associates interpret your own behaviour and biography in ways which differ from yours. There is no option for us as social members or as social anthropologists but to proceed from the premise of self. It does not have to be a flabby procedure. Its virtue lies in more than its logical inevitability: it also replicates the process of ordinary interaction, of our lay assumptions that we have understood each other, that we have achieved 'intersubjectivity'. '... [E]very version of an other', says Clifford, 'wherever found, is also the construction of a "self" ...' (1986: 23). We have long recognised this as a characteristic of social life. It has taken us longer to recognise it as a necessary condition of anthropology. Now we should celebrate it as our most potent interpretive resource.

contrive in us particular interpretations, or attempt to limit the kinds of interpretation we might make. But there is evidence to suggest that we are still left with plenty of interpretive (or misinterpretive) space. The 'self-driven' view does not deny the reality of the pressure exerted on individuals to conform to normative role definitions, nor that many, even most people do succumb. There is no suggestion here of the individual compulsively fighting a war of resistance against society. But *experientially* most of us would feel ourselves to be in control – that is, in control of our selves. Indeed, the contrary feeling is defined as pathological and may be diagnosed as 'a breakdown'. Nor should we dismiss the claim to self-control as self-delusion for, in organising our time and space, our social relationships, our self-definition and presentation, we are struggling to keep ourselves in balance, to keep ourselves, as the Mbuti would have it, in the centres of our spheres. In this struggle, the self speaks in the active voice as 'idealist' rather than as *passiones* (see Douglas 1982; Heelas 1981b: 39 ff). Fortes might not have wished to go quite as far as my own overstated suggestion, but he nevertheless points us in the direction: 'The individual is not a passive bearer of personhood; he must appropriate the qualities and capacities, and the norms governing its expression to himself' (1973: 287). The public expression or presentation of an identity may be very different from its private experience. It is not merely a matter of making the implicit explicit (to borrow a phrase from Crick (1989)); nor, as Hastrup laments, of turning oneself 'inside out' (1989). Marilyn Strathern has recently showed that this assumption is one of the curses of our time (1989). Public identity is a transformation of the self, not an equivalent expression of it. To mistake the two is to make a fundamental error about self-perception. So far as anthropology is concerned, the active self is present again (and again) when the selves of the anthropologist ('personal' and 'anthropological') contrive an interpretation of what is said and heard in the field (Rosaldo 1980: 233; Cohen 1987: 94; 1989: 47–9.)

This does not dispute the existence of cultural theories of the self, nor of indigenous theories both of the constitution of the individual and of personhood. Rather, it complements them. For example, take the idea, reported both in Morocco and Andalucia, that a person's moral credibility (and, therefore, social visibility) is based less upon identity (who the person *is*), than upon acknowledged moral accomplishment – or, at least, conformity with an ideology of accomplishment. Writing about Sefrou, Morocco, Rosen observes,

> It is world – and hence a self – in which people are known by their situated obligations and by the impact their actions have on the entire chain of obligations by which they and their society are known. Human beings do not create themselves but they do place themselves in those contexts . . . (1984: 179)

Pitt-Rivers describes reputation in Alcalá de la Sierra in similar terms (1972). Notwithstanding the putative doctrine of Moroccan Islam, Rosen subtly shows that self-hood, like most other variable things in Sefrou life, *is* negotiable and, therefore, *is* created by the power and ingenuity with which the individual deploys and exploits available resources.

There may be tensions between the society's dominant ideology of personhood and the individual's concept of self. There are societies which apparently attempt to impose complete control over the definition and experience of self: Goffman's asylum and Campbell's Sarakatsani (1964) (at least with respect to inmarrying brides) are examples. Goffman himself, like other 'total institutions' theorists (1964) (but not Campbell), acknowledges the difference between imposed role behaviour and self-perception, a distinction manifested in the resilience of self both in 'total institutions' and, *pace* Campbell, in rural Greek marriages (Du Boulay 1974). There are societies in which these tensions are resolved, but without sanctions; Hastrup's medieval Iceland (above) is a case in point. And there are societies which theorise the constitution of personhood, but in a way which may be tantamount merely to providing terms of reference for the self, while *appearing* to do rather more. That is the sense with which I read Wendy James's account of Uduk personhood (1988). The characteristics attributed to stomach and liver constitute physiological explanations of personal psy-

# Part III

# Fieldwork Relations and Rapport

*Jeffrey A. Sluka*

The success of ethnographic fieldwork is in large measure determined by the ability to establish good rapport and develop meaningful relations with research participants. When fieldwork fails, it is generally due to a failure to either establish rapport and good relations or maintain them over time. These relations range from friendship to hostility, and may be influenced by ethnicity, religion, class, gender, age, and whether the field researcher is alone or accompanied by family.

One of the main ways cultural anthropologists have established fieldwork relations is by the development of strong bonds of friendship with particular individuals who often become both key informants and research assistants. Making a friend has proven to be one of the main ways that anthropologists have been able to establish rapport during the early part of fieldwork, and Oberg (1974) has suggested that it is one of the best ways of countering culture shock. This idea is well established in the discipline, and was the central idea expressed in the title of Powdermaker's classic study of her fieldwork *Stranger and Friend* (1966). She believed that "Whatever the degree of the fieldworker's participation in the whole society, friendships with a few people develop, and they help him to find a niche in the community. It is these friends who often become his best informants" (1966:420).

In the 1990s, reflection on the nature and quality of field relations between researchers and informants grew, and the theme of fieldwork and friendship was explored in a number of important publications, including Beverley Chiñas' *La Zandunga: Of Fieldwork and Friendship in Southern Mexico* (1992), Nita Kumar's *Friends, Brothers, and Informants: Fieldwork Memoirs of Banaras* (1992), and Bruce Grindal and Frank Salamone's edited volume *Bridges to Humanity: Narratives on Fieldwork and Friendship* (1995). In the second half of the 1990s, this interest in fieldwork relations began to extend to consideration of the question of sexual relationships in the field. In particular, this was explored in two edited volumes, Don Kulick and Margaret Willson's *Taboo: Sex, Identity, and Erotic Subjectivity in Anthropological Fieldwork* (1995) and Fran Markowitz and Michael Ashkenazi's *Sex, Sexuality, and the Anthropologist* (1999). Sex as part of participant observation is both a methodological and ethical issue. Traditionally, there was a strong taboo against ethnographers engaging in sex with research participants, because it was believed that it represented an unfair use of their position of power and an

"Champukwi of the Village of the Tapirs" (1960), Gerald Berreman's "Behind Many Masks: Ethnography and Impression Management" (1972), and Antonius Robben's "Ethnographic Seduction, Transference, and Resistance in Dialogues About Terror and Violence in Argentina" (1996).

Published in 1960, Wagley's account of his fieldwork in 1939 in a small village of about 175 Tapirapé Indians in Brazil comes from the first volume dedicated to exploring field relationships between anthropologists and key informants (Casagrande 1960). He chose his relationship with Champukwi for this article because he "became my best informant, and after a time, an inseparable companion" (1960:400), and "for the brief span of about a year, he was my most intimate friend" (1960:398). But their close association began to cause trouble, and they had a falling-out. Champukwi was an inveterate Lothario who gave his paramours beads which Wagley had given him. This caused stress between him and Wagley because "as people continued to criticize Champukwi, much of their criticism [revolved] around his relationship with me" (1960:408). Nonetheless, their relationship continued, and Champukwi gradually evolved from being not only a friend and anthropological informant but also a field assistant. Wagley reflects that:

> In the security of our studies and in the classroom, we claim that anthropology is a social science ... But, at its source, in the midst of the people with whom the anthropologist lives and works, field research involves the practice of an art in which emotions, subjective attitudes and reactions, and undoubtedly subconscious motivations participate ... Anthropological field research is a profoundly human endeavor ... the anthropologist almost inevitably is involved in a complex set of human relations among another people ... and each anthropologist is a distinctive personality and each undoubtedly handles in his own way his dual role as a sympathetic friend to key informants and as scientific observer of a society and culture which is not his own. (1960:414–15)

Wagley concludes with a deeply personal reflection on his relationship with this "key informant," which expresses the fundamentally human nature of the face-to-face encounter in ethnographic fieldwork: "To me, Champukwi was, above all, a friend whom I shall remember always with warm affection" (1960:415).

Gerald Berreman's "Behind Many Masks" was published as the prologue to his outstanding ethnography *Hindus of the Himalayas* (1972). In the Preface to the second edition, he states:

> I believe firmly that ethnography cannot be understood independently of the experience which produces it. Just as I felt obliged to be thorough and candid in presenting my research findings, so I have felt obliged to be thorough and candid about how I did that research. If fault be found with the former, it is the result of the latter. Written from an "interactionist" perspective, "Behind Many Masks" emphasizes the problems for research generated by the conflicting interests of the various castes and by their divergent culture and lifestyles, because even in small and isolated Sirkanda, social heterogeneity loomed as a major obstacle to my rapport and my understanding, just as it does to the people's relationships with one another. (1972:vi–vii)

Berreman presents a case study and analysis of "some of the problems and consequences inherent in the interaction of ethnographer and subjects," particularly the inappropriate practice on other grounds. However, intimacy helps break down the distinction between "self" and "other" and the cultural boundaries that obscure the common humanity of researchers and their informants, and consensual sex between adults can be seen not as a power relationship but rather its opposite – the breaking down of hierarchy and establishment of equality between them.

The classic image of successful rapport and good fieldwork relations in cultural anthropology is that of the ethnographer who has been "adopted" or named by the tribe or people he or she studies (see Kan 2001), and less successful fieldwork relations were rarely discussed until the reflexive trend began to emerge in the 1970s. One of the first and best discussions of less than ideal field relations was presented by Jean Briggs (1977) in her honest account of her fieldwork in 1963 with an Eskimo (Inuit) community in the Canadian Arctic. Briggs was "adopted" by the tribe, but failed to maintain rapport with them. In order to achieve acceptance, anthropologists must assume a role that is defined as believable and non-threatening in the eyes of the people they wish to do research with. Briggs found acceptance in the form of fictive kinship, by playing the role of a daughter. While this was a classic means of achieving rapport and a working relationship with people, this case shows that it is not always ideal, and even fictive kinship roles can break down under the stress of relations between researchers and the "others" they work with.

In practice, Briggs found it impossible to reconcile the two roles and "be simultaneously a docile and helpful daughter and a conscientious anthropologist" (1977:65). She became insubordinate, disobedient, obstinate, and grumpy, expressed her anger, did not always help with the chores, and sometimes refused requests to share her possessions – pretty much violating most of the fundamental values of Inuit culture. Not surprisingly, the tribe found her behavior "completely incomprehensible" (1977:65). She grew "annoyed and frustrated" (1977:67) with them, and they with her, and their relationship erupted into open conflict. She went from being treated as a slow child to being treated as "an incorrigible offender, who had unfortunately to be endured but who could not be incorporated into the social life of the group" (1977:72). After several months of "uneasiness," the Inuit began to avoid her, and expressed their wish that she leave. Eventually, however, she was able to satisfactorily explain her behavior and achieved a rapprochement with them. Briggs concluded: "Although an anthropologist must have a recognized role or roles in order to make it possible to interact with him sensibly and predictably, nevertheless it will be evident from what I have described of my own case that the assignment of a role may create as many problems as it solves for both the anthropologist and his hosts" (1977:78).

Similarly, William and Margaret Rodman (1990) suffered a loss of rapport in their relationship with their research participants. While conducting fieldwork on the small South Pacific island of Ambae in 1985, Margaret contracted malaria and nearly died. The tribe insisted that her illness was caused by a *wande*, or river spirit. In a "failure to believe" the Rodmans chose not to rely on the local cures their research participants insisted they use, and instead went to a hospital. This caused a falling-out with the tribe, particularly the chief they were living with. They lost rapport and had to leave the field for health reasons, and thus ended their fieldwork on Ambae.

In this part of the reader, we present three articles which illustrate and explore important dimensions of fieldwork relations and rapport – Charles Wagley's

Robben was already sensitive to the risks to objectivity in doing research with victims of state terror, aware that it is easy in face-to-face research to be "seduced" by their obvious emotion and suffering. But he did not anticipate that this would be a problem when "studying up" state terrorists. He found that it was. The general and other military officers treated him well and with respect, and stressed their class affinities with him (well educated, elite, bourgeois): "Although my initial sympathy and tolerance of personal idiosyncrasies was greater with members of human rights organizations than with the armed forces, I soon met officers whose politics I detested but for whom I felt a personal liking" (1996:99–100).

Once sensitized to this, Robben then found that he also recognized it in his subsequent meetings with bishops, human rights activists, and former guerrilla leaders, and that "each group was seductive in its own way" (1995:83). He observes that "It is much easier to acknowledge manipulation by victimizers than by victims. We have more sympathy for unmasking abuses of power than doubting the words of their victims. I have the same sympathies" (1995:84). Robben argues that seduction "disarms our critical detachment" (1996:86), and that "victims may be harmed and their testimonies discredited if we report their views naively and uncritically" (1995:84).

Robben's article not only exemplifies some of the problems of establishing rapport and fieldwork relations in contexts of political violence, state repression, and popular resistance, but also the expanding venues of fieldwork into war zones and sites of violent conflict (cf. Nordstrom and Robben 1995), where there are major divisions between the parties to the conflict, and it is almost impossible to establish and subsequently maintain research relationships with both at the same time.

differential effects of his identification with high- and low-status groups in the village (1972:xix).

As a result of the fact that Hindu villages in India are divided within themselves along caste, religious, and other lines, "acceptance by one element of the community does not imply acceptance by the whole community and it frequently, in fact, precludes it" (1972:xxii). Berreman was not "adopted" by the locals, but he describes how he was able to establish acceptance and rapport in the village, and is candid about the nature of this relationship:

> Although I remained an alien and was never made to feel that my presence in the village was actively desired by most of its members, I was thereafter tolerated with considerable indulgence. I became established as a resident of Sirkanda, albeit a peculiar one, and no one tried to get me to leave. I have heard strangers ... inquire of Sirkanda villagers as to my identity ... and be left to ponder the succinct reply, "He lives here." (1972:xxvii)

At different times during his fieldwork, Berreman employed two interpreter assistants – a Brahmin who became ill four months into the research and a Muslim who replaced him. Berreman applies Erving Goffman's interactionist model of "impression management" to reflect on the implications for his research of the differences in status of these two assistants. Pronounced stratification in the village meant that there were many competing groups or "teams," including the ethnographer and his research assistants, the village as a whole, low-caste and low-status villagers, and high-caste villagers. Berreman found that his interaction with these teams was different and the use of the two assistants had different effects when working with each team.

Today, the communities with which cultural anthropologists do research are nearly always stratified, plural, and internally divided, and relations have to be maintained with different factions and interest groups who may be in conflict or competition with each other. Establishing acceptance and working relationships with both a community as a whole and the various factions within it is frequently a difficult goal to achieve.

This reality, of the competing interests of research participants, is also apparent in Antonius Robben's article "Ethnographic Seduction, Transference, and Resistance in Dialogues about Terror and Violence in Argentina." Usually, cultural anthropologists do fieldwork with people they like, respect, or even admire, but Robben raises the issue of establishing research relationships with people you do not like and whom you may even consider to be morally reprehensible. As a result of his fieldwork with both victims and perpetrators of violence during the "dirty war" in Argentina, Robben warns of the dangers of "ethnographic seduction," which he defines as "a complex dynamic of conscious moves and unconscious defenses that may arise in interviews with victims and perpetrators of violence" (1996:72) and which undermine critical detachment (see also Robben 1995). Few anthropologists have had his experience: "There were days when I talked in the morning to a victim of political persecution and in the afternoon with a military officer who had been responsible for the repression. These days were stressful because they demanded radical swings in empathetic understanding" (1996:97).

# 8

# Champukwi of the Village of the Tapirs

## *Charles Wagley*

Champukwi was not the first person who came to mind when a contribution to this volume was considered. I thought of Gregorio Martin, a dignified and wise old Mayan Indian of Santiago Chimaltenango in Guatemala, who in 1937 had taught me the way of life of his people. I thought of Camirang, the dynamic young chieftain whom I had known in 1941 in a village of Tenetehara Indians along the Pindaré River in northeastern Brazil. I thought also of Nhunduca, a gifted and witty storyteller from a small Amazon community, who in 1948 introduced me to the rich folklore of the Amazon *caboclo* or peasant. But then, among all the people I had known in the various primitive and peasant cultures in which I have done ethnological research, I chose Champukwi, a man of no outstanding talent, yet talented all the same – a man of not the highest prestige in his society, yet admired by all. For the brief span of about a year he was my most intimate friend.

I knew Champukwi some 20 years ago when I lived in his small village of about 175 Tapirapé Indians in central Brazil. I must have seen him at once, for presents were distributed to the whole population on the day of my arrival in late April of 1939. But I did not distinguish Champukwi as an individual, nor did he, at first, stand out in any way from the other men of his village. His name does not appear in the notes taken during my first month among the Tapirapé.

For me, and even more for Valentim Gomes, the Brazilian frontiersman who was my companion and employee, the first weeks in the Village of the Tapirs, as the small settlement was known, were a period of grappling with a strange and often confusing world. The Tapirapé Indians lived between the Araguaia and Xingú Rivers, an area at that time almost entirely isolated from modern Brazil. They had been visited by only a few people from the "outside" – by one or two missionaries; by Herbert Baldus, a German-Brazilian anthropologist; and by a few frontiersmen from the Araguaia River. The nearest Brazilian settlement to the Village of the Tapirs was Furo de Pedra, a town of 400–500 persons that lay some 300 miles away on the Araguaia River. Three Tapirapé youths had spent a few months at mission stations and thus spoke a rudimentary form of Portuguese, using a vocabulary limited to a few basic nouns and verbs. At first our main problem was communication, but these youths were able to help us. Aside from them, the only individuals we knew by name during the first two weeks were the "captains," the older men who were the heads of the six

Charles Wagley, 1960, "Champukwi of the Village of the Tapirs," pp. 398–415 from Joseph B. Casagrande (ed.), *In the Company of Man: Twenty Portraits of Anthropological Informants* (New York: Harper & Row, 1960). Copyright © 1960 by Joseph B. Casagrande.

judiciously from time to time. After a few days, I began noting questions to be asked of Champukwi in the late afternoon when he now habitually visited our house. But this was the time that others also liked to visit. At this hour of the day our house was often crowded with men, women, children, and even pets – monkeys, parrots, and wild pigs – for which the Tapirapé along with other Brazilian tribes have an especial fondness. Such social gatherings were hardly conducive to the ethnological interview or even to the systematic recording of vocabulary. So I asked Champukwi if I might go with him to his garden. There, alternating between helping him cut brush from his garden site and sitting in the shade, I was able to conduct a kind of haphazard interview. Often, while he worked, I formulated questions in my halting Tapirapé and I was able by repetition to understand his answers. Although the Tapirapé villagers began to joke of Champukwi's new garden site as belonging to the two of us, these days were very valuable for my research.

Walking through the forest to and from Champukwi's garden, we often hunted for *jacu*, a large forest fowl rather like a chicken. I attemped to teach Champukwi how to use my .22 rifle, but he had difficulty understanding the gunsights and missed continually. He attempted to show me how to "see" the *jacu* hidden in the thick branches of the trees, but I seldom caught sight of the birds until they had flown. Thus, our complementary incapacities combined to make our hunting in the tropical forest quite unproductive, and in disgust Champukwi often resorted to his bow and arrow. Only later in the year, after he had practiced a great deal by shooting at tin cans did Champukwi master the use of the rifle, and this new-found skill greatly added to his prestige among the Tapirapé.

My abiding friendship with Champukwi perhaps really began when I came down with malaria about six weeks after our arrival. During the first days of my illness, I was oblivious to my surroundings. I am told that while one *panché*, or medicine man, predicted my death, another tried to cure me by massage, by blowing tobacco smoke over my body, and by attempting to suck out the "object" that was causing the fever. Evidently his efforts – plus the atabrine tablets administered by Valentim Gomes – were successful, for my fever abated. I realized, however, that convalescence would be slow. Unable to leave the house for almost three weeks, I spent my days and evenings suspended in a large Brazilian hammock. In this state of enervation, I must have been the very picture of the languid white man in the tropics. Each late afternoon our house became a gathering place for the Tapirapé villagers, who came not only to visit with me (communication was still difficult) but also with each other, and to gaze upon the belongings of the *tori* (non-Indian). My illness proved to be a boon for ethnographic research. People were more patient with the sick anthropologist than with the well one. They told stories, not only for my benefit, but also to entertain each other. In attempting to explain to me about a mythological culture hero, a man would find himself telling a myth to the attending audience. Thus, I heard (and saw) Tapirapé stories told as they should be – as dramatic forms spoken with vivacity and replete with mimicry of the animals that are so often characters in these folktales. With my still imperfect knowledge of Tapirapé I inevitably lost the thread of the story and it had to be retold to me more slowly.

Champukwi was a frequent visitor during these days of my convalescence. He came each morning on the way to his garden and he became accustomed to drinking morning coffee with us. And, each late afternoon after he had returned from his garden, he came "to talk" – often slowly retelling the stories and incidents that I had difficulty understanding the evening before. Several days during this period he did not work in his garden but sat for two or three hours talking. He learned when he should pause or repeat a phrase or sentence in order that I might take notes. He came to understand what writing meant, discovering that what I wrote in my notebook I could repeat to him later. In time he appreciated the fact that I was not so much interested in learning the Tapirapé language as I was in comprehending the Tapirapé way of life. As so often is the case when a person understands and speaks a foreign language poorly, one communicates best with but a single person who is accustomed to one's mistakes and one's large haypile-like houses arranged in a circular village pattern. These, we later learned, were each occupied by a matrilocal extended family. But even the personal names – such as Oprunxui, Wantanamu, Kamanare, Mariapawungo, Okané, and the like – were then hard to remember, let alone pronounce.

During the first weeks in the Village of Tapirs, I began to study intensively the Tapirapé language, a language belonging to the widespread Tupí-Guaraní stock. Until I could use this language at least passably, I was limited to observing and recording only those forms of Tapirapé culture that the eye could see. Even these usually needed explaining. I visited the extensive Tapirapé gardens in which manioc, beans, peanuts, cotton, and other native American crops were grown. I watched the women fabricate flour from both poisonous and "sweet" varieties of manioc, and make pots out of clay. I watched the men weave baskets out of palm fiber and manufacture their bows and arrows as they sat in hammocks in the large palmthatched structure in the center of the village circle. This building was obviously the men's club, for no women ever entered. I rapidly became accustomed to nudity. The women wore nothing at all, and the men only a palm fiber band around the prepuce. But even nude women could be modestly seated, and the men were careful never to remove thir palm band to expose the glans penis. Obvious also to the uninstructed eye was the fact that the Tapirapé expressed their personal vanity in the elaborate designs carefully painted on their bodies with *rucu* (red) and *genipa* (black). These and many other overt aspects of Tapirapé culture could be recorded in notes and photographs while I studied their language.

The Tapirapé, a friendly and humorous people, seemed rather pleased with the curious strangers in their midst. They found our antics amusing; the gales of laughter that accompanied the conversations that we could hear but not understand seemed evoked by tales of our strange behavior. (It is so easy to presume that oneself is the subject of conversation when listening to a strange language.) Then, of course, our presence was materially valuable – for the salt, knives, needles, beads, mirrors, and other presents we brought were greatly appreciated. However, within a very short time some of these people began to emerge as individuals. Awanchowa, a small boy of about 6, followed me about and literally haunted our little house, staring at our large bag of salt which he ate with the same relish children in other cultures eat sweets. Then there was Tanui, a woman of middle age (whose hair was cropped short indicating that a near relative had recently died) who often brought us presents of food. Gradually most of the villagers emerged as distinctive personalities and among them was Champukwi. I cannot remember when I first came to know him as an individual, but his name begins to appear regularly in field notebooks about one month after our arrival. Soon, he became my best informant, and after a time, an inseparable companion.

In 1939 Champukwi must have been about 25 years of age. He was tall for a Tapirapé male, measuring perhaps about 5 feet 6 inches, strongly built but lean, and weighing, I should judge, about 150 lbs. Like all Tapirapé men he wore his hair in bangs across his forehead with a braided pigtail tied at the back of his neck. He was somewhat of a dandy, for his feet and the calves of his legs were painted bright red every evening with *rucu*. From time to time he painted an intricate design on his body, and he wore crocheted disc-like wrist ornaments of cotton string dyed red. He was obviously a man of some prestige among men of his age, for youths and younger men treated him with deference, always finding a seat for him on the bench that was built against one wall of our house. I soon learned that he, too, had spent a short period at a mission station several years earlier and that he knew a few words of Portuguese. He was married and had a daughter about 2 years of age. His wife, hardly attractive according to my American tastes, appeared to be somewhat older than he, and was pregnant when we first met.

Champukwi seemed more patient than other Tapirapé with my attempts to use his language and to seek information. He would repeat a word, a phrase, or a sentence several times so that I might write it down phonetically. He resorted to his meager Portuguese and even to mimicry to explain what was meant. His patience was of course requited by gifts of beads, hardware, and salt which I provided meals became more or less accustomed to his nakedness, but sometimes he forgot to dress before sallying forth into the street. The rural Brazilian diet, derived in large measure from native Indian foods, seemed to please Champukwi, but he could not be comfortable eating at the table. He preferred during meals to sit across the room on a low stool.

Champukwi's reaction to this rural form of Brazilian civilization was not childlike in any way. He in turn became an ethnologist. He wanted to see gardens that provided the food for so many people (Furo de Pedra had hardly more than 400 people at that time). He was fascinated by the sewing machines with which he saw the women working. He attended the Catholic ceremonies held in the little chapel. He saw pairs of men and women dance face to face in semblance of an intimate embrace. About these and other strange customs he had many questions. But like the inquisitive anthropologist who had come to live in his village, his own curiosity sometimes became obtrusive. He peered into the homes of people and sometimes entered uninvited. And he followed the Brazilian women to their rather isolated bathing spot in the Araguaia River to discover if there were any anatomical differences between these women and those in his village. He even made sexual advances to Brazilian women, actions which, if he had known, were very dangerous in view of the jealous zeal with which Brazilian males protect the honor of their wives and daughters. On the whole, however, Champukwi became quite a favorite of the local Brazilians during his two week visit to Furo de Pedra. His Portuguese improved while he visited in their homes, and he collected simple presents, such as fish hooks, bottles, tin cans, and the like, to take home with him. Even during this short period away from the village, my work with him continued. He told me of antagonisms, gossip, and schisms in the Tapirapé village which he would have hesitated to relate on home grounds. He told me of adulterous affairs in process and of the growing determination among one group of kinsmen to assassinate Urukumu, the powerful medicine man, because they suspected him of performing death-dealing sorcery.

After two weeks in Furo de Pedra, I found that it would be necessary for Valentim Gomes and me to go to Rio de Janeiro. It was not possible for Champukwi to accompany us and so I arranged for two Brazilian frontiersmen to return him to a point on the Tapirapé River from which he could easily hike to his village in a day. Valentim and I then began our slow trip up the Araguaia River to the motor road and thence to Rio de Janeiro. Two months later, rid of malaria and with a new stock of supplies, we returned to spend the long rainy months from November until the end of May in the Village of the Tapirs. Champukwi was there to welcome us, and he came each day to help repair and enlarge our house. We easily fell into our former friendly relationship, now strengthened by the experience in common of the trip to Furo de Pedra and by the feeling which many anthropologists have shared with the people of their communities – that anyone who returns is an "old friend."

My return to the village that November marked, in a sense, the end of what might be called the first phase of my relationship with Champukwi as friend and as anthropological informant. During the course of at least 200 hours of conversation (many of which may be methodologically dignified as interviews), I had learned much about Champukwi as a person as well as about Tapirapé culture. I knew that as a small boy he had come from Fish Village, where his parents had died, to live in the Village of the Tapirs. He had lived with his father's younger brother, Kamaira, who was the leader of a large household. He even confided to me his boyhood name; Tapirapé change their names several times during their lifetimes and mention of a person's first childhood name, generally that of a fish, an animal, or simply descriptive of some personal characteristic, causes laughter among the audience and considerable embarrassment to the individual. I knew that Champukwi had been married before he took his current wife, and that his first wife had died in childbirth. He revealed that her kinsmen had gossiped that her death was caused by his lack of respect for the food taboos imposed upon an expectant father. This same set of taboos now bothered him again. A series of foods, mainly meats and particularly venison, is prohibited to fathers of infants and to husbands of pregnant women.

meager vocabulary. Thus, I could understand and make myself understood to Champukwi better than any other Tapirapé. Moreover, because he spent long hours in our house, he was learning Portuguese from Valentim Gomes, and this was an aid in helping me translate newly learned words and phrases in Tapirapé and even helped me understand his explanations of Tapirapé culture patterns. Champukwi thus consciously became my teacher, and others came to realize that he was teaching me. During the next two months we had daily sessions, some very brief and others lasting two or more hours.

In October of 1939, some six months after my arrival, I found it necessary to leave the Village of the Tapirs to go to Furo de Pedra for supplies and to collect mail that was held there for me. Valentim Gomes and I had come up the Tapirapé River, a tributary of the Araguaia, pulled by an outboard motor belonging to an anthropological colleague who had since returned to the United States. Now we had to paddle ourselves downstream. We could expect little help from the sluggish current and since the river was so low, it might be necessary to haul our canoe through shallows. Malaria had left me weak and I doubted that I was equal to this strenuous task. Several Tapirapé men, including Champukwi, were anxious to accompany us, but having Indians with us in Furo de Pedra was not advisable. First, they were susceptible to the common cold, which among relatively uncontaminated peoples such as these American aborigines often turns into a serious, and even fatal, disease. Second, unaccustomed to clothes, money, many foods, and other Brazilian customs and forms of etiquette, they would be totally dependent upon us during our stay in this frontier community. Nevertheless, the temptation to have my best informant with me during the trip and during our stay in Furo de Pedra was great and so we agreed to take Champukwi.

The trip was made slowly. Two good frontiersmen in a light canoe could have made it in three days, but we took eight. Champukwi was of little help in the canoe; unlike the riverine tribes the Tapirapé are a forest people who know little about the water, and few of them had ever traveled by canoe. Champukwi was unusual in that he could swim. Although he had more endurance than I, his efforts at paddling endangered the equilibrium of our canoe. However, he could shoot fish with his bow and arrow. The dry season had driven game from the open savanna which borders the Tapirapé River so that we were able to kill deer, *mutum* (another species of large forest fowl), and a wild goose to supplement the less palatable fare we had brought with us. Each night we camped on a beach from which we were able to collect the eggs of a small turtle, the *tracaja*, that had been buried in the sand. Only the mosquitoes which swarmed during sundown and early evening marred our trip. The experience remains one of the most memorable of my life, a feeling that was shared, I believe, by Valentim Gomes and by Champukwi.

Champukwi adjusted to Furo de Pedra with amazing rapidity. His short visit as a youth to the mission station undoubtedly contributed to his quick adaptation although, to be sure, there were minor problems and incidents. The Brazilians of Furo de Pedra were accustomed to Indians, for nearby there was a village of semi-civilized Caraja Indians who frequently visited and traded in the settlement. Yet, Champukwi was a bit of a curiosity – the townspeople had seen only one other Tapirapé. The local Brazilians invited him into their homes and offered him coffee and sweets. Both Valentim Gomes and I watched over his movements with all the anxiety of overprotective parents for fear that he might be exposed to a respiratory infection (he did not contract any) or that the hospitality of the local Brazilians might persuade him to drink *cachaça* (sugar cane *aguardiente*). Alcoholic beverages were unknown to the Tapirapé who are unlike most South American groups in this respect. According to Champukwi's own report, he tried *cachaça* only once in Furo de Pedra and (quite normally) found it distasteful and unpleasant. Yet there were moments that were awkward at the time however humorous they seem in retrospect. One day when I bought several dozen oranges in the street, Champukwi calmly removed the trousers that had been provided for him and made a sack to carry home the oranges by tying up the legs. In Furo de Pedra, he often went nude in the house we had rented for our stay. Even the Brazilian woman who came to prepare our so (I refused because I had already given him one bushknife), and the like.

Champukwi reacted moodily, often violently, to this situation. I could no longer count on his visits nor on our research interviews. He now visited us with a glum look on his face, and when he was not at once offered coffee, he left offended. But the very next day he might return, gay and joking, yet without his former patience for teaching or explaining Tapirapé culture. Once he returned tired from a hunting trip, and, irritated by his wife, he beat her with the flat side of his bushknife and marched off in anger, thoughtfully taking the family hammock and a basket of manioc flour, to sleep four nights in the forest near his garden. Soon afterwards, he left his wife to take the wife of a younger man. This did not become a major scandal in the village. After some tense yet calm words between the two men, it seemed clear that the young woman preferred Champukwi and the abandoned husband peacefully moved into the men's house. Champukwi's former wife and their two young daughters continued to live with her relatives as is the Tapirapé rule. But the switch of spouses caused tension between Champukwi and his former wife's kinsmen, and between Champukwi and the abandoned husband's kinsmen; and, to multiply his woes, he now had a new set of in-laws to satisfy. For about a month thereafter I rarely saw Champukwi; he obviously avoided our house. When we met in the village or in the men's house, he simply said that he was busy repairing his house or hunting.

Discussing emotions with someone from a culture as widely different as Tapirapé is from my own was difficult, and the language barrier was still a real one. Although my Tapirapé vocabulary was increasing, it was hardly adequate to probe deeply into emotional responses; nor was Champukwi given to introspection. I shall probably never fully understand Champukwi's temporary rejection of me, but the cause was probably both sociological and psychological. First, his apparent influence with me and our close friendship had created antagonism on the part of other villagers. By rejecting the outsider, he now hoped to reinstate himself in his own society. A second, deeper and more personal reason, contributed to his rejection of me; he had told me too much about himself, and feared that he had lost face in the process. Also, it was obvious that I was growing less dependent upon him for knowledge as my facility with the language improved and my information about the culture grew. Finally, the rejection was not one-sided. Now additional informants were desirable for my research. Also, if I remember correctly (it is not stated in my notebooks), I was annoyed by Champukwi's neglect and disappointed by his lack of loyalty.

When the heavy rains of late December and January set in, we were all more or less confined to the village as the rivers and streams rose to flood the savanna. What had been brooks in the tropical forest became wide streams, difficult, and sometimes dangerous, to ford. It rained many hours each day. The Tapirapé women and children spent most of the time in their dwellings, and the men and older boys lounged in the men's house. Our house again became a meeting place. And as this was of course an opportune time for interviewing, I joined the men in their club or entertained visitors at home. I began to see more of Champukwi – first, in the men's house and then as he again became a regular visitor at our house. Now, he brought his new (and younger) wife with him. He liked to sit up with us late at night after the other Tapirapé visitors had retired to their dwellings or to the men's house for the night-long sings that are customary during the season of heavy rains. Under the light of our gasoline lamp, we again took up our study of Tapirapé culture. Not once did he mention his period of antagonism except to complain that the Tapirapé gossip too much.

Sometime late in January there began what might be considered the second phase of my relationship with Champukwi. Our friendship was no less intimate than before, but our conversations and more formal interviews were not now as frequent. During the next months, Champukwi became almost my assistant, an entrepreneur of Tapirapé culture. He continued to provide invaluable information, but when I became interested in a subject of which he knew little, he would recommend that I talk to someone else. Though he directed me to Urukumu on the subject of medicine men or shamans, Champukwi himself related dreams

On two excursions to the savanna (which abounds with deer) Champukwi had eaten venison. Moreover, since the Tapirapé identified cattle with deer, and thus beef with venison, he had broken the taboo several additional times by partaking also of this forbidden meat. The rather scrawny condition of his 2-year-old daughter, he feared, resulted from his faults. Just after our return to the village in early November, his wife gave birth to a second daughter. She had a difficult delivery, and he remembered his transgressions. Several village gossips, without knowing anything about his misdeeds, had nevertheless accused him of this breach of taboo.

Champukwi's home life was not a happy one. He was frequently in conflict with his second wife, who had, indeed, considerable basis for complaint. She could not claim that he was a poor provider, for Champukwi was a good hunter and a diligent gardener. But he confided to me that he did not find her attractive, or at least not as attractive as other women in the village. Champukwi had a lusty sense of humor and enjoyed joking with Valentim and me. In this mood he told of his many extramarital affairs, which were in truth but slightly concealed. I would in any event have heard of these liaisons; he gave his paramours beads which everyone in the village knew I had given him as presents. This practice caused trouble for the women because their husbands could readily identify the source of the gifts. It also created trouble for Champukwi at home. His wife complained of his affairs and on one occasion, according to Champukwi, she attacked him, grabbing him by his pigtail and squeezing his exposed testicles until he fell helpless into a hammock. On other occasions, she retaliated in a manner more usual for a Tapirapé woman – she simply refused to carry drinking water from the creek, to cook food for him, and to allow him to sleep in the hammock which she and Champukwi shared. For a Tapirapé man to carry drinking water, to cook, or to sleep on a mat is considered ridiculously funny. In other circumstances, Champukwi would have had to seek recourse with a female relative. However, to do so would be tantamount to a public announcement of his marital difficulties; the whole village would have known, to their considerable merriment and jest. But having *tori* friends in the village, Champukwi could come quietly to us at night to drink water, to ask for something to eat, and even to sleep in an extra hammock we had for visitors. His affairs were evidently extensive, for he once divided all of the adult women of the village into two categories – those "I know how to talk with" (i.e., to seduce) and those "I do not know how to talk with." There were many with whom he "could talk."

Unfortunately, by late November of 1939, I knew too much about Champukwi's affairs either for his comfort or for mine. His wife sometimes came to my house to ask if I knew where he had gone (I could generally guess), and once an irate husband even came to inquire of his whereabouts. His Don Juan activities had evidently increased. His friendship with me caused him trouble with other Tapirapé who were envious of the presents he received. The story was circulated that he had stolen a pair of scissors which, in fact, I had given to him. Moreover, several people caught colds, and he was accused of bringing the infection from Furo de Pedra (actually it was probably transmitted by the frontiersman who had helped transport us to the village in November). Champukwi sought revenge by cutting down one of the main supports of the men's house, which promptly caved in. No one died or was seriously injured and the destruction of the men's house was soon forgotten since it is normally rebuilt each year. However, people continued to criticize Champukwi, much of their criticism revolving around his relationship with me. There are no realms of esoteric secrets in Tapirapé culture (as there are in many cultures) that must not be revealed to an outsider; there is only the "secret" of the men from the women that the masked dancers are not supernatural beings but merely masquerading men, but I had been fully and openly brought into the "secret." I was, moreover, exceedingly careful in conversation never to refer to any bit of personal information that some informant, Champukwi or another, had told me. But rumors were rife in the small village – that I was angry and would soon leave (I was by then a valuable asset), that Champukwi told me lies about others, that I refused to give a bushknife to a household leader because Champukwi had urged me not to do his body elaborately, and making him the center of dancing and singing – although the youth must himself dance continuously for a day and a night. Champukwi led the singing most of the night, but at dawn he came to our house to supervise the packing of our belongings into the basket-like cases made of palm which are used for carrying loads of any kind. He divided the baggage among the younger men. Even some of the older household leaders decided to accompany us but they, of course, did not carry anything. Our trip was slow because everyone was tired after the all-night festival and because of the water through which we had to wade. At one point, rafts had to be made to transport our baggage across a still-swollen stream. Since the Tapirapé do not swim – or, like Champukwi, they swim but poorly – it was the job of the *tori* to swim and push the rafts. I had the honor of swimming across the stream, pushing the respected chieftain, Kamiraho. (How he got back, I shall never know.) After a day and a half, we reached the landing on the Tapirapé River, and the next morning we embarked downriver. My last memory of Champukwi was of him standing on the bank waving in *tori* style until our boat made the curve of the river.

I did not return to visit the Tapirapé until 1953, but news of them came to me at intervals. Valentim Gomes returned to the region in 1941 as an officer of the Brazilian Indian Service, and his post was charged with the protection of the Tapirapé Indians. In his first year in this capacity, he wrote me: "I report that I was in the village of the Tapirapé on the 26th of July [1941]. They were in good health and there were plenty of garden products such as manioc, yams, peanuts, and the like. There were plenty of bananas. But I am sorry to say that after we left them, twenty-nine adults and a few children have died. Fifteen women and fourteen men died. Among those who died was Champukwi, the best informant in the village, and our best friend." Several slow exchanges of letters brought further details from Valentim. In some manner, perhaps through a visit from a Brazilian frontiersman, several Tapirapé had contracted common colds. Its fatalness to them is indicated by the name they give it – *ó-ó* (*ó* is the augmentative which might be translated as "big, big"). Since they have no knowledge of the process of contagion and have not acquired immunity to the common cold, the disease spread rapidly throughout the village. The Tapirapé realized, I knew, that colds and other diseases such as measles which they had suffered before, were derived from visitors. Yet they also believed that death resulted from evil magic or sorcery. Why do some people who are very sick from colds get well, they asked, while others who are no more ill, soon die? It is only because those who die are the victims of sorcery, they had explained to me. So, following many deaths, including that of a young man like Champukwi, who enjoyed prestige and had many kinsmen, I was not surprised to learn from Valentim Gomes that the powerful shaman, Urukumu, had been assassinated. As Champukwi had told me, suspicion of Urukumu had already been growing even during my residence in the village. After the death of Champukwi, one of his many "brothers" (actually a cousin but called by the same term as brother in Tapirapé) had entered Urukumu's house late at night and clubbed him to death. To the Tapirapé, grief and anger are closely related emotions and there is one word, *iwúterahú*, that describes either or both states of mind. Thus in both word and deed grief can be quickly transformed into vengeful anger.

In 1953, when I returned to the Araguaia River, I found only fifty persons, the remnants of the Tapirapé tribe, settled under the protection of the Brazilian Indian Service in a small village near the mouth of the Tapirapé River. My old companion, Valentim Gomes, was the Indian officer in charge. The history of the intervening years had been a tragic story; the Tapirapé had suffered steady depopulation from imported diseases and they had been attacked by the warlike and hostile Kayapo tribe, who had burned their village and carried off several younger women. They had been forced to leave their own territory to seek the protection of the Indian Service, and then cattle ranchers encroached upon the Tapirapé savannas, once rich with game. Champukwi was but one of the many victims of this disintegration of Tapirapé society. Upon my arrival several of Champukwi's surviving relatives met me with the traditional "welcome of tears"; to the Tapirapé, such a return mixes emotions of joy at seeing an old friend with the sadness of he had heard other shamans tell. He explained that he did not want to become a shaman himself, for he had seen grieved relatives beat out the brains of Tapirapé shamans whom they suspected of causing a death by sorcery. He was not certain, he said, whether such shamans had actually performed sorcery; but he reasoned that any shaman might come to such an end. Champukwi did have the frequent dreams that are indicative of one's powers to become a shaman and, in some of this dreams, he saw *anchunga*, the ghosts and supernaturals who are the aids of shamans. He had told only one or two of his kinsmen about this, and he did not want it to be known throughout the village lest there be pressure on him to train for shamanism.

Champukwi sketched for me the stories of Petura, the Tapirapé ancestral hero who stole fire from the King Vulture, daylight from the night owl, *genipa* (used for dye) from the monkeys, and other items for the Tapirapé. However, he persuaded Maeumi, an elder famous for his knowledge of mythology, to relate the details although he himself helped considerably to clarify for me the meaning of native phrases and to make the stories told by Maeumi more fully understandable. Champukwi also forewarned me of events that I might want to witness, events that without his warning I might have missed. Such were the wrestling matches which took place upon the return of a hunting party between those men who went on the hunt and those who remained at home. He told me of a particularly handsome basket a man had made, which I might want to add to my collection for the Brazilian National Museum. He came to tell me that a young woman in a neighboring house was in labor, thus enabling me to get a photograph of the newborn infant being washed in the stream, and he urged the men to celebrate for my benefit a ceremony which might easily have been omitted. Champukwi was no longer merely an informant. He became a participant in ethnographic research although, of course, he never thought of it in these terms. He seemed somehow to understand the anthropologist's task in studying his culture, and in the process he gained considerable objectivity about his own way of life.

Yet it must be said that Champukwi did not seem to discredit the norms, institutions, and beliefs of his own people. Although he saw Valentim and me walk safely down the path through the forest late at night, he steadfastly refused to do the same; for the path was a favorite haunt of the lonely ghosts of deceased Tapirapé who might harm the living. He reasoned that the *tori* were probably immune to this danger. When he was ill, he took the pills we urged upon him but he also called in the shaman. His curiosity about airplanes, automobiles, and "gigantic canoes" (passenger boats) which he saw pictured in the magazines we had brought with us, was great; but he boasted that the Tapirapé could walk farther and faster than any *tori* or even the Caraja (who are a canoe people). In fact, his interest in, and enthusiasm for, certain Tapirapé activities seemed to be heightened by our presence. Almost all Tapirapé ceremonials involve choral singing and Champukwi was a singing leader of one of the sections of the men's societies. He was always pleased when we came to listen, particularly if we made the motions of joining in. He was an excellent wrestler in Tapirapé style, in which each opponent takes a firm grip on the pigtail of the other and attempts to throw him to the ground by tripping. Our wrestling match was brief although I was much taller than he; and his match with Valentim Gomes, who outweighed him by more than forty pounds, was a draw. Unlike so many who get a glimpse at a seemingly "superior" cultural world, Champukwi never became dissatisfied with his own way of life.

In June of 1940, my period of residence among the Tapirapé Indians ended. The waters on the savanna which had to be crossed afoot to get to the Tapirapé River where our canoe was moored had not completely receded. Many Tapirapé friends, among them Champukwi, offered to carry our baggage, made lighter after a final distribution of gifts, down to the river. The night before our departure a festival with the usual songfest was held to celebrate the final phase of a ceremony during which a youth, this time the nephew of Kamiraho, became a man. Some Brazilian tribes make this occasion an ordeal by such means as applying a frame of stinging wasps to the body of the novice, but it is characteristic of the Tapirapé that the "ordeal" consists only of decorating the youth with a headdress of magnificent red macaw feathers, painting

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# Behind Many Masks: Ethnography and Impression Management

*Gerald D. Berreman*

Ethnographers have all too rarely made explicit the methods by which the information reported in their descriptive and analytical works was derived. Even less frequently have they attempted systematic descriptions of those aspects of the field experience which fall outside of a conventional definition of method, but which are crucial to the research and its results. The potential fieldworker in any given area often has to rely for advance information about many of the practical problems of his craft upon the occasional verbal anecdotes of his predecessors or the equally random remarks included in ethnographic prefaces. To the person facing fieldwork for the first time, the dearth of such information may appear to be the result of a conviction, among those who know, that experience can be the only teacher. Alternatively, he may suspect ethnographers of having established a conspiracy of silence on these matters. When he himself becomes a bona fide ethnographer he may join that conspiracy inadvertently, or he may feel obligated to join it not only to protect the secrets of ethnography, but to protect himself. As a result of the rules of the game which kept others from communicating their experience to him, he may feel that his own difficulties of morale and rapport, his own compromises between the ideal and the necessary, were unique, and perhaps signs of weakness or incompetence. Consequently, these are concealed or minimized. More acceptable aspects of the field experience such as those relating to formal research methods, health hazards, transportation facilities and useful equipment suffice to answer the queries of the curious. This is in large measure a matter of maintaining the proper "front" (see below) before an audience made up not only of the uninitiated, but in many cases of other ethnographers as well.

As a result of this pattern "Elenore Bowen" shared the plight of many an anthropological neophyte when, according to her fictionalized account she arrived in West Africa girded for fieldwork with her professors' formulae for success:

> Always walk in cheap tennis shoes; the water runs out more quickly, [and] You'll need more tables than you think. (Bowen, 1954, pp. 3–4)

This prologue is not an exposition of research methods or field techniques in the

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the memory of those who have died during the interim. Both the sadness and the joy are expressed almost ritually by crying. People spoke sympathetically to me of the loss of my friend and they brought a young man, who had been but a small boy in 1940, but who was now known as Champukwi. This boy had visited for many months, and had even studied a little, with the Dominican missionaries on the lower Araguaia River; he therefore spoke Portuguese well. He remembered my friendship with his namesake and perhaps felt, as I did, some strange bond between us. So again for a few days the name of Champukwi was entered into my notebook as my source of information on Tapirapé culture.

In the security of our studies and in the classroom, we claim that anthropology is a social science in which regularities of human behavior and of social systems are studied. But, at its source, in the midst of the people with whom the anthropologist lives and works, field research involves the practice of an art in which emotions, subjective attitudes and reactions, and undoubtedly subconscious motivations participate. Of course, the well-trained anthropologist takes all possible precautions to be objective and to maintain a detached attitude. He gathers information from a "cross section" of the population – from a variety of informants selected for their different status positions in their society. He interviews, as far as is possible, men and women, young and old, rich and poor, individuals of high and low status, so that his picture of the culture may not be distorted. The anthropologist might (he seldom has done so) go so far as to keep a record of his subjective reactions in an attempt to achieve greater objectivity. Yet he is never the entirely detached observer he may fancy himself to be – nor am I sure that this should be so. Anthropological field research is a profoundly human endeavor. Faced over a long period by a number of individuals, some intelligent and some slow, some gay and some dour, some placid and some irritable, the anthropologist almost inevitably is involved in a complex set of human relations among another people just as he is by virtue of his membership in his own society. And each anthropologist is a distinctive personality and each undoubtedly handles in his own way his dual role as a sympathetic friend to key informants and as a scientific observer of a society and culture which is not his own. To me, Champukwi was, above all, a friend whom I shall remember always with warm affection.

> him; no one has sent me so much as a grain of millet; no one has asked me to sit and talk with him; no one has even asked me who I am or whether I have a family. They ignore me.

He fared better than the teacher in another village of the area who had to give up after three months during which he and his proposed school were totally boycotted.

Among the forestry officers whose duty it is to make periodic rounds in these hills, villagers' lack of hospitality is proverbial. They claim that here a man has to carry his own food, water, and bedroll because he cannot count on villagers to offer these necessities to him on his travels. Community development and establishment of credit cooperatives, two governmental programs in the area, have been unsuccessful largely because of their advocates' inability to establish rapport with the people. My assistant, who had worked for more than a year in an anthropological research project in a village of the plains, was constantly baffled at the reticence and lack of hospitality of villager's. As he said:

> In Kalapur, when you walked through the village, men would hail you and invite you to sit and talk with them. Whether or not they really wanted you to do so, they at least invited you out of common courtesy. Here they just go inside or turn their backs when they see you coming.

The reasons for such reticence are not far to seek. Contacts with outsiders have been limited largely to contacts with policemen and tax collectors – two of the lowest forms of life in the Pahari taxonomy. Such officials are despised and feared not only because they make trouble for villagers in the line of duty, but because they also extort bribes on the threat of causing further trouble and often seem to take advantage of their official positions to vent their aggressions on these vulnerable people. Since India's independence, spheres of governmental responsibility have extended to include stringent supervision of greatly extended national forest lands, rationing of certain goods, establishment of a variety of development programs, etc. The grounds for interfering in village affairs have multiplied as the variety of officials has proliferated. Any stranger, therefore, may be a government agent, and as such he is potentially troublesome and even dangerous.

Villagers' fears on this score are not groundless. Aside from the unjust exploitation which such agents are reputed to employ in their activities, there are many illegal or semilegal activities carried on by villagers which could be grounds for punishment and are easily used as grounds for extortion. In Sirkanda, national forest lands and products have been illegally appropriated by villagers, taxable land has been underreported, liquor is brewed and sold illicitly, women have been illegally sold, guns have gone unlicensed, adulterated milk is sold to outside merchants, children are often married below the legal age, men have fled the army or escaped from jail, property has been illegally acquired from fleeing Muslims at the time of partition. Any of these and similar real and imagined infractions may be objects of a stranger's curiosity and therefore are reasons for discouraging his presence in the village.

Paharis are thought by people of the plains to be ritually, spiritually, and morally inferior. They are suspected of witchcraft and evil magic. In addition they are considered naive bumpkins – the hillbilly stereotype of other cultures. Paharis try to avoid interaction with those who hold these stereotypes. Alien Brahmins may seek to discredit their Pahari counterparts by finding evidence of their unorthodoxy; alien traders may seek to relieve them of their hard-earned cash or produce by sharp business practices; scoundrels may seek to waylay or abduct village women; thieves may come to steal their worldly possessions; lawyers or their cohorts may seek evidence for trumped-up legal proceedings which a poor Pahari could not hope to counteract in court; Christian missionaries may hope to infringe on their religious beliefs and practices. Strangers are therefore suspected of having ulterior motives even if they are not associated with the government.

The only way to feel sure that such dangers do not inhere in a person is to know who he is, and to know this he must fit somewhere into the known social system. Only then is he subject to effective local controls so that if he transgresses, or betrays a trust, he can be usual sense. It is a description of some aspects of my field research, analyzed from a particular point of view. As such, it is an attempt to portray some features of that human experience which is fieldwork, and some of the implications of its being human experience for ethnography as a scientific endeavor. It is not intended as a model for others to follow. It tells what happened, what I did, why I did it and with what apparent effect. As in all fieldwork, the choices were not always mine and the results were frequently unanticipated. But the choices and results have proved instructive. I hope that this account will add depth to the ethnographic study which follows by conveying the methods and circumstances which led to it.

## Introduction

Every ethnographer, when he reaches the field, is faced immediately with accounting for himself before the people he proposes to learn to know. Only when this has been accomplished can he proceed to his avowed task of seeking to understand and interpret the way of life of those people. The second of these endeavors is more frequently discussed in anthropological literature than the first, although the success of the enterprise depends as largely upon one as the other. Both tasks, in common with all social interaction, involve the control and interpretation of impressions, in this case those conveyed by the ethnographer and his subjects to one another. Impressions are derived from a complex of observations and inferences drawn from what people do as well as what they say both in public, i.e., when they know they are being watched, and in private, i.e., when they think they are not being watched. Attempts to convey a desired impression of one's self and to interpret accurately the behavior and attitudes of others are an inherent part of any social interaction, and they are crucial to ethnographic research.

My research in a tightly closed and highly stratified society can serve as a case study from which to analyze some of the problems and consequences inherent in the interaction of ethnographer and subjects. Special emphasis will be placed upon the differential effects of the ethnographer's identification with high-status and low-status groups in the community.

## The Setting

The research upon which this account is based took place in and around Sirkanda, a peasant village of the lower Himalayas of North India. Its residents, like those of the entire lower Himalayan area from Kashmir through Nepal, are known as *Paharis* (of the mountains). The village is small, containing some 384 individuals during the year of my residence there in 1957–8, and it is relatively isolated, situated as it is in rugged hills accessible only on foot and nine miles from the nearest road and bus service.

Strangers in the area are few and readily identifiable by dress and speech. People who are so identified 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. Since the first two are ascribed characteristics, the only hope an outsider has of achieving acceptance is by establishing residence and, through social interaction, acquiring the status of a community-dweller; a slow process at best.

The reluctance of Sirkanda villagers and their neighbors to accept strangers is attested to by the experience of those outsiders who have dealt with them. In 1957 a new teacher was assigned to the Sirkanda school. He was a Pahari from an area some fifty miles distant. Despite his Pahari background and consequent familiarity with the language and customs of the local people, he complained after four months in the village that his reception had been less than cordial:

> I have taught in several schools in the valley and people have always been friendly to me. They have invited me to their homes for meals, have sent gifts of grain and vegetables with their children, and have tried to make me feel at home. I have been here four months now with almost no social contact aside from my students. No one has asked me to eat with

prise at local rituals, this suspicion gradually faded. We had anticipated this interpretation of our motives and so were careful not to show undue interest in religion as a topic of conversation. We purposely used Hindu rather than areligious forms of greeting in our initial contacts to avoid being identified as missionaries. As a topic for polite and, we hoped, neutral conversation, we chose agriculture. It seemed timely too, as the fall harvest season began not long after our arrival in the village. Partly as a result of this choice of conversational fare, suspicion arose that we were government agents sent to reassess the land for taxation purposes, based on the greater-than-previously-reported productivity of the land. Alternatively, we were suspected of being investigators seeking to find the extent of land use in unauthorized areas following the nationalization of the surrounding uncultivated lands. My physical appearance was little comfort to villagers harboring these suspicions. One man commented that "Anyone can look like a foreigner if he wears the right clothes." Gradually these fears too disappeared, but others arose.

One person suggested that our genealogical inquiries might be preliminary to a military draft of the young men. The most steadfast opponent of our presence hinted darkly at the machinations of foreign spies – a vaguely understood but actively feared type of villain. Nearly four months had passed before overt suspicion of this sort was substantially dissipated, although, of course, some people had been convinced of the innocence of our motives relatively early and others remained suspicious throughout our stay.

One incident nearly four months after our first visit to the village proved to be a turning point in quelling overt opposition to our activities in the village. We were talking one afternoon to the local Brahmin priest. He had proved to be a reluctant informant, apparently because of his fear of alienating powerful and suspicious Rajputs whose caste-fellows outnumbered his own by more than thirty to one in the village (his was the only Brahmin household as compared to 37 Rajput households in Sirkanda), and in whose good graces it was necessary for him to remain for many reasons. However, he was basically friendly. Encouraged by our increasing rapport in the village at large, by his own feelings of affinity with my Brahmin assistant, Sharma, and by the privacy of his secluded threshing platform as a talking place, he had volunteered to discuss his family tree with us. Midway in our discussion, one of the most influential and hostile of the Rajputs came upon us – probably intentionally – and sat down with us. The Brahmin immediately became self-conscious and uncommunicative but it was too late to conceal the topic of our conversation. The Rajput soon interrupted, asking why the Brahmin was telling us these things and inquiring in a challenging way what possible use the information could be to an American scholar. He implied, with heavy irony, that we had ulterior motives. The interview was obviously ended and by this time a small crowd of onlookers had gathered. Since a satisfactory answer was evidently demanded and since most members of the audience were not among the people we knew best, I took the opportunity to answer fully.

I explained that prior to 1947, India had been a subject nation of little interest to the rest of the world. In the unlikely event that the United States or any other country wanted to negotiate regarding matters Indian, its representatives had merely to deal with the British who spoke for India. Indians were of no importance to us, for they were a subject people. They, in turn, had no need to know that America existed as, indeed, few did. Then in 1947, after a long struggle, India had become independent; a nation of proud people who handled their own affairs and participated in the United Nations and in all spheres of international relations on a par with Britain and the United States. Indians for the first time spoke for themselves. At once it became essential for Indians and Americans to know one another. Consequently India sent hundreds of students to America, among other places, and we sent students such as myself to India. We had worked at learning their language and we also wanted to learn their means of livelihood, social customs, religion, etc., so that we could deal with them intelligently and justly, just as their students were similarly studying in and about America. Fortunately I had an Indian acquaintance, then studying a rural community in Utah, whom I could cite as a case comparable to my own. I pointed out that Indian brought to account. The person who is beyond control is beyond trust and is best hurried on his way. This is, therefore, a relatively closed society. Interaction with strangers is kept to a minimum; the information furnished them is scanty and stereotyped. Access to such a society is difficult for an outsider.

Within this closed society there is rigid stratification into a number of hereditary, ranked, endogamous groups – castes – comprising two large divisions: the high or twice-born castes and the low or untouchable castes. The high castes, Rajputs and Brahmins, are land-owning agriculturalists who are dominant in numbers, comprising ninety per cent of the population. They are dominant in economic wherewithal, in that they own most of the land and animals, while the other castes depend on them for their livelihood. They are dominant in political power, for both traditional and new official means of control are in their hands. They dominate in ritual status as twice-born, ritually clean castes while all other castes are untouchable (*achut*). In most villages, as in Sirkanda, Rajputs outnumber Brahmins and so are locally dominant, but the ritual and social distance between them is not great and the economic difference is usually nil (Srinivas, 1959).

The low castes, whose members are artisans, are disadvantaged in each respect that the high castes are advantaged. They are dependent upon the high castes for their livelihood and are subject to the will of the high castes in almost every way. Ideally their relationship to the high castes is one of respect, deference, and obedience. In return high-caste members are supposed to be paternalistic. In practice there is a good deal of tension in the relationship, and it is held stable largely by considerations of relative power (Berreman, 1960a).

In addition there are nonhierarchical cleavages within the high castes and within the low castes based upon kinship ties (lineage and sib lines being paramount) and informal cliques and factions. As a result of these factors the community is divided within itself. While there is consensus on some things, there is disagreement on others. Acceptance by one element of the community does not imply acceptance by the whole community and it frequently, in fact, precludes it.

## The Research

It was into this community that my interpreter-assistant and I walked, unannounced, one rainy day in September, 1957, hoping to engage in ethnographic research. On our initial visit we asked only to camp there while we visited a number of surrounding villages. We were introduced by a note from a non-Pahari wholesaler of the nearest market town who had long bought the surplus agricultural produce of villagers and had, as it turned out, through sharp practices of an obscure nature, acquired land in the village. He asked that the villagers treat the strangers as "our people" and extend all hospitality to them. As might have been expected, our benefactor was not beloved in the village and it was more in spite of his intercession than on account of it that we ultimately managed to do a year's research in the village.

The note was addressed to a high-caste man who proved to be one of the most suspicious people of the village; the head of a household recently victorious in a nine-year court battle over land brought against the household by virtually the entire village; the leader of a much-resented but powerful minority faction. That he gave us an unenthusiastic reception was a blow to our morale but probably a boon to our chances of being tolerated in the village.

The interpreter-assistant who accompanied me was a young Brahmin of plains origin who had previously worked in a similar capacity for a large research project carried out in the plains village of Kalapur. I shall hereafter refer to him as Sharma.

For the first three months of our stay in the village, most of our time was spent keeping house and attempting to establish rapport, both of which were carried out under trying circumstances.

According to their later reports to us, villagers at first assumed that we were missionaries, a species which had not previously invaded this locality but which was well known. Several villagers had sold milk in Mussoorie, a hill station sixteen miles distant that is frequented by missionaries. When we failed to meddle in religious matters or to show sur-their hostility, and my presence as defined in this statement counteracted these feelings. It was especially effective in response to the Rajput who put the challenge; a man with an acute, and to many aggravating, need for public recognition of his importance. He had gained some eminence by opposing my work; he now evidently gained some by eliciting a full explanation from me and magnanimously accepting it.

Although I remained an alien and was never made to feel that my presence in the village was actively desired by most of its members, I was thereafter tolerated with considerable indulgence. I became established as a resident of Sirkanda, albeit a peculiar one, and no one tried to get me to leave. I have heard strangers en route to or from further mountain areas inquire of Sirkanda villagers as to my identity, presuming that I was out of earshot or could not understand, and be left to ponder the succinct reply, "He lives here."

Other, less spectacular rapport-inducing devices were employed. Unattached men in the village were considered, not unjustly in light of past experience and Pahari morality, a threat to village womanhood. This fear with regard to my assistant and myself was appreciably diminished when our wives and children visited the village and when a few villagers had been guests at our house in town where our families normally resided. We won some good will by providing a few simple remedies for common village ailments. One of the most effective means of attracting villagers to our abode in the village during this period was a battery radio which we brought in; the first to operate in this area. It was an endless source of diversion to villagers and attracted a regular audience, as well as being a focal attraction for visiting relatives and friends from other villages.

At first, reportedly, there had been considerable speculation in the village as to why two people of such conspicuously different backgrounds as Sharma and myself had appeared on the scene as a team if, as we claimed, we were not sent by the government or a missionary organization. The plausibility of our story was enhanced when Sharma made it clear to villagers that he was my bona fide employee who received payment in cash for his services.

Villagers never ceased to wonder, as I sometimes did myself, why I had chosen this particular area and village for my research. I explained this in terms of its relative accessibility for a hill area, the hospitality and perspicacity of Sirkanda people, the reputation Sirkanda had acquired in the area for being a "good village," and my own favorable impression of it based on familiarity with a number of similar villages. The most satisfactory explanation was that my presence there was largely chance, i.e., fate. Everyone agreed that this was the real reason. Villagers pointed out that when the potter makes a thousand identical cups, each has a unique destiny. Similarly, each man has a predetermined course of life and it was my fate to come to Sirkanda. When I gave an American coin to a villager, similar comment was precipitated. Of all the American coins only one was destined to rest in Sirkanda and this was it. What greater proof of the power of fate could there be than that the coin had, like myself, found its way to this small and remote village.

All of our claims of motive and status were put to the test by villagers once they realized that we planned to remain in Sirkanda and to associate with them. Sharma's claim to Brahmin status was carefully checked: extensive inquiry was made about his family and their origins; his behavior was closely watched; his family home was inspected by villagers on trips to town. Only then were villagers satisfied that he was what he claimed to be. When all of the claims upon which they could check proved to be accurate, villagers were evidently encouraged to believe also those claims which could not be verified.

That suspicions as to our motives were eventually allayed did not mean we therefore could learn what we wanted to learn in the village. It meant only that villagers knew in a general way what they were willing to let us learn; what impressions they would like us to receive. The range of allowable knowledge was far greater than that granted a stranger, far less than that shared by villagers. Although at the time I did not realize it, we were to be told those things which would give a favorable impression to a trustworthy plains Brahmin.

and American scholars had studied Indian cities, towns and villages of the plains so that their ways were well known, but that heretofore the five million Paharis – residents of some of the richest, most beautiful, historically and religiously most significant parts of India – had been overlooked. I emphasized that Paharis would play an increasing role in the development of India and that if they were to assume the responsibilities and derive the advantages available to them it was essential that they be better known to their countrymen and to the world. My research was billed as an effort in this direction.

I would like to be able to report that on the basis of this stirring speech I was borne aloft triumphantly through the village, thereafter being treated as a fellow villager by one and all. Needless to say, this did not happen. My questioner was, however, evidently favorably impressed, or at least felt compelled to act as though he were before the audience of his village-mates. He responded by saying that he would welcome me in his house any time and would discuss fully any matters of interest to me. He also offered to supply me with a number of artifacts to take to America as exhibits of Pahari ingenuity. I might add, anticlimactically, that in fact he never gave me information beyond his reactions to the weather, and that the Brahmin, evidently shaken by his experience, was never again as informative as he had been immediately prior to this incident.

The Rajput challenger, however, ceased to be hostile whereas formerly he had been a focus of opposition to my presence. General rapport in the village improved markedly and the stigma attached to talking with me and my interpreter almost disappeared. One notable aftereffect was that my photographic opportunities, theretofore restricted to scenery, small children, and adolescent boys in self-conscious poses, suddenly expanded to include a wide range of economic, ritual, and social occasions as well as people of all castes, ages, and both sexes. Photography itself soon became a valuable means of obtaining rapport as photographs came into demand.

The degree to which I was allowed or requested to take photographs, in fact, proved to be a fairly accurate indicator of rapport. One of the more gratifying incidents of my research in Sirkanda occurred at an annual regional fair some eight months after the research had begun. Soon after I arrived at the fair a group of gaily dressed young women of various villages had agreed to be photographed when a Brahmin man, a stranger to me, stormed up and ordered them to refuse. An elderly and highly respected Rajput woman of Sirkanda had been watching the proceedings and was obviously irritated by the fact and manner of the intervention. She stepped to the center of the group of girls, eyeing the Brahmin evenly, and said, "Please take my photograph." I did so, the Brahmin left, and my photography was in demand exceeding the film supply throughout the fair.

The incident described above, in which the Rajput challenged my interviewing of the Brahmin priest, came out favorably partly because of the context in which it occurred. For one thing, it occurred late enough so that many people knew me and my assistant. Having no specific cause for doubting our motives, they were ready to believe us if we made a convincing case. Also, there was a sizeable audience to the event. My explanation was a response to a challenge by a high-status villager and the challenger accepted it gracefully. It was the first time that many of these people had been present when I talked at any length and my statement was put with a good deal of feeling, which fact they recognized. It was essentially an appeal for their confidence and cooperation in a task they knew was difficult and which I obviously considered important. They were not incapable of empathy. As one man had said earlier, "You may be a foreigner and we only poor villagers, but when we get to know you we will judge you as a man among other men; not as a foreigner." With time, most of the villagers demonstrated the validity of his comment by treating me as an individual on the basis of their experience with me, rather than as the stereotyped outsider or white man.

Most important, my statement placed the listeners in a position of accepting what I said or denying their own importance as people and as citizens – it appealed to their pride. They have strong inferiority feelings relative to non-Paharis which account in large measure for

After a period alone in the village, I realized that I could not work effectively without assistance because of my inadequate knowledge of the language. Although I dreaded the task of selecting and then introducing a new and inexperienced assistant into the village, this seemed to be a necessary step to preserve the continuity of the research. My hope and intention was to utilize a substitute only until Sharma would be able to work again. Not wishing to spend too much time looking for a substitute, and with qualified people extremely scarce, I employed with many misgivings and on a trial basis the first reasonably promising prospect who appeared. Happily, he proved to be an exceptionally able, willing, and interested worker. He differed from Sharma in at least three important respects: age, religion, and experience. Mohammed, as he will hereafter be called, was a middle-aged Muslim and a retired school teacher who had no familiarity with anthropological research.

These facts proved to have advantageous as well as disadvantageous aspects. I was able to guide him more easily in his work and to interact more directly with villagers than had been the case with Sharma simply because he realized his inexperience, accepted suggestions readily, and was interested in helping me to know and communicate directly with villagers, rather than in demonstrating his efficiency as a researcher and his indispensability as an interpreter. As a result of his age he received a certain amount of respect. As a Muslim he was able to establish excellent rapport with the low castes but not with the high or twice-born castes. Perhaps most importantly, he had no ego-involvement in the data. He was interested and objective in viewing the culture in which we were working, whereas Sharma had been self-conscious and anxious to avoid giving an unflattering view of Hinduism and of village life to an American in this unorthodox (to him often shockingly so) example of a Hindu village. Moreover, the Brahmin, almost inevitably, had his own status to maintain before the high castes of the village while the Muslim was under no such obligation.

Since it seemed probable that Sharma would return to work after a few weeks, I decided to make the best of the situation and utilize Mohammed in ways that would make the most use of his advantages and minimize his disadvantages, for he was strong where Sharma had been weak, and vice versa. While high-caste people were suspicious of Mohammed on the basis of his religion, low-caste people were more at ease in his presence than they had been with Sharma. Furthermore, low-caste people proved to be more informative than high-caste people on most subjects. I therefore planned to utilize this assistant to get data about low castes and from them to get as much general ethnographic data as possible. I was counting on the return of Sharma to enable me to return to the high castes and my original endeavor to secure information from and about them. However, after several weeks it became evident that Sharma could not return to work in the village. By then we were beginning to get a good deal of ethnographic material with the promise of much more. In addition to remarkably good rapport with the low castes (greater than that Sharma and I had had with anyone in the village) we were also winning the confidence of some high-caste people. In view of these circumstances I felt encouraged to continue with Mohammed and to broaden our contacts in the village in the remaining months of research.

I had not anticipated the full implications for research of the differences in status of my associates, Sharma and Mohammed. For example, villagers had early determined that Sharma neither ate meat nor drank liquor. As a result we were barely aware that these things were done by villagers. Not long after Mohammed's arrival villagers found that he indulged in both and that I could be induced to do so. Thereafter we became aware of frequent meat and liquor parties, often of an inter-caste nature. We found that these were important social occasions; occasions from which outsiders were usually rigidly excluded. Rapport increased notably when it became known that locally distilled liquor was occasionally served at our house. As rapport improved, we were more frequently included in such informal occasions. Our access to information of many kinds increased proportionately.

Mohammed's age put him virtually above the suspicion which Sharma had had to overcome regarding possible interest in local

Other facts would be suppressed and, if discovered, would be discovered in spite of the villagers' best efforts at concealment, often as a result of conversation with some disaffected individual of low esteem in the village. Our informants were primarily high-caste villagers intent on impressing us with their near conformity to the standards of behavior and belief of high-caste plainsmen. Low-caste people were respectful and reticent before us, primarily, as it turned out, because one of us was a Brahmin and we were closely identified with the powerful high-caste villagers.

Three months were spent almost exclusively in building rapport, in establishing ourselves as trustworthy, harmless, sympathetic, and interested observers of village life. In this time we held countless conversations, most of them dealing with the weather and other timely and innocuous topics. A good deal of useful ethnographic information was acquired in the process, but in many areas its accuracy proved to be wanting. Better information was acquired by observation than by inquiry in this period. We found cause for satisfaction during this frustrating and, from the point of view of research results, relatively fruitless time in the fact that we were winning the confidence of a good many people which we hoped would pay off more tangibly later. When the last open opponent of our endeavor evidently had been convinced of our purity of motive in the incident described above, we felt that we could begin our data collecting in earnest.

Until this time we had done all of our own housekeeping, cooking, dishwashing, carrying of water and firewood. These activities gave us opportunity to meet people in a natural setting and to be busy in a period when rapport was not good enough to allow us to devote full time to research. As rapport improved we found our household chores too time-consuming for optimal research. We attempted to find assistance in the village but, unable to do so, we added as a third member of our team a 17-year-old boy who was of low-caste plains origin but had lived most of his life in the hill station of Mussoorie and was conversant with Pahari ways and the Pahari language. His role was that of servant and he assumed full responsibility for our housekeeping in the village. His informal contacts with some of the younger villagers were a research asset and his low-caste origin was not overlooked in the village, but otherwise he had little direct effect on our relations with villagers. His contribution to the research was primarily in the extreme reliability of his work and his circumspection in relations with villagers.

At this point of apparent promise for productive research, Sharma, the interpreter-assistant, became ill and it was evident that he would be unable to return to our work in the village for some time. Under the circumstances this was a disheartening blow. It plunged my morale to its lowest ebb in the fifteen months of my stay in India, none of which could be described as exhilarating. I cannot here go into the details of the causes for this condition of morale: the pervasive health anxiety with which anyone is likely to be afflicted when he takes an 18-month-old child to the field in India, especially if, as in this case, he is away from and inaccessible to his family a good share of the time; the difficulties of maintaining a household in town and carrying on research in an isolated village; the constant and frustrating parrying with petty officials who are in positions to cause all kinds of difficulty and delay; the virtual lack of social contact outside of one's family, employees, and the villagers among whom one works; the feeling of being merely tolerated by those among whom one works and upon whom one is dependent for most of his social interaction. In such circumstances research is likely to become the primary motivating principle and its progress looms large in one's world view. Therefore, to lose an assistant whose presence I deemed essential to the research, when I was on the threshold of tangible progress after a long period of preparation, was a discouraging blow. I shall not soon forget the anxiety I felt during the five-hour trek to the village alone after learning of Sharma's illness and incapacity. To await his recovery would have been to waste the best months for research because his illness came at the beginning of the winter slack season when people would, for the first time since my arrival, have ample time to sit and talk. In two months the spring harvest and planting season would begin and many potential informants would be too busy and tired to talk.

insights into the back region of the performance of his subjects. His subjects are evaluated by their fellows on the basis of the degree to which they protect the secrets of their team and successfully project the image of the team that is acceptable to the group for front-region presentation. It is probably often thought that this presentation will also satisfy the ethnographer. The ethnographer is likely to evaluate his subjects on the amount of back-region information they reveal to him, while he is evaluated by them on his tact in not intruding unnecessarily into the back region and, as rapport improves, on his trustworthiness as one who will not reveal back-region secrets. These are likely to be mutually contradictory bases of evaluation. Rapport establishment is largely a matter of threading among them so as to win admittance to the back region of the subjects' performance without alienating them. This is sometimes sought through admission to the subjects' team; it is more often gained through acceptance as a neutral confidant.

The impressions that ethnographer and subjects seek to project to one another are, therefore, those felt to be favorable to the accomplishment to their respective goals: the ethnographer seeks access to back-region information; the subjects seek to protect their secrets since these represent a threat to the public image they wish to maintain. Neither can succeed perfectly.

## Front and back regions

One must assume that the ethnographer's integrity as a scientist will insure the confidential nature of his findings about the individuals he studies. Those individuals, however, are unlikely to make such an assumption and, in fact, often make a contrary one. While I think it practically and ethically sound for the ethnographer to make known his intention to learn about the way of life of the people he plans to study, I believe it to be ethically unnecessary and methodologically unsound to make known his specific hypotheses, and in many cases even his areas of interest. To take his informants into his confidence regarding these may well preclude the possibility of acquiring much information essential to the main goal of understanding their way of life. I think here of my own interest in the highly charged sphere of inter-caste relations, where admission of the interest to certain persons or groups would have been inimical to the research effort.

Participant observation, as a form of social interaction, always involves impression management. Therefore, as a research technique it inevitably entails some secrecy and some dissimulation on the level of interpersonal relations. If the researcher feels morally constrained to avoid any form of dissimulation or secrecy he will have to forgo most of the insights that can be acquired through knowledge of those parts of his informants' lives that they attempt to conceal from him. With time, a researcher may be allowed to view parts of what was formerly the back region of his informants' performance, but few ethnographers can aspire to full acceptance into the informants' team in view of the temporary nature of their residence and their status as aliens. In a society where ascription is the only way to full acceptance, this is a virtual impossibility.

If the ethnographer does not gain access to back-region information he will have to content himself with an "official view" derived from public sources publicly approved, and his research interests will have to be sharply limited. An out for those sensitive on this point may be, of course, to do the research as it must be done but to use the findings only with the explicit approval of the subjects. In any case, the ethnographer will be presenting himself in certain ways to his informants during the research and concealing other aspects of himself from them. They will be doing the same. This is inherent in all social interaction.

## Teams and roles

Impression management in ethnographic research is often an exhausting, nerve-wracking effort on both sides, especially in the early phases of contact. Ethnographers may recognize themselves and their informants in this description:

> Whether the character that is being presented is sober or carefree, of high station or low, the individual who performs the character will be seen for what he largely is, a solitary player involved in a harried concern for his production. Behind many masks and many

women. Mohammed's association with me in my by then generally trusted status, precluded undue suspicion of missionary intent or governmental affiliation. Probably his most important characteristic with regard to rapport was his religion. As a Muslim he was, like me, a ritually polluted individual, especially since he was assumed to have eaten beef. For most purposes he and I were untouchables, albeit respected for our presumed wealth and knowledge.

With this description as background, the differential effects which my association with these two men had on the research can be analyzed. In discussing this topic special attention will be given to the implications of the status of each of them for the impressions we gave to villagers and received from them. Some of the more general problems of research in a tightly closed and highly stratified system will also be considered.

## Analysis

Erving Goffman, in *The Presentation of Self in Everyday Life*, has devised a description and analysis of social interaction in terms of the means by which people seek to control the impressions others receive of them. He has suggested that this "dramaturgical" approach is a widely applicable perspective for the analysis of social systems. In this scheme social interaction is analyzed "from the point of view of impression management."

> We find a team of performers who cooperate to present to an audience a given definition of the situation. This will include the conception of [one's] own team of [one's] audience and assumptions concerning the ethos that is to be maintained by rules of politeness and decorum. We often find a division into back region, where the performance of a routine is prepared, and front region, where the performance is presented. Access to these regions is controlled in order to prevent the audience from seeing backstage and to prevent outsiders from coming into a performance that is not addressed to them. Among members of the team we find that familiarity prevails, solidarity is likely to develop, and that secrets that could give the show away are shared and kept. (Goffman, 1959, p. 238)

The ethnographic research endeavor may be viewed as a system involving the social interaction of ethnographer and subjects. Considered as a basic feature of social interaction, therefore, impression management is of methodological as well as substantive significance to ethnographers.

The ethnographer comes to his subjects as an unknown, generally unexpected, and often unwanted intruder. Their impressions of him will determine the kinds and validity of data to which he will be able to gain access, and hence the degree of success of his work. The ethnographer and his subjects are both performers and audience to one another. They have to judge one another's motives and other attributes on the basis of short intensive contact and then decide what definition of themselves and the surrounding situation they want to project; what they will reveal and what they will conceal and how best to do it. Each will attempt to convey to the other the impression that will best serve his interests as he sees them.

The bases for evaluation by an audience are not entirely those which the performer intends or can control.

> Knowing that the individual is likely to present himself in a light that is favorable to him, the [audience] may divide what they witness into two parts; a part that is relatively easy for the individual to manipulate at will, being chiefly his verbal assertions, and a part in regard to which he seems to have little concern or control, being chiefly derived from the expressions he gives off. The [audience] may then use what are considered to be the ungovernable aspects of his expressive behavior as a check upon the validity of what is conveyed by the governable aspects. (Goffman, 1959, p. 7)

In their awareness of this, performers attempt to keep the back region out of the range of the audience's perception; to control the performance insofar as possible, preferably to an extent unrealized by the audience. The audience will attempt to glimpse the back region in order to gain new insights into the nature of the performance and the performers.

An ethnographer is usually evaluated by himself and his colleagues on the basis of his

> the audience are referred to [in their absence] not even by a slighting name but by a code title which assimilates them fully to an abstract category. (Goffman, 1959, pp. 172–3)
>
> Perhaps the cruelest term of all is found in situations where an individual asks to be called by a familiar term to his face, and this is tolerantly done, but in his absence he is referred to by a formal term. (Ibid., p. 174)

Had I been alone in the village I would have had a relatively free hand in attempting to determine whom I associated with, so long as I did not infringe too freely on village backstage life or on matters of ritual purity. However, since I was in almost constant association with an assistant whose performance was closely tied to my own, my status and his were interdependent. The definition of ourselves which we cooperated in projecting had to correspond to known and observable facts and clues about ourselves and our purposes. Since to villagers my assistant was more conventional and hence comprehensible as a person than I, it was largely from him that impressions were derived which determined our status. It is for this reason that the characteristics of the interpreter-assistant were of crucial significance to the research effort.

### *The Brahmin assistant*

Sharma, the Brahmin assistant, was able to establish himself before villagers as a friendly, tactful and trustworthy young man. As such he was well-liked by high-caste villagers and was respected by all. Once his plains Brahmin status had been verified, it affected the tenor of all his relationships, and consequently of the ethnographic team's relations with villagers. The effects of these relationships on the research derived from his own attempts at impression management as a performer before several audiences, and from the attempts by villagers to control his impressions of them.

Most importantly, Sharma was a Brahmin of the plains. As such, he felt obliged to convey an acceptable definition of himself in this role to the villagers among whom he worked and to the ethnographer for whom he worked. Before villagers he was obliged to refrain from extensive informal contacts with his caste inferiors. He was expected to refuse to participate in such defiling activities as consumption of meat and liquor, and was in general expected to exemplify the virtues of his status. He was, in this context, acting as the sole local representative of plains Brahmins, a group with which he was closely identified by himself and by villagers.

In the presence of the ethnographer he joined a larger team, or reference group, of high-caste Indian Hindus. In this role he wished to convey a definition of Hinduism that would reflect well on its practitioners in the eyes of the foreigner. When possible he demonstrated an enlightened, sophisticated, democratic Hinduism quite unlike that indigenous to the village. Since, as a Hindu, he considered himself a teammate of villagers, he felt obliged to convey to the ethnographer an impression of village affairs that was not too greatly at variance with the notion of Hinduism which he wished to convey. He was, therefore, reluctant to discuss matters which might contradict the impression he had fostered – especially high-caste religious practices and inter-caste relations, the areas of most flagrant deviation (from his point of view) from the Hindu ideal. He tended, probably unconsciously, to color his accounts and structure our interactions with villagers to bias the impressions I received in this direction. On behalf of the ethnographic team, he was intent upon winning the villagers' acceptance and confidence, a fact which colored his accounts of us to them. His skill at impression management was evidenced by the rapport he achieved with both the ethnographer and the villagers, and by the fact that I, as ethnographer, was largely unaware of his manipulation of impressions until later when I had access to information without his management.

### *The village team*

Villagers, too, had particular definitions of themselves that they wished to convey to the ethnographic team determined, to a large extent, by their interpretation of the nature and motives of this team. With a Brahmin in an important position on the team, low-caste people were reluctant to have close contact with it. High-caste people, on the other hand, were eager to demonstrate the validity of their

> characters, each performer tends to wear a single look, a naked unsocialized look, a look of concentration, a look of one who is privately engaged in a difficult and treacherous task. (Goffman, 1959, p. 235)

The task is especially difficult and treacherous when the cultural gap between participants and audience is great. Then the impression that a given action will convey cannot always be predicted; audience reaction is hard to read and performance significance is hard to judge. Misinterpretation occurs frequently and sometimes disastrously in such circumstances. Anyone who has been in an alien culture can cite *faux pas* resulting from such misinterpretation. Inadvertent disrespect is a common type. Although no vivid example occurred in the research being reported here, largely due to an exaggerated caution about this, the author experienced such a misinterpretation in the course of research among the Aleuts. He once amused local children by drawing cartoon faces on the steamy windows of the village store. These were seen by an adult who interpreted them as insulting caricatures of Aleuts, although they were in reality generalized cartoons, totally innocuous in intent, and he reacted bitterly. He saw them in the light of unhappy past experience with arrogant non-Aleuts. Strained relations resulting from this incident could well have halted research had it not occurred late in the research effort, after most villagers had been convinced of the ethnographer's good intentions and friendly attitude.

In a tightly closed and highly stratified society the difficulty of impression management is compounded. In a closed society the outsider may be prevented from viewing the activities of its members almost completely. The front region is small and admittance to any aspect of the performance is extremely difficult to obtain. Pronounced stratification makes for many teams, many performances, many back regions (one for each performance group, as well as for each audience), and considerable anxiety lest one group be indiscreet in revealing the "secrets" its members know of other groups.

In Sirkanda the ethnographic team consisted of the anthropologist, an interpreter-assistant and, as a peripheral member for part of the time, a houseboy. This was a team in that it constituted

> a set of individuals whose intimate cooperation is required if a given projected definition of the situation is to be maintained. (Goffman, 1959, p. 104)

Villagers considered it to be a team. In their eyes the actions of each member reflected on the others.

## *The ethnographer*

The initial response to an ethnographer by his subjects is probably always an attempt to identify him in familiar terms; to identify him as the performer of a familiar role. The impressions he makes will determine how he is identified.

In Sirkanda several roles were known or known of, under which strangers might appear, and each – missionary, tax collector or other government agent, spy – was for a time attributed to our ethnographic team by some or all villagers as being our real, i.e., back-region role. None of these was a suitable role for accomplishing our purposes and it was only by consistently behaving in ways inconsistent with these roles that we ultimately established a novel role for ourselves: that of scholars eager to learn what knowledgeable villagers could teach us about Pahari culture. I drew heavily on the familiar role of student, and my associates on the familiar role of employee or "servant." Foreign origin was an important aspect of my status, for I was a "sahib" and an "untouchable"; a person of relative wealth and influence but of ritually impure origin and habits.

For me the former was a more distressing status than the latter, but an equally inevitable one. I was always referred to as "the sahib" by villagers, although I succeeded in getting them not to address me as such. Goffman comments on the differences between terms of address and terms of reference in this context noting that

> . . . in the presence of the audience, the performers tend to use a favorable form of address to them. . . . Sometimes members of

With a Brahmin on the ethnographic team and with high-caste people as our associates, low-caste villagers were disinclined to associate with us, much less to reveal backstage information. We were in their view associates of the high-caste team and as such were people to be treated cautiously and respectfully. High-caste villagers could not reveal such information to us either because we were, in their view, members of the plains Brahmin team and a source of potential discredit to high-caste Paharis.

Ethnographic information that was acquired in this context was largely of a sort considered innocuous by villagers – observations about the weather and current events, agricultural techniques, etc. Much of it was distorted. For example, our initial genealogies omitted all reference to plural wives; accounts of marriage and other ritual events were sketchy and largely in conformance with the villagers' conception of plains orthodoxy. Some of the information was false. Most of it was inaccessible. The back region was large and carefully guarded. Yet relations between the ethnographic team and the village were relatively congenial.

### *The Muslim assistant*

When after four months the Brahmin assistant was replaced by a Muslim, there were important consequences for the villagers' conception of the ethnographic team and consequently for their performance before that team. The progress and results of the research reflected these changes.

Mohammed, the Muslim assistant, was respected for his age and learning, liked for his congeniality and wit, but doomed to untouchable status by his religion. This did not disturb him. As an educated and not particularly religious Muslim he had little personal involvement in the caste hierarchy of the village and little vested interest in the ethnographer's impression of village Hinduism. As an individual he was objective and interested but concerned more with projecting to villagers a favorable view of the ethnographic team than any particular image of his personal status. As a performer he played a less prominent role than his predecessor. This was reflected in his interpreting. Sharma had preferred to interpret virtually all statements and to direct the course of conversation to keep from offending villagers (and embarrassing himself) by treading on dangerous ground. Mohammed was anxious that communication between ethnographer and subjects be as direct as possible; that conversation be as undirected as possible except when particular topics were being pursued. Consequently interpreting occurred only as necessary; ethnographer and subjects determined the direction of conversation.

As an audience, the Muslim's effect on the village performance was drastically different from the effect of the Brahmin. High-caste people did not wish to associate openly with a Muslim, for he was by definition ritually impure. He was in no sense their fellow team member as the Brahmin had been; he was in some respects almost as alien as was the ethnographer himself. Consequently, high-caste villagers' behavior became correct but distant: informal conversations and visitations decreased in frequency; the ethnographer was told in private by some high-caste villagers that they could no longer associate closely with him.

Low-caste people, on the other hand, became less inhibited than formerly was the case. When by experimentation they found that the Muslim was apparently oblivious to caste, these people began to be friendly. In the vacuum of social interaction left by withdrawal of the high castes, they were not rebuffed. The effect was circular and soon the ethnographer's dwelling became identified as primarily a low-caste area.

Not all high-caste people were alienated, but most preferred to talk in their own homes, with low castes excluded, rather than in the ethnographer's house. Some would visit the ethnographer only when they had been assured that no low-caste villagers would be present.

In these circumstances the village no longer presented the aspect of a unified team. Now ir became clear that the village was divided. From the point of view of the high castes there were at least two teams: low and high castes. The former feared the power of the latter; the latter feared the revelation of back-region secrets that might be given by the former. From the point of view of low castes there seem to claims to high-caste status before this patently high-status outsider.

Pahari Brahmins and Rajputs (the high castes of this area) customarily do many things that are unacceptable in high-caste plains circles. As a result they are denied the esteem of such people. The appellations "Pahari Brahmin" and "Pahari Rajput" are often used in derision by people of the plains. Among other unorthodox activities, these Paharis sacrifice animals, eat meat, drink liquor, are unfamiliar with the scriptures, largely ignore the great gods of Hinduism, consult diviners and shamans, fail to observe many of the ceremonies and ritual restrictions deemed necessary by high-caste plainsmen, take a bride price in marriage, marry widows, are not infrequently polygynous (and in some areas are polyandrous), occasionally marry across caste lines, share wives among brothers, "sell" women to men of dubious character from the plains. In order favorably to impress a plains Brahmin they must conceal these activities insofar as possible, and this they indeed do. Just as Sharma wished to convey an impression of enlightened Hinduism to the ethnographer, villagers wished to convey their idea of enlightened Hinduism to Sharma. The two aims were complementary. Both resulted in projection of an exaggerated impression of religious orthodoxy. This exaggeration of behavior, indicating adherence to the "officially accredited values of the society," is a feature characteristic of impression management before outsiders (cf. Goffman, 1959, p. 35).

Impression management of this kind is especially difficult when the intended audience, as in the case of the ethnographic team, has a known or suspected interest in the detection of back-region attitudes and behaviors, and when it is in intimate association with the performers.

Virtually the entire village of Sirkanda was at first a back region for the ethnographic team: a great deal of the conventional behavior therein was back-region behavior. Attempts were made by villagers to avoid "inopportune intrusions" which Goffman describes as follows:

> When an outsider accidentally enters a region in which a performance is being given, or when a member of the audience inadvertently enters the backstage, the intruder is likely to catch those present *flagrante delicto*. Through no one's intention, the person present in the region may find that they have patently been witnessed in activity that is quite incompatible with the impression that they are, for wider social reasons, under obligation to maintain to the intruder. (Goffman, 1959, p. 209)

When, for instance, an opportunity arose for the ethnographic team to move from a buffalo shed on the periphery of the village to a house in its center, villagers' desire to maintain a modicum of overt hospitality wavered before their covert alarm until an untouchable was induced to place an objection before the potential intruders. The objection had the desired effect although it was immediately repudiated by its high-caste instigators, who blamed it upon the irresponsible meddling of a mere untouchable. They thus assured the continued privacy of the village while maintaining their front of hospitality. The untouchable who voiced the objection had been coerced and bribed with liquor to do so. He later commented that villagers had said that people, and especially women, would be inhibited in the performance of their daily rounds if strangers were to be continuously in their midst; that is, the backstage would be exposed to the audience.

Before the ethnographic team the village at this time presented an apparently united front. Villagers of all castes cooperated not only in concealing things inimical to the high-caste performance, but also those thought to reflect adversely on the people as a whole. For example, an intra-caste dispute among untouchables came to a head at a high-caste wedding where the disputants were serving as musicians. While the disputants were presenting their case to an informal council of high-caste guests which had convened one afternoon, a heated argument erupted. It was suppressed and the council disbanded with the explicit warning that the ethnographer would hear and think ill of the village.

During this period of the research, untouchables were usually relegated to an unobtrusive secondary role, largely in the back region.

ciated with it. But the range of such back-region secrets among low castes is limited in comparison to that among high castes. It does not extend to Pahari practices as such, but instead is limited primarily to those few practices crucial to their status competition with other low castes in the village and, even more importantly, to negative attitudes toward the high castes – attitudes which must be concealed in view of the power structure of the society.

Goffman notes that

> ... to the degree that the teammates and their colleagues form a complete social community which offers each performer a place and a source of moral support..., to that degree it would seem that performers can protect themselves from doubt and guilt and practice any kind of deception. (Goffman, 1959, pp. 214–15)

It is because, in this highly stratified society, moral support and rewards are allotted on the basis of caste that high-caste performers cannot trust their low-caste colleagues to sustain the performance – to practice the deception – voluntarily. Low-caste people resent their inferior position and the disadvantages which inhere in it (cf. Berreman, 1960a). Not only are they uncommitted to the village performance which is largely a high-caste performance; they are in private often committed to discrediting some aspects of this performance. Both of these are facts of which the ethnographer must be aware. As a result of them, if low-caste members feel they can do so in safety, they are not reluctant to reveal information about village life which embarrasses high-caste villagers. They may also, of course, manufacture information intended to discredit their caste superiors, just as the latter may purposely purvey false information to justify their treatment of low castes. The ethnographer must be constantly alert to the likelihood of such deceptions, using cross checks, independent observation and the like for verification. Eventually he can identify reliable informants and the subjects upon which particular informants or categories of informants are likely to be unreliable.

That high-caste people recognize the vulnerability of their performance and are anxious about it is revealed in their suspicion and resentment of low-caste association with outsiders, such as the ethnographic team. Anyone who associates too freely with such outsiders is suspected of telling too much, but only low-caste villagers are suspected of telling those facts which will seriously jeopardize the status of the dominant high castes. The suspicion that low castes are not entirely to be trusted to keep up the front is therefore not paranoia on the part of those they may reveal; it is a real danger. On the other hand, high-caste members encourage association between strangers and low castes by sending the latter to appraise strangers who come to the village and, if possible, to send them on their way. By so doing high castes avoid the risks of being embarrassed or polluted by the aliens. At the same time they increase low-caste opportunities for outside contact, acquisition of new ideas, etc., and they thereby increase their own anxieties about low-caste behavior and attitudes. They are apparently more willing to face this anxiety than to risk initial personal contact with strangers. As a result, some low-caste people are more at ease with strangers and more knowledgeable about them and their thought patterns, than are most high-caste people.

Since they are not willing to extend to low castes the status, power, and material rewards which would bring them into the high-caste team or commit them to the high-caste performance, high castes rely heavily on threats of economic and physical sanctions to keep their subordinates in line and their secrets, which these people know, concealed from outsiders. To the extent that low-caste people do sustain the performance they are evidently responding to their fear of high-caste reprisals more than to an internalized commitment to the performance.

## *High-caste teams*

Even high-caste villagers do not present a united front or consistent performance on all matters. Bride-price marriage, for example, is traditional in these hills and until recent times only poverty accounted for failure to pay for a bride. To high-caste people of the plains bride-price marriage is reprehensible; a dowry is always demanded. This attitude has had its have been at least three teams: high castes, "our caste" and (other) low castes. High castes were feared and resented; other low castes were to some extent competitors for status before outsiders. Competition took the form of conflicting claims as to the type and nature of interaction with one another, each caste claiming to treat as inferiors (or sometimes as equals) others who, in turn, claimed equal or superior status. Actually, in the context of the closed village a good deal of interaction took place among low castes with few status considerations.

### *Low-caste teams*

The position of low castes – the untouchables – was an interesting one relative to the village team and its performance. Untouchables were in a position such that they might easily admit an audience to backstage village secrets. They were members of the village team perforce, but they were uneasy and not fully trusted members. Goffman has appropriately stated that:

> One overall objective of any team is to sustain the definition of the situation that its performance fosters. This will involve the over-communication of some facts and the under-communication of others. Given the fragility and the required expressive coherence of the reality that is dramatized by a performance, there are usually facts which, if attention is drawn to them during the performance, would discredit, disrupt, or make useless the impression that the performance fosters. These facts may be said to provide "destructive information." A basic problem for many performances, then, is that of information control; the audience must not acquire destructive information about the situation that is being defined for them. In other words, a team must be able to keep its secrets and have its secrets kept. (Goffman, 1959, p. 141)

In Sirkanda, low-caste people are in a position to know high-caste secrets because all villagers are in almost constant contact with one another; they have little privacy. Castes are not separated physically, socially, or ritually to the extent that they are in many areas. Low-caste and high-caste cultures, including back-region behavior, proved to be very similar among these hill people (cf. Berreman, 1960b). But, for low-caste people the back region – the part that is to be concealed – is much smaller than for high-caste people. They do not feel obligated to protect village secrets to the extent that high-caste people do simply because their prestige and position are not at stake. They do not share, or are not heavily committed to, the "common official values" which high-caste people affect before outsiders. High-caste men, for example, were careful to conceal the fact that, in this society, brothers have sexual access to one another's wives. However, a low-caste man who had listed for the ethnographer the name and village of origin of the women of his family, including his wife and his brothers' wives, was not embarrassed to remark, when asked which was his wife, that "They are all like wives to me." A more striking contrast was evidenced in attitudes toward village religious behavior. After some time, low-caste people encouraged the ethnographer to attend their household religious observances wherein possessed dancing and animal sacrifice occurred. High-caste villagers did not want the ethnographer to be present at their own performances of the same rituals. Some of them also objected to my presence at the low-caste functions and exerted pressure to have me excluded. The reason was apparently that high-caste people felt such behavior, if known outside, would jeopardize their claims to high status. High-caste people, recognizing that village culture was essentially the same in all castes and that I was aware of this, felt their position threatened by the performance of the low-castes. Low-caste people had no such status to maintain.

Low-caste people, unlike their high-caste village-mates, had little prestige at stake in outsiders' conceptions of the Pahari way of life. They were not competing with plains people for status nor seeking acceptance by them to the extent that the high castes were. People assume the worst about untouchables so they have little to gain by concealment. This is not to say that there is no particular definition of their situation that untouchables try to project, or that it takes no effort to perpetuate it; the lowest-status group in Sirkanda, for instance, has tried to suppress its reputation for prostitution by giving up some of the activities asso-

The same kinds of statements can be made about particular low castes, although the low castes as a group rarely cooperate to put on a team performance. Usually, each low caste sustains its own performance, attempting to substantiate its claims to status relative to other low castes adjacent in the hierarchy.

[. . .]

## Data, secrets, and confidence

As rapport increased and back-region information accumulated it became possible for the ethnographic team to accomplish useful research on a broader scale – to understand formerly incomprehensible activities and attitudes; to relate previously disparate facts, to make more sensible inquiries, to cross-check and verify information. The effect was cumulative. As we learned more, more information became accessible. By being interested, uncritical, circumspect, and meticulous about maintaining their trust, we won villagers' confidence. For example, high-caste people who avoided close contact with Mohammed in the village visited his home in town and even ate with him, with the plea that he tell no one in the village. No one ever discovered these indiscretions, and those who committed them were not unappreciative. Contrary to villagers' early fears, no missionaries, policemen, tax officers, or other outsiders came to Sirkanda as a result of what we learned there. We tried to show our increasing knowledge in greater comprehension of our environment, rather than by repetition of items of information. As we learned more, concealment from us decreased because we were apparently already aware of, and largely indifferent to, many of the facts about which villagers were most self-conscious or secretive. We took for granted things some villagers supposed were "dark secrets" (i.e., things contrary to the impression they hoped to convey to us) and far from our knowledge (cf. Goffman, 1959, p. 141). When we had asked, in genealogical inquiry, what a man's wife's name was, we always got one name. When we later found that polygyny was not uncommon we asked first how many wives a man had and thereby got accurate information. Most villagers were unaware that our interests went beyond formal genealogical records, economic techniques and ritual observances. Many secrets were revealed largely because of the apparent casualness of our interest in them, and because villagers had become accustomed to our presence in the village so that we were not considered to be as critical an audience as had once been the case.

Some of the most revealing instances of social interaction occurred between people who were apparently oblivious to the fact that the ethnographer was present. Frequently this was a temporary lapse. A performance for the ethnographer would be abandoned as tension, conviviality, concentration on a topic of conversation, or some other intensification of interaction occurred among the participants. Such instances of preoccupation with one another were conspicuous by the fact that attitudes were expressed or information divulged that would normally be suppressed. The breach in the performance would sometimes be followed immediately, or after some time, by embarrassment, apology, or anxious efforts to counteract its presumed effect on the ethnographer's view of the village or of those involved in the incident. Minor instances of the same phenomenon were frequent sources of insight into the functioning of the society, and sources of confirmation or contradiction of informants' data.

The accuracy of information on back-region subjects could often be checked through informants who would not have intentionally revealed it, by bringing the subject up naturally in conversation as though it were a matter of general information. That is, it was defined by the ethnographer as no longer restricted to the back region of the performance to which he was audience.

Some "secrets," however, could not be adequately verified simply because to do so would precipitate difficulty for all concerned, especially for those who would be suspected of revealing the secrets. Such secrets ranged from gossip about various past transgressions and indiscretions by particular families or individuals, to the fact that villagers of all castes were reported to eat occasionally the flesh of animals such as deer and goats found freshly dead or killed in the forest. One low-caste man told me there were secrets he could not reveal until I had my pack on my back and was effect in the hills so that Paharis, and especially those of high caste, not infrequently forgo the bride price in a wedding. There was an interesting division of expressed attitude among high-caste villagers in Sirkanda on this matter. Although there was no consistent difference among families in practice, some claimed that their families would never accept or demand a bride price while others claimed that their families would never give or take a bride without an accompanying bride price. I was unsuccessful in attempting to account for this difference in terms of the economic, educational, or other readily apparent characteristics of those concerned. I finally realized that it was largely a function of the relationship of the particular informant to me and my assistant and, more specifically, the impression the informant wished to convey to us. Many wanted to convey a picture of plains orthodoxy and, not realizing that we knew otherwise, or hoping that we would think their families were exceptions, they tailored their accounts of the marriage transaction to fit this. A few, notably some of the older men of the Rajput landowning caste, wanted to convey their conception of the proper Pahari tradition, perhaps in view of the fact that they knew we were aware of their practice of bride-price marriage and that to conceal it was by then useless. They expressed disapproval of dowry marriage and disclaimed willingness to be parties to such arrangements. They explained that as Rajputs they would not take charity (as a Brahmin would) and would insist on paying for anything they got, including a wife; conversely they would require payment for their daughters, because one does not give charity to other Rajputs. Moreover, gift brides die young and do not produce heirs, they asserted. Some villagers were more frank than either of the above groups when they got to know us, and described quite freely the specific circumstances under which bride price and dowry were and were not included in recent marriage transactions.

On at least one occasion highly controversial information was revealed by a Rajput because of an erroneous assumption on his part that others in his caste had been telling the ethnographer the story in a manner uncomplimentary to himself. Early in the research I learned that the village had been riven by a legal battle over land begun some twenty years previously, and although I knew in a general way who and what was involved, I had not ascertained the details. One evening the proudest, most suspicious and tight-lipped of the members of the winning faction appeared unexpectedly at my house, lantern in hand, and without introduction proceeded to recount the nine-year legal battle blow-by-blow. He was evidently attempting to counteract information which he presumed the losing faction had given me. I was subsequently able to check his version with several other versions from both sides in order to reconstruct approximately the factors involved in this complex and emotionally loaded episode.

Thus, high-caste members are not entirely free of suspicion and doubt regarding the extent to which they can rely upon their teammates to sustain their performance. Even among high castes there are different performances which various groups try to project to one another and occasionally to outsiders. Lines of differential performance and impression management among them most often follow kin group and caste affiliation. These high-caste performance teams are usually factional groups in the village, competing and disputing with one another. They often attempt to disparage one another within the high-caste context by such means as questioning purity of ancestry. The head of the largest family in Sirkanda, a member of one of the two large Rajput sibs of the village, expressed doubt that the other large sib, to which his wife (the mother of his five adult sons) belonged, was actually and legitimately a Rajput sib. This was a recurrent theme. Often cleavages between high-caste groups involved long-standing disputes over land and/or women.

High-caste performance teams also differed from one another in the age, sex, education, and outside experience of their members. Groups so defined can be described as performance teams because they differ in the definitions of their own and the village situation which they attempt to project to various audiences. Rarely, however, do they desert the high-caste team before outsiders or low castes, the two most crucial audiences.

doubt one of the most anxiety-producing situations known to man is to make public that which he considers to be private, back-region behavior.

[...]

### *Impression management in pursuit of research*

Finally, my own behavior was tailored for my village audience. I carefully and, I think successfully, concealed the range of my interests and their intensity in some matters – such as inter-caste relations. I refrained from going where I was not wanted, even when I could have gone without being challenged and when I very much wanted to go. One instance occurred when I decided not to move into the proffered house in the center of the village. As another example, I never attended a village funeral. On the two occasions upon which I could have done so, I found that there was considerable anxiety lest my presence upset guests from other villages, though Sirkanda villagers claimed they would welcome my presence. There was evident relief when I stayed home.

In the village I concealed the extent of my note-taking, doing most of it at night or in private. I felt free to take notes openly before only a few key informants, and then only after I had known them for a considerable time. I recorded some kinds of detailed information, such as genealogies and crop yields, in the presence of all informants when I found that I could do so without inhibiting responses appreciably. This, too, took time and circumspection. Some subjects, such as ceremonial activities, could be freely recorded before some informants and not at all before others. I discarded my plans to use scheduled interviews and questionnaires because I thought they would do more harm in terms of rapport than good in terms of data collection, in view of village attitudes and my relationship with villagers. I never took photographs without permission. I concealed such alien practices as my use of toilet paper – a habit for which foreigners are frequently criticized in India. I took up smoking as a step to increase rapport. I simulated a liking for millet chapaties and the burning pepper and pumpkin or potato mixture which makes up much of the Pahari diet. Even more heroically, I concealed my distaste for the powerful home-distilled liquor, the consumption of which marked every party and celebration. Such dissimulations were aimed at improving rapport and they were worth the trouble. In this behavior a front was maintained in order to sustain a particular definition of my situation; a definition which I thought would increase my access to village backstage life, thereby contributing to the ultimate goal of understanding the lifeways of these people.

## Conclusion

In such a society as this the ethnographer is inevitably an outsider and never becomes otherwise. He is judged by those among whom he works on the basis of his own characteristics and those of his associates. He becomes identified with those social groups among his subjects to which he gains access. 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 the confidential regions of the life of those who extend it. The stranger will be excluded from a large and vital area if he is seen as one who will not safeguard secrets, and especially if he is identified as a member of one of those groups from which the secrets are being kept.

Sharma was a high-caste plainsman and consequently identified with very important groups in the village, groups rigorously excluded from large areas of the life of both high-caste and low-caste villagers. As such he could likely never have achieved the kind of relationship to villagers which would have resulted in access to much of the life of Sirkanda. Access to that information was essential to the ethnographer because it constituted a large proportion of all village attitudes and behaviors. Mohammed was able to gain substantial rapport with the low castes. In view of the attitudes of villagers and the social composition and power structure of the village, the low castes were the only feasible source of information which high-caste villagers considered to be embarrassing, leaving the village permanently. He was afraid that some intimation of my knowledge might leak out and he would be punished as the only one who would have revealed the damaging information. After I had said my final farewells this man journeyed sixteen miles to my home in town, primarily, to be sure, to get some utensils I had offered him, but partly to tell me some incidents which he had been afraid to tell or even hint at during my residence in the village and which he would not tell in the presence of my assistant or any other person. These incidents had to do primarily with the sensitive area of inter-caste and other illicit sexual behavior among powerful members of the community.

To this man, as well as to other low-caste friends, the ethnographer had become what Goffman refers to as a "confidant," one who is located outside of the team and who participates "... only vicariously in back and front region activity" (Goffman, 1959, p. 159). In this role I had access to a range of information not often accessible to those who came from outside the group. Where group membership is by ascription this seems to be the only feasible role for which the ethnographer may strive.

Certain secrets remained too dark to be told even by those who trusted us most. The village remained a team, united in its performance, with regard to some practices or beliefs which were too damaging to all (or to certain powerful high-caste people) to permit their revelation to an outsider. Obviously, like the perfect crime, most of these remain unknown. Indications of a few of them were received, however. For example, one old dispute which resulted in a factional split among Rajputs would have escaped me had not an old man referred to it briefly, bitterly, and inadvertently. Despite my best efforts I learned nothing about it beyond his chance remark that it involved a man and woman of the disputing sibs seen talking and laughing together at the water source some generations ago. Even the most willing informants would only say that "Those people are all dead now so it doesn't matter."

I learned that some Paharis occasionally sacrifice a buffalo to their gods, but that this has never occurred in Sirkanda. I was convinced that this was the case when considerable inquiry and observation seemed to verify it. Then, shortly before my final departure, a dog deposited the embarrassing evidence of such a sacrifice – the neatly-severed head of a buffalo calf – on the main village trail shortly before I chanced by. Villagers of all castes refused to discuss the matter in which all were obviously implicated. My one opportunity for a candid explanation occurred at the moment of discovery when I asked a child at my heels which god the buffalo had been sacrificed to. A reply seemed imminent until his elder, a few steps back on the trail, caught up and silenced the discussion as well as all chance for future fruitful inquiry on the subject. Villagers thought that to plains people this would seem akin to cow-killing, the greatest sin of all, and so it had to be rigorously concealed.

The sacrifice had evidently occurred during my absence from the village.

> If an individual is to give expression to ideal standards during his performance, then he will have to forgo or conceal action which is inconsistent with these standards. When this inappropriate conduct is itself satisfying in some way, as is often the case, then one commonly finds it indulged in secretly, in this way the performer is able to forgo his cake and eat it too. (Goffman, 1959, p. 41)

It was six months after my arrival before animal sacrifices and attendant rituals were performed in my presence, although they had been performed in my absence or without my knowledge throughout my residence in the village. Likewise, it was not until after Sharma left that I witnessed drinking and meat-eating parties in the village.

Since I left the village for two or three days every week or so, there was an opportunity for essential and enjoyable back-region activity to be carried on quite freely in my absence, and this opportunity was not neglected. In fact, it probably made my research in the village much more bearable to villagers than if I had been there constantly. It was largely the threat to their privacy that motivated villagers to make sure that I did not take up residence in the center of the village (as described above), but continued instead to live on its periphery. No

# 10

# Ethnographic Seduction, Transference, and Resistance in Dialogues about Terror and Violence in Argentina

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Anthropology has witnessed in recent years a growing interest in the study of violence. Most of these studies derive their research date from interviews with victims traumatized by violence, not from the direct observation of the violence itself.[1] What is the influence of the violence to which the informants have been exposed on their relationships with ethnographers, and what methodological adjustments should researchers make in these emotionally charged encounters? How are anthropologists themselves affected by listening to accounts of traumatizing events? How may their reactions interfere with the gathering of research data, and, finally, what can be done to recognize and cope with such reactions?

In this article I shall indicate the relevance of psychoanalytic practice to these issues through a discussion of the interview problems with victims and perpetrators of violence during more than two years of fieldwork on the contested historical reconstruction of the so-called dirty war in Argentina. I will focus on the complex social and transferential relationship with my interviewees, and delineate in which ways they tried to influence my understanding of the political violence of the 1960s and 1970s.

Anthropologists can learn much from psychoanalysts on how to interact with victims of violence, torture, and terror. For instance, the treatment of Jewish holocaust survivors has shown that analysts should initially refrain from the common practice of probing for any deeper meanings hidden in the manifest discourse. The analyst should begin the therapeutic relationship by accepting at face value whatever is told about life in the concentration camps and, in some cases, even fill in the gaps. Victims of extreme violence have often lost all trust in other human beings, and their accounts have met with disbelief. They have to be convinced that their analyst believes them, and that he or she can endure the pain their accounts evoke, before they will enter into an intensive working alliance. Ethnographers who interview victims and perpetrators of violence will encounter similar perusals of credibility and trust, and will also need to adapt their research practices to such unusual circumstances.

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damaging or secret. They were a reasonably satisfactory source of such information about the entire village because all castes were in such close contact that they had few secrets from one another and did not differ greatly in culture. This is not to say that the information obtained was complete or totally accurate, but only to assert that it was much more so than would have been the case had Sharma been my assistant throughout the research.

Thus, there is more than one "team" which makes up Sirkanda; more than one definition of the village situation is presented or may be presented to the outsider. As the ethnographer gains access to information from people in different social groups and in different situations he is likely to become increasingly aware of this.

The question of whether the performance, definition or impression fostered by one group is more real or true than that put forth by another, or whether a planned impression is more or less true than the backstage behavior behind it, is not a fruitful one for argument. All are essential to an understanding of the social interaction being observed.

half-truths by torturers and have sympathy for a critical stance toward their masters, but we raise our eyebrows at probing into the words of their victims. I have the same sympathies and reservations. However, as the psychoanalytic treatment of victims of violence has shown, people may repress and sublimate traumatic experiences. If done responsibly and with the highest ethical care, we should not shy away from reaching into the depths of human suffering.

Much has been written about the cultural presuppositions and blind spots of anthropologists that may skew their observations, and we are also conscious of the impression management, mistrust, deceit, exaggeration, backstage defenses, and even disinformation by informants who try to shield sensitive knowledge from the inquisitive fieldworker. Such circumventions are generally regarded as obstructing rather than contributing to a greater ethnographic understanding. Anthropology simply lacks a theory of concealment that transforms these obstructions into a deeper cultural insight. Psychoanalysis, on the other hand, has placed concealment at the center of its theory and method. Its interpretation of how analysands try to avoid discussing their traumas is instructive to anthropology. Close attention to the ways in which informants may use psychological processes to lead unaware ethnographers away from certain areas of knowledge may therefore contribute to greater ethnographic insight and improve anthropological research.

There is an additional problem with accounts of violence. Traumatic experiences can only reach us through the distortion of words. As Young has said about our understanding of the Jewish holocaust:

> What is remembered of the Holocaust depends on how it is remembered, and how events are remembered depends in turn on the texts now giving them form. . . . This is not to question the ultimate veracity in any given account, but it is to propose a search for the truth in the interpretation intrinsic to all versions of the Holocaust: both that interpretation which the writer consciously effects and that which his narrative necessarily accomplishes for him. (1988:1–2)

Moreover, these traumatic experiences are related in a dialogue between victim and ethnographer whose proper dynamic influences their rendition. This encounter should be built on empathy, trust, and openness, without forgetting that the oral account is a discursive and interactional edifice of meaning that inevitably fails to do justice to the emotions and experiences of the narrator. If we do not acknowledge that we can only understand torture and abuse through the inadequacies of language, and if we furthermore shirk away from our own psychological reactions to their description and do not examine the social dynamic between us and our informants, then we may unwillingly harm these victims by simply reproducing their accounts as told.

Although it will become clear in this article that I have drawn extensively on the professional experiences of psychoanalysts who have worked with victims of violence, there are two important differences that deserve mentioning. First of all, psychoanalysts are called upon to relieve suffering. Ethnographers, on the other hand, approach people to evoke their traumas but can do nothing to lessen the pain that ensues from reliving them. I must confess that this burden was sometimes a heavy load to carry during my research in Argentina, now and then provoked feelings of guilt, and made me decide more than once not to ask any further about certain traumatic experiences.

A second difference between psychoanalysis and ethnographic research is that psychoanalysts and their analysands work under strict confidentiality, while ethnographers are expected to report their findings. My research in Argentina had the complicating factor that it consisted mainly of interviews with prominent public figures who did not ask for confidentiality but, instead, wanted their opinions to be widely known. Many persons I interviewed in Argentina were aware of the potential personal, political, and even legal repercussions of their accounts. Most interviewees were representative members of the armed forces, the Roman Catholic Church, human rights groups, and former guerrilla organizations who were, after the 1983 turn to democracy, in the middle of a heated public debate about the years of repression. I do not doubt that many shared their views with

Anthropologists can also benefit from the psychoanalytic understanding of transferential phenomena in emotionally charged encounters. Much has been written in psychological anthropology about the problems of transference and countertransference in the relationship between ethnographers and informants, and calls for a heightened psychological reflexivity about data gathering and even preparatory therapy before entering the field continue to be made (Crapanzano 1994:236–42; Luhrmann 1994:74–7). In this article I want to elaborate on these interactional and transferential problems by drawing attention to ethnographic seduction as a complex dynamic of conscious moves and unconscious defenses that may arise in interviews with victims and perpetrators of violence. I use the term *seduction* to denote ways in which interviewees influence the understanding and research results of their interviewers. This seduction is carried out most effectively, but not exclusively, by way of the ethnographer's unconscious countertransferential reactions. Seduction prevents interviewers from probing the discourse of the interviewee and, instead, makes them lose their critical stance toward the manifest discourse. George Devereux (1967:44–5) has also used the term *seduction* in his discussion of countertransference among anthropologists. However, he only refers to an unconscious emotional attraction, while I have extended the concept to the conscious utilization of the complex social, emotional, dialogic, and transferential dynamic between ethnographer and informant.

I first became aware of the importance of this dynamic through the elaborate display of *caballerosidad* (gentlemanlikeness) and the barrage of kindness and courtesy bestowed on me by several Argentine generals. I soon identified this attention as *seducción* and began to recognize it also in my meetings with clergymen, human rights activists, and former guerrilleros. The word *seducción* is often used by Argentines to describe the political practice of charming, captivating, persuading, encouraging, misleading, and corrupting people.[2] The term arose frequently during my fieldwork in Argentina. It was couched in comments such as "The book by Giussani is seductive"; "Be careful in your interview with Admiral Rojas because he can be very seductive"; "President Menem is a seductive politician"; and "The documentary *La República Perdida* [The Lost Republic] was a seductive propaganda film for Alfonsín's political campaign." These remarks were intended to warn me about the dangers of being absorbed by the rhetoric argument, the distorted discourse, and the visual manipulation by interpreters and protagonists of political events. A seduced audience is, after all, a captive audience. As a captive audience, I might not realize that the accounts of my interlocutors were slanted, partisan, and politically motivated.

I have decided to retain the term *seduction* as descriptive of the interview dynamic discussed here, not only in continuation of its Argentine meaning, but also because *seduction* means literally "to be led astray from an intended course." Other terms, such as *deception* or *allurement*, carry negative overtones that suggest dishonesty and malintent, while the dramaturgical term impression management fails to take account of unconscious countertransferential reactions.

A disadvantage of the term is that it risks an association with Freud's seduction theory. My use of *seduction* stands clear from Freud's theory about hysteria and from any implied commonsense connotations with sexual desire, allurement, and entrapment. I am aware of the sexual overtones of the word *seduction* in English colloquial speech, but these overtones are far less prominent in Spanish. Furthermore, the term is comparable to other words that in certain contexts have sexual connotations, such as *satisfaction*, *excitation*, *penetration*, *stimulation*, and *scoring*. However, these partial meanings do not deter us from using them in their etymologically neutral sense.

Finally, I recognize the risks of using *seduction* in the context of violence. The association of the words *victim* and *seduction* makes me vulnerable to the unwanted charge that I somehow imply the victim brought upon himself or herself the pain that was inflicted, while the mere proposition that victims of violence might mold what they tell us could contribute to their victimization and, ultimately, might cast a shadow on my moral standards. How can I question the horror stories I have been told? We can easily imagine being given the unconscious. The analysis of these resistances may be a collaborative effort, but at times it may also require an adversarial stance by the analyst. Analysands may refuse to admit the existence of the transference, or they may wallow in it. They may resist acknowledging the relations between past experiences, present transferences, and recent stimuli that triggered these connections. Much of the analytic treatment exists in understanding these resistances and providing the analysand conscious insight into the repressed wishes and desires of the unconscious (see Freud 1905; Rawn 1987; Zetzel 1956).

Psychoanalysts are not immune to transference. They themselves may project feelings on their analysands. Some of these projections hark back to their own childhood. Others may be triggered by the patient's transferences. In a short but influential article, Heimann uses the term *countertransference* "to cover all the feelings which the analyst experiences towards his patient" (1950:81). Some of these feelings may be transferential, but others may be *counter*-transferential in that they are "the patient's *creation*" (Heimann 1950:83). Unrecognized countertransference reactions will draw analysts into a whirlpool of uncontrolled associations that make them lose their analytic stance. Heimann argues that analysts must therefore not deny or repress these feelings but must analyze them to the benefit of their analysands. Countertransferential phenomena do not necessarily interfere with a proper analysis but, instead, should be welcomed and utilized to gain a deeper insight in the patient's unconscious. Heimann does not recommend an empathetic discharge because "[i]n my view such honesty is more in the nature of a confession and a burden to the patient. In any case it leads away from the analysis" (1950:83).

Despite Heimann's heed to restrict the analysis to interpretational interventions and not to burden the analysand with an emotional involvement, soon psychoanalysts began to explore the possibilities of actively manipulating the relationship with their patients.[4] They realized that nontransferential interactions with their analysands could substantially facilitate but also frustrate the analytic treatment. Hence the transferential relationship was differentiated from both the analytic alliance and the "real" relationship between analyst and analysand. The term *analytic* or *working alliance* refers to "the patient's motivation to overcome his illness, his sense of helplessness, his conscious and rational willingness to cooperate, and his ability to follow the instructions and insights of the analyst" (Greenson 1967:192). In other words, there must be a basic trust between analyst and patient that both will and can help one another in a successful treatment.

Attention was also drawn to the so-called real interaction as a critique of the Kleinian position that all reactions in the analytic session are transferential. There was a growing awareness that analyst and analysand have opinions about one another that cannot be subsumed under either their transferential or their working relationship. Some even argued that patients must first be able to sustain "real" transference-free relationships before they can enter into an analytic working alliance (Greenson and Wexler 1969; Sechehaye 1956). Both relationships influence the interpretive efforts (Abend 1989; Greenson 1967:217; Greenson and Wexler 1909). Taken together, these reassessments of the transferential relationship transformed the psychoanalyst from the stereotypic stoic and faceless tabula rasa, which was to be the most receptive object of the analysand's inscriptions, to a person who demonstrated genuine care and, albeit cautiously, displayed empathy and emotion toward his or her patients.

## Empathy and Anxiety in Analyzing Victims of Violence

The therapeutic significance of empathy received important clinical support from the treatment of Jewish holocaust survivors. In a groundbreaking article Grubrich-Simitis argued that expressed empathy was crucial to psychoanalytic work with first- and second-generation survivors of Nazi concentration camps: "What he perceives of his analyst's anxiety, horror, shame, and pain . . . *proves* to the patient, psychically, the *reality* of these events" (1984:313). The survivor finds in the candor, but I also know that their discourse was repeatedly managed to enhance its effectiveness. Again, there is a similarity with contemporary accounts of the Jewish holocaust. Discussing eyewitness accounts of the mass killings in Auschwitz and the Warsaw and Belzec ghettos, Young concludes: "Unlike memoirs written after events, these first reports tend to be more self-reflexive and aware of their words' immediate impact on readers. In this way, they are not calls for reflection or contemplation of the events' meaning (even as they suggested meaning) so much as they are demands for immediate action and justice" (1988:29). Most of the public figures I interviewed in Argentina were well aware of the weight of their words in a time when opinions and interpretations about the "dirty war" were still being formed and reformulated. In the midst of this public debate I analyzed my own inhibitions, weaknesses, and biases, all to the benefit of a better understanding of both victim and victimizer. The ethnographic seduction by victims and perpetrators of violence became, therefore, a font of, rather than an obstruction to, insight.

Before exploring ethnographic seduction I shall enter into a brief but necessary discussion of transference in the analytic setting to be able to indicate where deduction fits in the multistranded relationship between anthropologist and informant. Then I shall draw on the work of analysts who have treated survivors of Nazi concentration camps. Their contributions are valuable for a better understanding of the unique epistemological and methodological problems faced when conducting research among victims and perpetrators of violence. In the main part of this article I shall analyze my resistance, encapsulation, and countertransferential reactions to my Argentine interlocutors and offer some reflections on how to cope with ethnographic seduction.

## Transference and Countertransference

The concept of transference is of paramount importance to psychoanalytic theory and practice. Controversy continues to reign among practitioners about the precise scope of transference, but many will agree with the following definition:

> *Transference* is the experiencing of impulses, feelings, fantasies, attitudes, and defenses with respect to a person in the present which do not appropriately fit that person but are a repetition of responses originating in regard to significant persons of early childhood, unconsciously displaced on to persons in the present. The two outstanding characteristics of transference phenomena are (1) it is an indiscriminate, non-selective repetition of the past, and (2) it ignores or distorts reality. It is inappropriate. (Greenson and Wexler 1969:28; see also Greenson 1967:151–2)[3]

An example of transference is an analysand's aggression toward the analyst that should, in fact, be directed at, and is intended for, the father. "What is fundamentally at issue in transference, is how a discourse that is masked, the discourse of the unconscious, takes a hold of a discourse that is apparent" (Lacan 1988:247). In other words, the analysand's hostility is the medium through which the unconscious desire – in this case the repressed wish to harm the father – is being transmitted. Many hours of contact between analyst and analysand may result in the development of a full-blown transference neurosis in which this all-absorbing relationship becomes the center of analytic attention. An analysis of the verbal and nonverbal manifestations of this neurosis allows the analyst to unravel the structure of the analysand's unconscious, and enables the analysand to correct his or her behavior. In addition to this transference on to the analyst, the latent discourse of the unconscious may tag on to the manifest discourse of dreams and free associations. The analyst will use dreams, slips of the tongue, and jokes as causeways into the unconscious. Truth is hidden in the error of manifest discourse, and accidentally revealed by inadequate ways of concealment. Lacan sums this up in the aphorism "Error taking flight in deception and recaptured by mistake" (1988:273).

Attention to the manifest discourse of transference is necessary to understand or overcome the analysand's resistance to an archaeology of been discredited publicly. Fourth, the interview situation has a social dynamic that exceeds the identifications, projections, transferences, and sympathies which occur during the meeting. The final implication is that our theories about face-to-face encounters influence our interpretation of the conversations. Concepts such as rapport, impression management, and presentation of self provide the interviewer with cues that affect the social dynamic and interpretation of the interview.

An interview situation does not provide enough time for the development of a transference neurosis, but it does offer a setting for transference, as has been shown systematically in Devereux's work (1967) on countertransference reactions among anthropologists (see also Hunt 1989). The interview between anthropologist and informant is in this respect similar to the pre-analytic interview. Except when severe neuroses or psychoses oblige the assignment of an analyst, an analysand will conduct a series of preliminary interviews with several potential analysts. Skin feeling and personal liking are far more important to the eventual choice than the display of theoretical knowledge or interpretive ability. Subtle transferential and countertransferential cues in this fleeting relationship are decisive.

Next to these transferential reactions, the social dynamics of the working alliance and the "real" relationship between analyst and analysand are relevant to ethnographic interview situations. The working alliance is the equivalent of rapport in anthropological fieldwork. Good rapport, or a productive dialogic alliance, makes the informant willing to respond to the anthropologist's questions and queries. Finally, the quality of the "real" relationship of nontransferential, everyday interaction between analyst and analysand is also important between ethnographer and informant. Ethnographers can only conduct proper research when they succeed in establishing and maintaining genuine social relationships with people.

These prerequisites of anthropological research harbor a potential danger. If trust and sincerity are so necessary for the successful outcome of a research project, then the ethnographer will go to lengths to achieve such favorable conditions and may unconsciously turn a blind eye to contrary signs. Room for circumvention has been created. The eager ethnographer might be kept away from culturally sensitive information under the false impression that the excellent rapport has opened all doors. The ethnographer may be seduced, may be directed away from his research objective under the impression of a good dialogic relationship.

Ethnographic seduction is the leading astray of an ethnographer from an intended path of knowledge and interpretation. This seduction occurs most effectively at the intersection of the transferential and the working relationship where the informant consciously directs the ethnographer away from a proper interpretation of the discourse that arises in the relationship.[7] Seduction cannot be confined to either one of these two relationships but straddles them. In more psychoanalytic terms, seduction is the conscious maneuvering of unconscious resistance.

## Ethnographic Seduction and the Dialogic Alliance

Just as an analyst may bow to the patient's effective resistance, so ethnographers may have inappropriate countertransferences that cause a loss of empathetic detachment and may even harm their informants (see Tobin 1986). Anthropologists may become involved in transferences with privileged informants but seldom have the professional training to turn the transferential relationships to their interpretational benefit. In their fleeting social encounters with many informants, they do not differ from analysts who conduct preliminary interviews with potential analysands. Seduction is most effective under these constraints.

Baudrillard (1990:8) defines seduction as the manipulation of appearances.[8] Seduction wins researchers over through a claim to truth, authenticity, and genuine insight. Seduction trades their critical stance as interpreters for an illusion of congeniality with the research subjects. This being so, seduction is more than just impression management. The manipulation succeeds best when it operates not only on the interactional but also on the psychological level of the two interlocutors.

emotions of the analyst a confirmation of the reality of a psychotic universe that systematically dismantled the ego boundaries between his or her inner and outer realities. "Examples of this are the deindividualization caused by replacing names with numbers, the demolition of shame barriers, the elimination of privacy and intimacy, and the removal of clocks and calendars" (Grubrich-Simitis 1984:307). Instead of interpreting the analysand's discourse as masking a latent discourse of the unconscious – the usual analytic procedure – the survivor's talk about concentration camps has to be accepted at face value. Treating the manifest discourse as a displaced unconscious fantasy would shatter the very reality that needs confirmation before the actual analysis can begin. Only the acknowledgment of the psychotic world of Auschwitz as real, and not as a nightmare, can lead to the restoration of a healthy ego that can distinguish reality from fantasy.[5] This "phase of joint acceptance of the Holocaust reality," as Grubrich-Simitis (1984:303) calls it, takes place only in part within the realm of transference and countertransference, and cannot be separated from the "real" relationship between analyst and analysand. As a matter of fact, the building of a "real" relationship through the demonstration of empathy and emotion is a prerequisite for a successful working alliance. The analysand has not only to be convinced that the analyst believes his or her reality, but also needs visible and audible signals that the analyst can bear the burden of transference. The survivor will only be able to use the analyst as a substitute – for the concentration camp guard or the mother who died in the gas chamber – when the emotional strength of the emerging relationship has been put to the test (Pines 1986). An analyst who either does not visibly empathize with the survivor or breaks down uncontrollably will not be able to establish an effective analytic alliance and therefore will fail to provide help and treatment. This empathy bears considerable emotional cost.

> The deep feelings of insecurity and destruction of basic trust engendered in these patients by their Holocaust experience contributed to the initial difficulty of maintaining a classical analytical technique for patient and analyst alike. Indeed, the painful countertransference feelings of shock and deep despair at man's inhumanity to man frequently evoked a strong desire to distance the patient emotionally, and to avoid the analytic empathy and understanding that are essential to the working through of the patient's problems. (Pines 1986:304)

The analyst tends to protect himself or herself against the onslaught of unwritten principles of social life. Fundamental social practices of coexistence – existential in origin yet cultural in practice – that constitute the rock bottom of humanness are disturbed. The analyst realizes that this foundation is a human construction that has to be reproduced continuously in a myriad of small and larger social acts. There is no guarantee that people will not suddenly rescind the social contract and create a world of utter debasement. The analyst realizes that this may also happen to her, and that she may never again regain trust and faith in her fellow human beings. She may even fear that she herself may be capable of extreme violence. The abhorrence is empathetic, the anxiety unconscious. These countertransference feelings are so troubling that they trigger defense reactions. It is this emotional as well as professional tendency to withhold empathy that need to be resisted by the analyst who wants to help concentration camp survivors and their children (Grubrich-Simitis 1981).[6]

These clinical experiences have several implications for interview situations between ethnographers and victims or perpetrators of violence. The first implication is that transferential and countertransferential feelings may arise in any extended face-to-face interview situation – not just analytic encounters – and especially when the conversation revolves around highly emotional issues that touch the basis of our common humanity. Second, personal defenses tend to be erected to protect oneself from these emotions. Such resistance is reinforced by social science research practices that favor objectivity, neutrality, and detachment. Third, visible empathy and countertransference facilitate the sharing of life stories when the interviewee's past experiences have caused a basic distrust of others, especially when his or her personal experiences have truth about what really happened in Argentina"; "We need foreign researchers like you who will be able to tell the truth which we cannot write"; "Abroad, they can write a truth which nobody wants to publish here." Some even tried to induce guilt: "I have told you my story so that you can write the truth"; "Do not use the things I have told you against us"; "Make sure that my story can never be used by those who killed my daughter."

But why did they resort to seduction? Those who dispute power, authority, and history realize that arguments alone fail to persuade, that charisma is the privilege of the gifted few, but that appearances are often taken at face value. This impression management is all the more effective when complemented by a subtle psychological dynamic that succeeds in making the researcher experience the discourse as genuine instead of constructed. Seduction is the strategy, while countertransference is the unconscious process through which this acceptance can be achieved most successfully.

## Seduction and Resistance in Argentina

Most generals, bishops, politicians, human rights leaders, guerrilla commanders, and former disappeared and political prisoners I interviewed in Argentina molded their discourse to enhance its effectiveness. I am not saying that accounts or events were made up, even though this may have occurred occasionally, but I argue that the repeated answering of the same questions and addressing the same issues have led to a selection, partly conscious and partly unconscious, of those replies and formulations that have proven to be most persuasive. They were keenly aware of the impact of certain words on their audience. For example, Argentine human rights leaders would consciously use words such as *concentration camp, Nazi, SS, Holocaust, Nacht und Nebel*, and *Auschwitz* to communicate most effectively the suffering, the state directed destructiveness, and the genocidal magnitude of the "dirty war."

These references to the Jewish holocaust were not unfounded. The anti-Semitism in the secret detention centers run by the Argentine armed forces became widely known after a national investigative commission on the fate of the disappeared published its findings. Prisoners of Jewish origin were treated with particular cruelty and obliged to shout "Heil Hitler!" and "I love Hitler!" Pictures of Adolf Hitler have been seen in torture chambers, and swastikas were commonly displayed (CONADEP 1986:67–72, 259; see also Díaz and Zucco 1987). The anti-Semitism of part of the military leadership was evident in delirious beliefs about Jewish conspiracies, such as the *Plan Andinia*. This plan was believed to be a conspiracy by Israel and the international Zionist movement to take over Patagonia and found an independent Jewish state to be called Andinia (Timerman 1981:73–4).

The anti-Semitism, the identification with Nazism and the frequent display of its symbols, and the extreme cruelty toward Jewish prisoners are undeniable, but these similarities do not necessarily make World War II and Argentina's "dirty war" comparable historical events. However, its proponents met any doubt on my side about this comparison with an accusation of Eurocentrism or, even worse from an emotional point of view, with the question of whether their dead and their suffering were somehow less important and painful than those of the Jewish holocaust. Take the following dialogue with the Jewish father of a disappeared son.

*Q:* . . . there are people who say, "This was a holocaust." What do you think of this?

*A:* This Tuesday we are meeting with a Holocaust survivor in Argentina to try to involve him, to see what room there is for our holocaust within the context of the holocaust in the world. Those who claim the Holocaust as their own do not like it that we have said that there was a holocaust in Argentina. Jews like it even less when we say that there were 1,500 disappeared Jews among the 10,000 mentioned by the CONADEP [Argentine National Commission on the Disappeared]. . . .

*Q:* But can one use, in your opinion, the term *holocaust* for Argentina as well?

*A:* I don't know about the meaning. I believe that it has the same value as *genocide*. When one part of a population kills another part of the population then it is a

Countertransference makes the ethnographer feel as if he or she has entered into the skin of the interviewee and understands the discourse from within its producer. In fact, what has happened is that the ethnographer has introjected the informant's created projection; a projection that has been disguised as transferential affect.

Ethnographic fieldwork operates and advances through a dialectic of involvement and detachment, of participation and observation, that is comparable to the psychoanalytic oscillation between introjection and projection. "As the patient speaks, the analyst will, as it were, become introjectively identified with him, and having understood him inside, will reproject him and interpret" (Money-Kyrle 1956:361). This dialectic becomes suspended by seduction. Seduction sidesteps empathy and insight. Seduction makes the ethnographer fail to maintain a degree of independence toward the informant. What is mistakenly regarded as empathy is in fact countertransferential identification. The distinction is important for an understanding of ethnographic seduction because undetected countertransference debilitates anthropological insight by drawing the ethnographer into an orbit of identification and a corresponding view of the world.

> In both empathy and countertransference an identification is effected with the patient. In empathy the identification is transient, a temporary sharing of derivative expressions of the patient's unconscious fantasies and wishes. ... In the case of countertransference, however, the analyst remains fixed at the point of identification with the patient. He is caught up in conflicts identical to those of the patient. Accordingly, the analyst becomes prone to the vicissitudes of these conflicts, and he may tend to act out or to respond defensively. (Arlow 1985:165–6; see also Abend 1986)

The difficulty with seduction is that the ethnographer is often not aware of its taking place and, moreover, may even seek it out unconsciously. Ethnographic seduction makes the interviewer feel that he or she has arrived at a profound understanding and has somehow entered a back region that hides a deeper truth. Like Freud's belated realization of his countertransference on Dora, it is only after reviewing the information gathered and examining the dialogic development of the interview that an ethnographer realizes that seduction had been mistaken for empathy and good rapport.

Ethnographers may become so wrapped in their countertransference that they are led astray from their research objective, namely, analyzing the interlocutor's discourse and piecing together an account of the issues under study. Seduction empowers the interviewee in relation to the ethnographer. Ethnographic seduction is the combination of a deliberate maneuvering of the dialogic alliance by the interviewee and the unconscious countertransferential reaction by the interviewer.[9] The adjective "ethnographic" refers to the intention of people to achieve the most favorable description of themselves and their social group within the expressed context of their culture and society. The ethnographer is not a passive subject in this manipulation. Crapanzano (1980, 1994:236) has convincingly shown that the depiction of culture is negotiated and constructed between anthropologists and informants. Ethnographic seduction adds one more dimension to this complex mutual assemblage.

This maneuvered construction becomes especially relevant in the case of victims and perpetrators of violence who have high personal and political stakes in legitimizing their interpretation of history. The retired generals, former guerrilla commanders, human rights leaders, bishops, and politicians I interviewed between 1989 and 1991 were conscious of their role in the national debate that waged during the fieldwork period about the historical reconstruction of the "dirty war." They did not simply want to tell their story to an interested outsider, clear their name, or give way to a catharsis; as important players in the public arena, they had a political stake in making me adopt their truths. They perceived foreign researchers as the harbingers of history who would retell their stories, and through their investiture as scientists provide these with the halo of objectivity and impartiality that their academic stature entailed.

Each party in Argentina tried to draw me into their camp and persuade me to analyze the decades of violence from their standpoint with phrases such as "You will be able to tell the

*A:* Certainly, but notice, take notice that this is a barbarity. [This abrupt switch from affirmation to outrage is crucial in this example of ethnographic seduction because it allows the general to dodge my question.] Do you realize that comparing the Holocaust in Europe – the Jews that were turned into soap in the gas camps and whatever more – with what has happened in Argentina. . . . [pause] But that is an enormous absurdity! [The general raises his voice in anger.]

*Q:* But there are people who say – [I am taken aback by his reaction and try to attenuate the situation. The general picks up the cue.]

*A:* Of course, because that is what the Marxist organizations, the left, the Mothers of the May Square, have achieved! [with a voice of indignation that turns to appeasement]. . . . There, millions of people, Jews and non-Jews died in the concentration camps of the Nazis. Here, what? No comparison. Here, there were no gas chambers, there was nothing that looked like it. So, you say that in Europe there was no written plan? [The general returns to my question.] Look, there were orders given and orders reissued. There was an entire organization, the SS, there were concentration camps, there were gas chambers and there were all –

*Q:* Of course.

*A:* Here, where?

The general guided the discussion away from the disappearances in Argentina by consciously turning my analytic analogy into an empirical comparison, and by making a rhetorical detour that deterred me from further pursuing the topic. After deliberately misinterpreting my question and placing me into the accuser's seat, he returns to the actual question and denies the existence of a criminal Argentine plan, simply because the two historical realities do not correspond. This and other instances of managed misinterpretation were not just displays of the rhetorical finesse of my interlocutors, of their adroitness to corner me with clever arguments; these were conscious attempts at ethnographic seduction because my interlocutors speculated on emotional effects – some of which have been identified here as countertransferential reactions – that did not require the perceptiveness of a psychoanalyst to anticipate. My status as a Dutchman, my country's obsession with World War II, and the great resonance in the Netherlands of the issue of the Argentine disappeared, constituted a fertile ground for seducción in its colloquial sense. If I was visibly moved, could no longer sustain the dialogue, did not pursue the topic raised, or failed to further explore their answers to my questions, then that was enough proof that the seduction had been successful.[10]

The verbal dimension of the attempts at seduction could be quickly discovered after analyzing the transcripts of the interviews.[11] The nonverbal dimension was harder to delineate because appearances and their manipulation are cultural practices. Nonverbal cues such as eye contact and tone of voice vary across cultures (Ewing 1987). The most important means of nonverbal seduction in Argentina was the show of emotion by my interlocutors. Let me emphasize that I am convinced that the tears I have seen people shed were real and sincere, but I also believe that their manifestation can be directed. These emotions were allowed to run free in my presence because they proved to be a powerful means of communicating grief. At other occasions, the harboring of feeling was more important. One member of the Mothers of the May Square – a group of women who drew worldwide attention to the disappearance of their children with weekly protest marches in down town Buenos Aires – told me that she repressed her emotions at conferences abroad in order not to pass off as a "hysterical woman." On the other hand, the open display of emotion in Argentina powerfully demonstrated the authenticity of her anguish but, at the same time, gave the military regime the opportunity to call the mothers derogatorily "the crazy women of the May Square."

Why is the show of emotion seductive? As Baudrillard has said, "To seduce is to appear weak" (1990:83). The interviewees did not try to impose an interpretation on me but coaxed me into a desired understanding by showing their own vulnerabilities. For example, I seldom asked directly about the abuse to which my interlocutors had been subjected, genocide or it is a holocaust. Whatever. If the problem is one of numbers, then let's return to Brecht.

*Q:* Indeed.

*A:* Well then, let's stop worrying about numbers, that is to say, as I believe a general or, no, a minister said: "When two children die of hepatitis it's a drama; when 200 die it's an epidemic." That's fine, it's a statistic.... I don't know if one can call 1,500 kids a holocaust. However, why I think, yes, it is important to refer to the Holocaust is that it was the same ideology that took the Nazis or the Germans to make concentration camps and to kill the Jews – and not only Jews – in the concentration camps; this is also what reigned here in the concentration camps. That's why there was a photograph of Hitler. That's why they sang "let's make soap" and that's why they killed people in the way they did, whether by throwing them from planes, executing them, killing them with injections, or whatever made them prepare clandestine cemeteries. All these things had to do with the same phenomenon. The same Holocaust policy was transferred to here.

Such dialogues overwhelmed me at first. Unlike the Argentine psychoanalyst Kusnetzoff, who asked the rhetorical question "What does the Holocaust have to do with me?" while treating a Jewish survivor from a Nazi concentration camp, I became absorbed in the analogies between Europe and Argentina. Kusnetzoff (1985) expressed the countertransferential denial of his Jewish identity during the analysis, while I had a countertransferential identification because of a personal fate of family and national history. It became virtually impossible to continue asking questions about the nature of the repression and the methods of disappearance when all further questions were already preempted by the reference to the Jewish holocaust.

Even though the analogies between Europe and Argentina were not just drawn in my presence but were part of the national discourse on the "dirty war," it became clear to me that my interviewees were conscious of both their emotional and rhetorical impact on me. Human rights leaders and former guerrilla leaders emphasized the similarities in ideology and repressive practice. Military officers vehemently rejected such comparisons. In the following exchange with a prominent general of the 1976 junta, I tried to inquire into the nature of the so-called criminal plan (*plan criminal*) designed to make political opponents disappear. This criminal plan was central to the prosecutor's case in the trial against the heads of the military juntas: "In sum, it can be affirmed that the commanders established secretly a criminal mode of combating terrorism" (Cámara Nacional 1987:266). The verdict stated, "It has been proven, through declarations from the highest military leaders who participated in the antisubversive operations, that the orders were oral, that the operations and detentions were covert" and that the plan consisted of an extensive network of special task forces, interrogation centers, secret detention centers, and procedures for the disappearance of people (Cámara Nacional 1987:857). In my effort to make myself understood, the dialogue takes an unexpected turn.

*Q:* The Armed Forces are accused of, of having had a criminal plan.

*A:* This, this, this, pardon me for interrupting you. About this criminal plan.... [pause] Let me remind you, if there is a criminal plan then someone must have written it somewhere.

*Q:* Not necessarily, because, for example, what we may call the criminal plan of the Holocaust was never written. It is not written. [I am referring to the absence of a written, worked-out plan that predates the 1942 Wannsee Conference.]

*A:* Which plan?

*Q:* Of, of, of, of the Holocaust in, in Germany. It was neither written.

*A:* No, no, no, but [in Germany] there is a something and that is –

*Q:* And I pose this question because the Europeans see it this way. No? They see a similarity between the two [criminal plans]. [I sensed that the general was groping for a reply and assumed that he might have felt an accusatory undertone in my question, so I interrupted him to draw the dialogue onto a less personal and more analytic level by referring to a common European perception of the disappearances.]

elastic blindfold; it was a black rag with an elastic. On the other hand, they used adhesive tape for me that they had stolen from my house. My husband had a pharmacy. Well then, this adhesive tape was poorly put, you see? They covered my eyes, both of them well-closed, poorly, OK, very ugly, ha, ha, ha.

*Q:* Terrible. And did they leave you later at home or . . .? [I quickly acknowledge her suffering, but then I move as fast as possible away from the scene of torture.]

This dynamic of flight and inveiglement demonstrates the complexity of ethnographic seduction. The tortured woman tries to convey her terrible experience in such a way that I will sense her physical pain and, by inference, will become convinced of her political view on the years of dictatorial repression. It is her awareness of a connection between palpable description and historical interpretation, together with her mission to publicize the political views of the Mothers of the May Square, that make this dialogue into an example of ethnographic seduction. In this example, the seduction succeeds in its unconscious effect, but fails in its conscious intent. It is successful in impairing my methodological separation of empathy and detachment, but it does not manage to redirect my attention toward herself and her standpoint. Furthermore, by disentangling myself from the woman's anguish I have neither accomplished the detached empathy essential to my research nor have I recorded the discourse I was after. It seems almost as if in his topsy-turvy situation the roles of researcher and research subject have become reversed, yet with both losing out. The interviewee tries to provide the most detailed account of her torture, while the interviewer shies away from the volunteered information he wants to collect.

Resistance among informants is a defense strategy with which anthropologists have great familiarity and which they try to overcome systematically, yet they pay generally little attention to their own resistances.[12] Unknowingly, anthropologists may erect their own obstructions. The ethnographer's obstacles may become the informant's defenses that, in turn, the ethnographer then tries to scale. Ethnographers try to gain access to their interlocutors' backstage, to the things that are most private to them. "The impressions that ethnographer and subjects seek to project to one another are . . . those felt to be favorable to the accomplishment of their respective goals: the ethnographer seeks access to back-region information; the subjects seek to protect their secrets since these represent a threat to the public image they wish to maintain" (Berreman 1972:xxxiv). However. which boundary should they protect, and which region do they wish to enter? The ethnographer's and the informant's understanding of what constitutes secret knowledge and which intrusions should be resisted do not necessarily coincide. This disagreement provides opportunities for ethnographic seduction. For example, many former political prisoners in Argentina regarded their torture as the most private and therefore most valuable back region. They sensed that this experience enhanced their credibility as a victim, voice, and witness of the years of violence and repression. Even though many had every intention of sharing their experiences with me, several still held back their accounts. The more persuasion was needed, the more persuasive the account would be. However, due to the unrecognized countertransference, I stopped at the threshold of this dark back region and did not insist on entering it. The consequence was that several informants failed to tell their story. The seduction had missed its goal. Nevertheless, the seduction succeeded unwantingly on another count. The voluntary offering of very personal experiences enhanced their credibility as informants. The stories which they did share with me became somehow more convincing, whether justified or not. The ethnographic seduction operated through a partial revelation of a hidden world that was not further explored but was taken at face value in the belief that more knowledge could always be uncovered.

Sometimes I would cross the threshold either because I wanted or because I was forced. This trespassing provoked a different countertransferential reaction. After listening to a hair-raising account of torture and abuse, I felt exhausted and my mind would go blank. This happened, for instance, after an interview with but I concentrated on their interpretation of the political violence of the 1970s. Some were surprised at my reluctance and volunteered to give me detailed accounts. They had become accustomed to journalists who asked them to provide graphic descriptions. I generally responded that such painful recollection was not necessary because I had already read their declarations to courts and human rights organizations. In retrospect I realized that my reluctance was countertransferential. I wanted to spare them, but I also wanted to spare myself. The stressful prospect of hearing these accounts provoked defenses against a verbal assault that triggered reactions of rejection and self-preservation (see also Bozzolo 1988). Listening to the helplessness of victims of torture arouses deep anxieties of abandonment that one wants to repress, even at the cost of sacrificing important research data. An excerpt from an interview with a very active member of the Mothers of the May Square, who had been tortured to reveal the whereabouts of her son, illustrates such reaction. In the following exchange I try repeatedly to draw the conversation away from the woman, who, however, continues to refocus my attention on herself.

*A:* Before my sons disappeared, they took my husband and me.
*Q:* Ah, before, before your sons?
*A:* Yes, because they were looking for them and not.... [pause] At that time they thought that it would be easier to force the mother to say something, no? Or to say where they were. But if I gave life to my son, would I then bring him death?
*Q:* Indeed [*claro*].
*A:* Eh? That they did not realize, those, those beings [*seres*], because they are not men. And, and I tell you that they take one's dignity away because they undressed me, they tied me to a table like Tupac Amaru. And this is to take one's dignity away because I would have preferred to have been shot instead of having them done this to me. Isn't it? And the prod? Not in my life did I know what an electric prod was and there I learned it. And you feel so impotent, so humiliated. Before . . . naked before . . . I don't know how many men there were, I was blindfolded. . . . [pause]
*Q:* Where did this all happen? At the police station or was it – [Here I begin to draw myself away from her story by switching from the event to its location.]
*A:* No, at the ESMA [Navy Mechanics School].
*Q:* Ah, at the ESMA.
*A:* Yes, at the ESMA.
*Q:* And for how long were you there?
*A:* 48 hours. So, I can imagine what it must be or what it must have been –
*Q:* One year or more. [This is an improper intervention that, again, tries to switch the attention from the woman to a more general discussion of how long disappeared persons were being held at the ESMA.]
*A:* – for those who were locked up. I was blindfolded for 48 hours. I thought I would become insane, I swear. I thought I would become insane because I even saw my dog. I saw the armchair from my house. And I, what am I going to see if I'm blindfolded? I did not take off the blindfold. My husband, yes. I was in a cell next to him but he did not know where I was. But I heard his voice and this, you see, gave me strength. Because when they applied the electric prod to me they told me that they were doing the same thing to my husband. And no, they did not touch him.
*Q:* Ah, they didn't touch him?
*A:* They didn't touch him.
*Q:* Why, why do you think? [Instead of allowing her to finish the account of her torture, I switch to the husband, who to my relief had not been harmed physically.]
*A:* Well, I think that they must have believed that the mother is weaker or that the mother is closer to the son –
*Q:* Yes.
*A:* – and knows more.
*Q:* Indeed.
*A:* It is possible, but they were mistaken, they were mistaken. [The woman tries to return to her account, but I continue asking about her husband.]
*Q:* And, and your husband took off the blindfold and, and, and, saw something?
*A:* He saw the place. Yes, he saw the place, saw inscriptions that were there. And every time they opened the door they made him cover his face again. He had an

tive. The woman's account made me ask questions that skirted her torture, while the man's story made me unable to penetrate his emotional shroud with any questions at all. The more emotional the reaction, the greater the personal inhibition to further discuss these themes. I could no longer see the discourse behind the conversation, and sometimes would end the interview right there.

This inhibition through silence can have at least four possible explanations.[14] First, it might be a resistance. I fell into silence because I did not want to listen anymore to so much human degradation. I erected a wall to protect myself. Second, silence might result from the tacit understanding that probing deeper would be harmful to the narrator. At these moments of intense communication, one becomes sensitive to the limits of people's ability to relive their traumatic experiences. Reaching beyond these limits would be unethical and irresponsible because as ethnographers we have not been trained to provide psychological care in crisis situations.[15] Third, there may be a common awareness that silence marks the boundary of a secret area of knowledge or emotion that cannot be trespassed upon. Finally, silence may be a consequence of overextended empathy. I became incorporated in the suffering through a common anguish fed by the countertransferential identification. I could only share the pain in silence during this joint acceptance of the reality of terror.

At other times I would try to relieve the tense silence by asking about some minute detail or by switching to highly abstract themes of war and peace, or justice and punishment. I could only regain my professional composure by making a radical break with my worked-up emotions, as can be seen in the following excerpt from an interview with a rhetorically gifted former guerrilla commander. We were discussing the debilitating effect of the disappearances on the guerrilla fighters and the organization's direct order either to commit suicide by swallowing a cyanide capsule or to resist torture for 48 hours if caught alive, all in the hope of protecting their comrades. The commander succeeded in drawing me into a world of army bases and secret camps by using the second person.

*A:* And you did not know for sure, you didn't have guarantees, that whether you contributed information or not, your destiny would always be the same. Or they used you because you did not confess and because you did not talk. They cut you up in pieces in front of the others to debilitate the weak or if you talked, then they would torture you afterwards anyway. And they kept you in a "refrigerator" [lingo for being in confinement] till they used you or they needed you. And if not, after you had the luck that they did not torture you, then they moved you [lingo for a liquidation] and killed you with whatever method they decided upon.

*Q:* Eh, at this sinister note let's move to –

*A:* It seems as if we were in the camp!

*Q:* So much the better! And, well, the first question I would like to ask is....

The commander could not hide his glee as he observed the contrast between the emotion on my face and my poorly veiled intention to move to another topic. He felt compelled to tell me so, while I pretended not to be shaken at all and even feigned a delight in his graphic description; only to quickly pose a new question.

At other occasions my countertransferential identification would also stir the emotions of my interlocutor. I was once told by an Argentine anthropologist that one of my interviewees, a former guerrillero, had told her that he had seen tears in my eyes during a stirring moment of our conversation. This show of emotion made him realize the tragedy of his own life and made him break down as well. A complete collapse – his as well as mine – of the guarded critical distance occurred that made me lose all dimensions of my research enterprise and made my interviewee forget his political reasons for consenting to the conversation. I had repressed this important moment in our dialogic relation until my Argentine colleague told me about it. The emotional breakdown marked a joint acceptance of "dirty war" reality that considerably strengthened my relation with the former guerrillero, and tore down our mutual resistance toward a more openhearted exchange.

Thus seduction can work through confession but also through secrecy and mystification.

the father of a 17-year-old member of an outlawed political organization who disappeared in April 1976. After his son failed to arrive at a birthday party where he was expected, the father began a desperate search. He contacted an acquaintance who was a police officer, and they started making inquiries at the precincts and hospitals of Buenos Aires, all to no avail. After several months, the father came into contact with a colonel in active service through the mediation of a befriended retired first lieutenant. The following dialogue took place:

> And he says, "Tell me what happened." So I told him what happened. And with all virulence, you looked at . . . [pause]. I looked at this man, but I tell you as I told you before, that I tried to see from all sides if I could find the point of the . . . of the thread of . . . to, to arrive at the thread or the needle in the haystack [*punta del ovillo*], trying to, to discover anything. After telling him everything, he says, "Good. Look, you have to do the following: you have to pretend as if your son has cancer." I was listening and saying to myself, "What is he saying?" [The colonel continues:] "Pretend that he has cancer and that they have . . . that he is in an operating room and that there is a butcher and a doctor; pray that it will be the doctor who will be operating on him." And then I looked at, at the one with whom I had made a certain friendship and he took hold of his head and covered his, his face. Because he must have said, he himself must have said, "What is this sonofabitch saying?" Because then he realized that all his venom, his virulence came out of him [the colonel]. This man had stuck a dagger in my wound and had twisted it inside me. I say to him, "Pardon me." I say, "Sir, but you know something?" I said this because of what he was telling me. "No, no, I am weighing the various possibilities [*hago una composición de lugar*] and I am making a supposition. I don't know anything of what might have ha –" And I say, "But how do you have the gall to . . .," and because of my nerves the words couldn't come out but I had wanted to say, "You are a son of a thousand bitches." You see, tell him whichever barbarity. And then the other saw my condition because he thought that I was going to lose it. . . . [pause] I wanted to grab him by the throat and strangle him, but then anyone of those who were there would have taken their gun and killed me. There, for the first time in my life, the desire came over me to murder someone. I had been destroyed. . . . Something [my wife] didn't know. With the passage of time I have told her. These are unfortunate things that happen to you in life. And there, yes, it crossed my mind that yes, that day I could have ended up killing that man. I don't know what stopped me. Because I was desperate. But you cannot imagine how, with what satisfaction he said what he was telling me. And you should analyze that, that this man was in active service.

But I was unable to analyze. Exactly as he had tried to detect any sign in the face and words of the colonel that betrayed the tiniest bit of information about his son, but became paralyzed by the cruel supposition, so I was unable to stand aside and observe. He had incorporated me into his torment, sometimes discursively placing me in his shoes, and at other times highlighting the moments of his greatest anguish. I could have asked him about the place of the meeting, the spatial arrangement of the offices, which army regiment had been involved, whether he ever heard of the colonel again, how he knew that the man was a colonel and not an extortionist, and whether he ever saw the first lieutenant again. But my mind went blank, and I could only share this man's sorrow in silence, while he took me along on the incessant search for his son.

Unlike the woman in the previous example, this man succeeds in totally absorbing me into his account. I believe that both interviewees were aware of the impact of their stories but did not stage them. There was no need to do so. The repeated narration of the experiences in public has led to its crystallization into the most moving and persuasive version.[13] Nevertheless, narrators can never foresee the emotional and countertransferential reactions of their interlocutors. The man and woman told their harrowing experiences in and with visible pain, but their accounts led in one instance to withdrawal and in the other to absorption. Both exchanges were seductive because they disabled the empathy-detachment dialectic, affected my critical stance, and deviated the research from its intended course and objecaddition to those basic anxieties, my research also uncovered cultural anxieties through my confrontation with different practices of violence. Torture may, at one time or another, occur in any country, but "there seem to be culturally-favoured forms of torture in different societies. In Latin America, for example, there is little use of the *falanga*-related tortures [beating the soles of the feet], and a great deal of use of electrical forms of torture; in Greece, however, falanga greatly predominated" (Peters 1986:171). Hispanic torture practices differ from those of my own northwest European culture (see CONADEP 1986:20–51; Graziano 1992:158–65; Suárez-Orozco 1987). Torturers hit where it hurts most. They are conscious of their victim's cultural sensitivities. Listening to descriptions of certain forms of torture attacked the foundation of my cultural constitution. There was an immediate unconscious reaction to deny the ensuing ethnographic anxieties produced by the repression of cultural experiences in the field that correspond to unconscious wishes and desires (Devereux 1967:42–5). The confrontation with forms of violence that do not belong to my cultural repertoire provoked anxieties that were difficult to recognize and understand but that nonetheless were present. Ethnographic anxiety was yet another expression of the countertransferential relationship between my Argentine interlocutors and myself, as an ethnographer and as a European.

## Coping with Ethnographic Seduction

How can anthropologists become conscious of ethnographic seduction and use their knowledge of transference and resistance to improve their cultural insight? Elsewhere I have shown how an analysis of ethnographic seduction by Argentine military officers led to a better understanding of the "dirty war" practice of disappearance (Robben 1995). In this article I have concentrated principally, although not exclusively, on the transferential relation between ethnographer and informant. By way of ethnographic seduction the informant wards off attempts by the researcher to break through the manifest discourse, and maneuvers the ethnographer's understanding, position, emotions, and unconscious countertransference to achieve that goal. Psychoanalytic theory proves helpful in making anthropologists aware of this seductive process within the ethnographic context. The principal differences between the ethnographic and the psychoanalytic encounter are that the ethnographer is often unaware of unconscious processes, and that the discourse is disguised in a seductive cloak that hampers instead of enhances ethnographic insight. Although attentive to narrative inconsistencies, jokes, mental blocks, and slips of the tongue, the ethnographer may still be seduced into accepting the surface discourse because of his or her unrecognized immersion in a transferential relationship.

There are several common countertransferential reactions among analysts that apply equally well to ethnographers. One set of reactions occurs during the interview, such as difficulties to grasp the flow of associations, excessive emotion or boredom, irritability or sleepiness, and even feelings of love or hate. Other reactions take place afterward, such as frequent thoughts about the interviewee, possibly accompanied by depression, dreams and fantasies, as well as slips of the tongue and the compulsion to talk to others about the interviewee (see Arlow 1985:173; Hunt 1989:61–76; Winnicott 1949). These cues will make the ethnographer conscious of countertransferential reactions so that in due time the strategy of seduction can be outlined. This discovery process is distinct from the usual analytic situation because, unlike transference, ethnographic seduction is a consciously maneuvered projection. Additional cues to the ones mentioned above will therefore be delineated here to recognize seduction.

Skin feeling is an aspect of seduction about which neither the ethnographer nor the interlocutor has much control, but which is of the utmost importance. Liking someone does not depend on ideology or social status. Although my initial sympathy and tolerance of personal idiosyncrasies was greater with members of human rights organizations than with the armed forces, I soon met officers whose politics I detested but for whom I felt a personal liking, while I talked to human rights activists

Western scientists have for centuries internalized a Hermetic stance, desiring to penetrate discourse to reach a greater and deeper truth (Eco 1992:29–33). Psychoanalysis has even made it the essence of its clinical practice. It assumes a fundamental affinity between secrecy and resistance.

> What is peculiar to the field of psychoanalysis is indeed the presupposition that the subject's discourse normally unfolds... within the order of error, of misrecognition, even of negation – it is not quite a lie, it is somewhere between an error and a lie. These are the truths of crude common sense. But – this is the novelty – during analysis, within this discourse which unfolds in the register of error, something happens whereby the truth irrupts, and it is not contradiction. [Lacan 1988:265]

Ethnographic seduction, on the other hand, tries to dissuade the interviewer from finding a deeper truth behind the manifest discourse.

> Seduction takes from discourse its sense and turns it from its truth. It is, therefore, contrary to the psychoanalytic distinction between manifest and latent discourses. For the latent discourse turns the manifest discourse not *from* its truth, but *towards* its truth.... In seduction, by contrast, it is the manifest discourse – discourse at its most superficial – that turns back on the deeper order (whether conscious or unconscious) in order to invalidate it, substituting the charm and illusion of appearances. (Baudrillard 1990:53)

The assumption in both psychoanalysis and anthropology that there is a deeper truth behind manifest discourse, and that a transference of that deeper truth on the analyst or ethnographer may tease out this truth, prepare the ground for seduction because it invites the recipient to surrender. Transference is only possible if the ethnographer opens himself or herself to the informant and allows for an empathetic introjection. Only a surrender to the informant's conditions of truth will yield the desired information and transference. Admonitory remarks about the political sensitivity of the information serve as an additional strategy to overpower the interpretive stance of the observer. It is an invitation to complicity. This complicity becomes seductive if it leads to a countertransferential identification. In a certain way, anthropologists want to be seduced because it gives them the desired feeling of gaining access to a hidden world.

The difficulty of obtaining such secrets does not only raise the value of the information but also inflates the researcher's ego and prestige. "When the knowledge is hidden, and revelation demands hard, painful work but brings status in its wake, one treats these secrets with overvaluing awe" (Luhrmann 1989:138). Seduction directs this pseudosecret, yet manifest, discourse to such an extent that the anthropologist believes that he or she is discovering a deeper truth that is validated by the protagonist. However, when the informant's seduction has succeeded, then part of this truth lies only inside the anthropologist, and the privileged knowledge is his or her own reflection.

People can look for a long time at themselves, provided they continue to see the same image. Reflections that keep changing can have a paralyzing effect. There were days when I talked in the morning to a victim of political persecution and in the afternoon with a military officer who had been responsible for the repression. These days were stressful because they demanded radical swings in empathetic understanding. Defenses were erected to prevent the incorporation of a dual identification with victim and victimizer. A failure to keep both at bay could have resulted in severe ego conflicts. I had the double task of resisting the seductive maneuvers that tried to draw me emotionally into one camp, and I had to erect barriers within myself between the two sides. If both would succeed in their seduction, then I would have to deal with the consequences of internalizing their conflict. At times I tasted of this volatile mixture as I felt torn between two opposite accounts of the same event. I only regained my balance by analyzing in a very detached manner the nonverbal interaction and the conflicting discourse in terms of content and rhetoric.

Some of my psychological tensions in Argentina were comparable to those experienced by psychoanalysts who have worked with Jewish holocaust survivors, but, in

# *Part IV*
# The "Other" Talks Back

## *Jeffrey A. Sluka*

Access to schooling and the dissemination of anthropological writings have turned once illiterate "informants" into avid critics of their ethnographers. Such "talking back" has made anthropologists aware of their conduct as researchers, led to soul-searching about their writings, and enhanced the quality of ethnographic research. In Part IV we present critical views of anthropology expressed by "others," including a native academic (Vine Deloria, Jr.), an indigenous research subject (King), and two case studies of ethnographers who faced the wrath of research communities that responded powerfully and negatively to newspaper representations of their work (Greenberg and Scheper-Hughes).

Today, many cultural anthropologists have had this experience with the press as a mediator of their texts, and this is recognized in Caroline Brettell's edited volume *When They Read What We Write: The Politics of Ethnography* (1993). The volume's contributors present many and varied examples of "the other" talking back, challenging, and criticizing ethnographic works. In her excellent introduction, Brettell observes that ethnographers "cannot control how what they put into print is read, let alone how it is publicly presented" (1993:17). She describes how anthropologists have responded to criticisms of their writing, and notes that the "others" who "talk back" to anthropologists generally include three groups of people:

1 Research participants or those about whom an ethnography is written, including both those who have read it and those who have only heard about it secondhand or read representations of it in newspapers.
2 Indigenous or native scholars, particularly anthropologists, in the country where an ethnography is set.
3 Journalists or other members of the press and media who write reviews of ethnographic writing.

Brettell also highlights Renato Rosaldo's identification of three forms of reactions by anthropologists when challenged by "native" readers (1993:20–1): First, the "Chicken Little Reaction" – they either retreat into hopelessness for the future of ethnography, or defend their interpretations and reject the validity of the response. Second, the "Two Worlds Reaction" – they emphasize that anthropologists and research participants speak two languages, that of science on the one hand and of whom I admired and whose political views I wholeheartedly shared but who had a demeanor that made me feel uncomfortable or even irritated. Such skin feelings depend in part on a countertransferential relation between ethnographer and interlocutor, and in part on the dissonance of personalities. My awareness could neutralize these feelings and reduce their influence on interviews.

As an anthropologist without analytical training, I learned most about countertransference and ethnographic seduction from an analysis of recorded interviews. "This intermediary, technologically efficient as a tape recorder is, contributes to the containment of both the patient's and the therapist's feelings, permitting the development of a countertransference that can be evaluated" (Lira and Weinstein, quoted in Bustos 1990:155). Since most of my interlocutors were highly visible public figures, I gained additional insight from comparing the interview transcriptions with their pronouncements published by the press.

In research on violent political conflict, one anticipates that interlocutors have a stake in presenting themselves in the most favorable light, in vilifying their opponents, and in trying to persuade the ethnographer of their right. Just as the psychoanalyst tries to get at the truth behind the manifest discourse of the analysand, so it is the ethnographer's aim to understand the subjectivity of his or her interlocutors. The crux of ethnographic seduction is not just that it tries to make the ethnographer accept the discourse uncritically, or that it is an attempt to disable the penetration of manifest discourse, but that the ethnographer either gives in to the manifest discourse or runs away from it without inquiring further into its meaning. Rhetoric tries to persuade from the outside, seduction from the inside, from the transferential relationship that arises between the interlocutors. By maneuvering the ethnographic dialogue and countertransference, the informant tries to seduce the anthropologist into accepting his or her discourse as the only discourse and as the only correct discourse. Seduction aims at either preventing the anthropologist from searching for a latent discourse or from thinking that the discourse offered is not genuine.

I do not pass judgment on attempts at seduction because I understand that a deliberately constructed rendition of events and the benefit of partisan scholarship may be emotionally, politically, and morally important for victims and perpetrators of violence. Be this as it may, research is not a vehicle for the political agendas of our informants and should not be steered by the dynamic of our unconscious. Awareness of ethnographic seduction does not mean that we should simply mistrust, disbelieve, or dismiss the accounts of people who have suffered or caused great injustices. On the contrary, by not naively accepting people's words at face value we demonstrate our professional responsibility: first, by trying to separate discourse from appearance and, second, by using those appearances to contextualize the discourse under study. The awareness of ethnographic seduction aims at restoring our empathetic detachment as ethnographers, disclosing concealed areas of cultural knowledge, improving our ability to understand discourse on its own terms, read its many latent meanings, and represent people's subjective notions, experiences, and interpretations in all depth and complexity.

the volume, Biolsi and Zimmerman observe that long before most anthropologists had heard of Michel Foucault or Pierre Bourdieu,

> Deloria had put his finger directly on what would later be called discursive formations, symbolic capital, and the micropolitics of the academy. Deloria asked, regarding Indian peoples, "Why should we continue to be the private zoos for anthropologists? Why should tribes have to compete with scholars for funds when the scholarly productions are so useless and irrelevant to real life"? (1997:4)

While part of Deloria's agenda was an attempt to deter anthropologists from further "meddling" with Native American communities, he did not reject the value of anthropology – only how anthropologists had acted; he respected anthropological research, as long as it was put at the service of Indian communities, and he had a major impact on the discipline as a whole. In "Growing up on Deloria," Elizabeth Grobsmith (1997) discusses the impact his work had on a generation of anthropologists:

> He [imposed] a test on us – a new standard, which those of us who would persevere had to meet. *Custer Died for Your Sins* became our primer for how not to behave, conjuring up the ultimate image of the tiresome meddler we dreaded and desperately hoped to avoid. It made us defensive, in the true sense of the term: we continually had to defend and justify our existence and practice self-reflection and introspection – tasks of self-evaluation critical to good social science. We would not advocate outside control or be party to schemes of exploitation, top-down development, or paternalistic imposition; rather, we applauded ... self-determination policies and attitudes ... and saw our role as facilitating indigenously defined agendas. (1997:36–7)

She argues this led to "a different breed of researchers" and helped moved us toward research relationships based on mutual reward or reciprocity (1997:47).

Deloria's and King's articles represent typical examples of the growing wave of criticism from Third and Fourth World research participants that led to the "reinvention" and "decolonization" of anthropology in the postmodern era. The publication of Deloria's book in 1969 inaugurated a new period in relations between American Indian people and anthropologists, and Deloria and King are typical of many other indigenous scholars around the world who have also articulated grave concerns about how anthropologists have represented them.

Cecil King's short article is in the spirit of Deloria and other indigenous critics of anthropology, and is the only article in this reader which is not by an anthropologist. We have included it because in a section devoted to "the other talks back" we thought we should include at least one unmitigated example. King, a member of the Odawa people from Manitoulin Island, Ontario, and professor of Education and Director of the Ontario Aboriginal Teacher Education Program at Queens University, expresses the all too common refrain about anthropologists:

> We, as Indian people, have welcomed strangers into our midst. We have welcomed all who came with intellectual curiosity or in the guise of the informed student. We have honored those whom we have seen grow in their knowledge and understanding of our ways. But unfortunately, many times we have been betrayed. Our honored guests have shown themselves to be no more than peeping toms, rank opportunists, interested in furthering their own careers by trading in our sacred traditions. Many of our people

everyday life on the other, and that "never the twain shall meet." Third, the "One Conversation Reaction" – they emphasize the new insights that can result from listening to native responses, and argue that these often outweigh any misunderstandings. This reaction usually involves incorporating these responses into the original text, frequently as an appendix in a subsequent edition, or presenting them in other publications – as the selected readings by Greenberg and Scheper-Hughes represent. Brettell also presents Rosaldo's conclusion that

> We should take the criticism of our subjects in much the same way that we take those of our colleagues. Not unlike other ethnographers, so-called natives can be insightful, sociologically correct, axe-grinding, self-interested, or mistaken. They do know their own cultures, and rather than being ruled out of court, their criticism should be listened to and taken into account, to be accepted, rejected, or modified, as we reformulate our analysis. (cited in Brettell 1993:16)

In 1969, Vine Deloria, Jr., a Standing Rock Sioux law student at the University of Colorado, published his controversial book *Custer Died for Your Sins: An Indian Manifesto*, in which he severely criticized anthropologists for the way they conducted their research with Native Americans. Part of the book is chapter four, "Anthropologists and Other Friends," on which the selection for this reader was based. It is the best known and among the most vehement of criticisms of anthropologists yet written, and begins with the memorable but scathing line "Into each life, it is said, some rain must fall . . . But Indians have been cursed above all other people in history. Indians have anthropologists" (1973:131). While Deloria's caricature of anthropologists is unfair because he overlooks the positive relationships anthropologists have had with Native Americans, he nevertheless makes important points that have not only been supported by many other Native Americans and anthropologists as well, but are typical of those made by a growing number of Third and Fourth World critics on other continents, which also began to emerge about the same time.

Despite the humor of his caricature of anthropologists, the message is very serious: Anthropologists have been a curse on Native Americans; they treat people as objects for observation, experimentation, manipulation, and "eventual extinction" (1973:132); and they are a greater threat to their existence than the US Cavalry ever was. In the book-length version, he adds "Thus has it ever been with anthropologists. In believing they could find the key to man's behavior, they have, like the churches, become forerunners of destruction" (1988:100). He concludes by warning anthropologists that "a new day is coming," and he advises us "to get down off [our] thrones of authority and . . . begin helping Indian tribes instead of preying on them" (1973:137). In particular, Deloria insisted that the "compilation of useless knowledge for knowledge's sake should be utterly rejected by the Indian people. We should not be objects of observation for those who do nothing to help us" (1973:136). Instead, he called for a new relationship between anthropologists and Native Americans based on reciprocity and Indian needs, and a redistribution of power in research relationships.

How Deloria's message has been interpreted and misinterpreted is explored in *Indians and Anthropologists: Vine Deloria, Jr., and the Critique of Anthropology* (1997), edited by Thomas Biolsi and Larry Zimmerman. In their introduction to and positive, and wrote a letter to the newspaper countering the original article. The controversy was picked up by a national newspaper, and Greenberg had to defend herself in the press at that level as well.

Greenberg's main point is that the newspapers played the leading role in shaping the public response to the book – in "influencing what happens when the people we write about read what we write" (1993:117). While a few people did read the book in order to form their own opinion, "the great majority of the public relied solely on information gathered from newspapers and formed their opinions on that basis" (1993:114). This is of critical importance, because it suggests that even today, when research participants themselves are nearly always literate, they still relatively rarely actually read what anthropologists write about them, and they are far more likely to gain their knowledge of what has been written about them from media reports of it. It seems ironic that most of Kiryat Shmona's residents "would not have learned about the book's publication had it not been for the appearance of the [newspaper] article" (1993:116).

Like Greenberg's case, the furor over Nancy Scheper-Hughes' ethnography about mental health in an Irish village was sparked by an article published in a newspaper. Scheper-Hughes conducted fieldwork in "Ballybran" in 1974, and in 1979 published her award-winning ethnography *Saints, Scholars, and Schizophrenics: Mental Illness in Rural Ireland.* In 1980, Michael Viney, a columnist from the *Irish Times,* visited "Ballybran" and wrote an article about the local anger he experienced in reaction to the book: "Since the publication last year, two or three copies of the book have been passing from house to house, hurt and anger flaring up like a gunpowder trail. The preface begs pardon 'for exposing the darker and weaker side of their venerable culture.' It seems unlikely to be granted" (Viney 1980:12). Like Greenberg, Scheper-Hughes responded by writing a "Reply to 'Ballybran' " (1981) for the *Irish Times*, defending the book. While she accepted that it may have caused a "diffuse and collective hurt experienced by members of a community that may view itself as betrayed," she denied that it should have caused individual hurt because she had only betrayed commonly known "community secrets" and no personal or family ones. Nonetheless, Scheper-Hughes exposed what people of "Ballybran" preferred not to have been exposed, and, in their words, "created a public shame" (1981:9–10).

In 1999, 20 years after the book was published, Scheper-Hughes finally returned to "Ballybran" and attempted to reconcile herself with the community by apologizing for any unintentional hurt given. But in this she failed; the trip ended with her "expulsion" from the village, and she concludes that she had created a new category of "unwanted people" in the village – "that new species of traitor and friend, the anthropologist" (1995:136).

Brettell's conclusions concerning fieldwork, text, and audience – or "when they read what we write" – provide a succinct summary of the situation today: It is inevitable that anthropological texts will be read, in one way or another, by those in the community studied; these are the greatest critics, followed by those closely associated or identified with those people – native scholars and other elites (1993:22). Issues of identity and representation will always be at stake, and anthropologists today are aware that the texts they write will increasingly be read and contested. Drawing attention to the politics of ethnographic writing, particularly the impact of the audience (readers) on both the anthropologist and text, has raised

> have felt anger at the way our communities have been cheated, held up to ridicule, and our customs sensationalized. Singer Floyd Westerman (Dakota), for example, expressed his anger in his 1969 recording "Here Come the Anthros." (1997:115)

Like other indigenous voices "speaking back" to anthropologists, King combines his criticism with a demand for reciprocity – a call for anthropologists to "become instrumental to our ambitions, our categories of importance" (1997:117) – with a demand for a redistribution of power in the traditional (empiricist) research relationship:

> We want to escape from the zoo. We want to be consulted and respected as not only human beings, at the very least, but as independent nations with the right to determine what transpires within our boundaries. We want to say who comes to our world, what they should see, hear, and take away. *Most important, we want to appraise, critique, and censure what they feel they have a right to say about us.* (1997:118; emphasis added)

After Ofra Greenberg's ethnography of an Israeli border community, where she had lived and conducted research for five years, was published in 1989, a journalist wrote a review for a small local newspaper which Greenberg says "grossly distorted facts and displayed a complete misunderstanding of the book's contents. According to this journalist, the people of Kiryat Shmona were depicted by me in a venomous manner, as being cruel and generally obnoxious" (1993:108). The story took some quotations from the book out of context, distorted others and made some up, and portrayed the anthropologist

> as an arrogant, privileged individual who observes the "natives" from a superior viewpoint, "like the traditional European anthropologists who investigated the native Africans, and who enjoys describing their backward and cruel world." She emphasized my affiliation with the veteran sector of Israeli society, my European origins, my secularity, and my academic education, and underscored the extent to which these attributes make me quite different from the people I studied. It was thus easy to characterize me as a European colonialist, invading the lives of the natives out of sheer curiosity. (1993:109)

When the article came to the attention of the people of Kiryat Shmona, they reacted angrily, even though they had not actually seen or read the book. There was a public furor, and Greenberg was attacked in the local newspaper and received numerous complaints and other negative reactions. In this article, she describes and analyzes the community's reactions to the book, stressing the role of the press in mediating the reading of her book and defining the anthropologist/ethnographer as a foreigner and outsider.

Greenberg was accused of superiority and condescension. Community people who read the newspaper article but not the book felt compelled to respond, and politicians took advantage of the situation to advance their own agendas. In her article, Greenberg emphasizes the multiplicity of responses that emerge from a diverse group of readers, especially when they are responding not to the actual text but to its (mis)representation in the press. The reactions were both direct and indirect, and both critical and supportive. She received telephone calls and letters, both negative

# 11

# Custer Died for Your Sins

## *Vine Deloria, Jr.*

Into each life, it is said, some rain must fall. Some people have bad horoscopes; others take tips on the stock market. McNamara created the TFX and the Edsel. American politics has George Wallace. But Indians have been cursed above all other people in history. Indians have anthropologists.

Every summer when school is out, a stream of immigrants heads into Indian country. The Oregon Trail was never as heavily populated as Route 66 and Highway 18 in the summertime. From every rock and cranny in the East, *they* emerge, as if responding to some primeval migratory longing, and flock to the reservations. They are the anthropologists – the most prominent members of the scholarly community that infests the land of the free and the homes of the braves. Their origin is a mystery hidden in the historical mists. Indians are certain that all ancient societies of the Near East had anthropologists at one time, because all those societies are now defunct. They are equally certain that Columbus brought anthropologists on his ships when he came to the New World. How else could he have made so many wrong deductions about where he was? While their origins are uncertain, anthropologists can readily be identified on the reservations. Go into any crowd of people. Pick out a tall, gaunt white man wearing Bermuda shorts, a World War Two Army Air Corps flying jacket, an Australian bush hat and tennis shoes and packing a large knapsack incorrectly strapped on his back. He will invariably have a thin, sexy wife with stringy hair, an IQ of 191 and a vocabulary in which even the prepositions have 11 syllables. And he usually has a camera, tape recorder, telescope, and life jacket all hanging from his elongated frame.

This odd creature comes to Indian reservations to make *observations*. During the winter, these observations will become books by which future anthropologists will be trained, so that they can come out to reservations years from now and verify the observations in more books, summaries of which then appear in the scholarly journals and serve as a catalyst to inspire yet other anthropologists to make the great pilgrimage the following summer. And so on.

The summaries, meanwhile, are condensed. Some condensations are sent to Government agencies as reports justifying the previous summer's research. Others are sent to foundations, in an effort to finance the following summer's expedition West. The reports are spread through the Government agencies and foundations all winter. The only problem is that no one has time to read them. So $5,000-a-year secretaries are assigned to decode them. Since these secretaries cannot comprehend complex theories, they reduce the reports to

Vine Deloria, Jr., "Custer Died for Your Sins," pp. 130–7 from Thomas Weaver (ed.), *To See Ourselves: Anthropology and Modern Social Issues* (Glenview, IL: Scott, Foresman & Co., 1973). Copyright © Scott, Foresman and Company.

theoretical, ethical, and practical questions for anthropologists who will increasingly be confronted by research participants who read and react. Consideration of cases of when "the other talks back" have helped us to move toward increased sensitivity and "new ways to involve our ethnographic subjects in their self-representation" (1993:22).

cal in purpose and content and often in the student-body itself.

The anthropological message to young Indians has not varied a jot or a tittle in ten years. It is the same message these anthros learned as fuzzy-cheeked graduate students in the post-War years – Indians are a folk people, whites are an urban people, and never the twain shall meet. Derived from this basic premise are all the other sterling insights: Indians are between two cultures, Indians are bicultural, Indians have lost their identity, and Indians are warriors. These theories, propounded every year with deadening regularity and an overtone of Sinaitic authority, have become a major mental block in the development of young Indian people. For these slogans have come to be excuses for Indian failures. They are crutches by which young Indians have avoided the arduous task of thinking out the implications of the status of Indian people in the modern world.

If there is one single cause that has importance today for Indian people, it is tribalism. Against all odds, Indians have retained title to some 53,000,000 acres of land, worth about three and a half billion dollars. Approximately half of the country's 1,000,000 Indians relate meaningfully to this land, either by living and working on it or by frequently visiting it. If Indians fully recaptured the idea that they are tribes communally in possession of this land, they would realize that they are not truly impoverished. But the creation of modern tribalism has been stifled by a ready acceptance of the Indians-are-a-folk-people premise of the anthropologists. This premise implies a drastic split between folk and urban cultures, in which the folk peoples have two prime characteristics: They dance and they are desperately poor. Creative thought in Indian affairs has not, therefore, come from the younger Indians who have grown up reading and talking to anthropologists. Rather, it has come from the older generation that believes in tribalism – and that the youngsters mistakenly insist has been brainwashed by Government schools.

Because other groups have been spurred on by their younger generations, Indians have come to believe that, through education, a new generation of leaders will arise to solve the pressing contemporary problems. Tribal leaders have been taught to accept this thesis by the scholarly community in its annual invasion of the reservations. Bureau of Indian Affairs educators harp continuously on this theme. Wherever authority raises its head in Indian country, this thesis is its message. The facts prove the opposite, however. Relatively untouched by anthropologists, educators, and scholars are the Apache tribes of the Southwest. The Mescalero, San Carlos, White Mountain, and Jicarilla Apaches have very few young people in college, compared with other tribes. They have even fewer people in the annual workshop orgy during the summers. If ever there was a distinction between folk and urban, this group of Indians characterizes it.

The Apaches see themselves, however, as neither folk nor urban but *tribal*. There is little sense of a lost identity. Apaches could not care less about the anthropological dilemmas that worry other tribes. Instead, they continue to work on massive plans for development that they themselves have created. Tribal identity is assumed, not defined, by these reservation people. Freedom to choose from a wide variety of paths of progress is a characteristic of the Apaches; they don't worry about what type of Indianism is real. Above all, they cannot be ego-fed by abstract theories and, hence, unwittedly manipulated.

With many young people from other tribes, the situation is quite different. Some young Indians attend workshops over and over again. Folk theories pronounced by authoritative anthropologists become opportunities to escape responsibility. If, by definition, the Indian is hopelessly caught between two cultures, why struggle? Why not blame all one's lack of success on this tremendous gulf between two opposing cultures? Workshops have become, therefore, summer retreats for nonthought rather than strategy sessions for leadership. Therein lies the Indian's sin against the anthropologist. Only those anthropologists who appear to boost Indian ego and expound theories dear to the hearts of workshop Indians are invited to teach at workshops. They become human recordings of social confusion and are played and replayed each summer, to the delight of a people who refuse to move on into the real world.

the best slogans possible. The slogans become conference themes in the early spring, when the anthropological expeditions are being planned. They then turn into battle cries of opposing groups of anthropologists who chance to meet on the reservations the following summer.

Each summer there is a new battle cry, which inspires new insights into the nature of the "Indian problem." One summer Indians will be greeted with the joyful cry "Indians are bilingual!" The following summer this great truth will be expanded to "Indians are not only bilingual, they are *bicultural*!" Biculturality creates great problems for the opposing anthropological camp. For two summers, they have been bested in sloganeering and their funds are running low. So the opposing school of thought breaks into the clear faster than Gale Sayers. "Indians," the losing anthros cry, "are a *folk* people!" The tide of battle turns and a balance, so dearly sought by Mother Nature, is finally achieved. Thus go the anthropological wars, testing whether this school or that school can long endure. The battlefields, unfortunately, are the lives of Indian people.

The anthro is usually devoted to *pure research*. A 1969 thesis restating a proposition of 1773, complete with footnotes to all material published between 1773 and 1969, is pure research. There are, however, anthropologists who are not clever at collecting footnotes. They depend on their field observations and write long, adventurous narratives in which their personal observations are used to verify their suspicions. Their reports, books and articles are called *applied research*. The difference, then, between pure and applied research is primarily one of footnotes. Pure has many footnotes, applied has few footnotes. Relevancy to subject matter is not discussed in polite company.

Anthropologists came to Indian country only after the tribes had agreed to live on reservations and had given up their warlike ways. Had the tribes been given a choice of fighting the cavalry or the anthropologists, there is little doubt as to who they would have chosen. In a crisis situation, men always attack the biggest threat to their existence. A warrior killed in battle could always go to the happy hunting grounds. But where does an Indian laid low by an anthro go? To the library?

The fundamental thesis of the anthropologist is that people are objects for observation. It then follows that people are considered objects for experimentation, for manipulation, and for eventual extinction. The anthropologist thus furnishes the justification for treating Indian people like so many chessmen, available for anyone to play with. The mass production of useless knowledge by anthropologists attempting to capture real Indians in a network of theories has contributed substantially to the invisibility of Indian people today. After all, who can believe in the actual existence of a food-gathering, berrypicking, seminomadic, fire-worshiping, high-plains-and-mountain-dwelling, horse-riding, canoe-toting, bead-using, pottery-making, ribbon-coveting, wickiup-sheltered people who began flourishing when Alfred Frump mentioned them in 1803 in *Our Feathered Friends*?

Not even Indians can see themselves as this type of creature – who, to anthropologists, is the "real" Indian. Indian people begin to feel that they are merely shadows of a mythical super-Indian. Many Indians, in fact, have come to parrot the ideas of anthropologists, because it appears that they know everything about Indian communities. Thus, many ideas that pass for Indian thinking are in reality theories originally advanced by anthropologists and echoed by Indian people in an attempt to communicate the real situation. Many anthros reinforce this sense of inadequacy in order to further influence the Indian people.

Since 1955, there have been a number of workshops conducted in Indian country as a device for training "young Indian leaders." Churches, white Indian-interest groups, colleges, and, finally, poverty programs have each gone the workshop route as the most feasible means for introducing new ideas to younger Indians, so as to create leaders. The tragic nature of the workshops is apparent when one examines their history. One core group of anthropologists has institutionalized the workshop and the courses taught in it. Trudging valiantly from workshop to workshop, from state to state, college to college, tribe to tribe, these noble spirits have served as the catalyst for the creation of workshops that are identi-

Sunday dinners. In the winter, the situation becomes critical for families who spent the summer dancing. While the poverty programs have done much to counteract the situation, few Indians recognize that the condition was artificial from start to finish. The people were innocently led astray, and even the anthropologists did not realize what had happened.

One example: The Oglala Sioux are perhaps the most well known of the Sioux bands. Among their past leaders were Red Cloud, the only Indian who ever defeated the United States in a war, and Crazy Horse, most revered of the Sioux war chiefs. The Oglala were, and perhaps still are, the meanest group of Indians ever assembled. They would take after a cavalry troop just to see if their bowstrings were taut enough. When they had settled on the reservation, the Oglala made a fairly smooth transition to the new life. They had good herds of cattle, they settled along the numerous creeks that cross the reservation, and they created a very strong community spirit. The Episcopalians and the Roman Catholics had the missionary franchise on the reservation and the tribe was pretty evenly split between the two. In the Episcopal Church, at least, the congregations were fairly self-governing and stable.

But over the years, the Oglala Sioux have had a number of problems. Their population has grown faster than their means of support. The Government allowed white farmers to come into the eastern part of the reservation and create a county, with the best farmlands owned or operated by whites. The reservation was allotted – taken out of the collective hands of the tribe and parceled out to individuals – and when ownership became too complicated, control of the land passed out of Indian hands. The Government displaced a number of families during World War Two by taking a part of the reservation for use as a bombing range to train crews for combat. Only last year was this land returned to tribal and individual use.

The tribe became a favorite subject for anthropological study quite early, because of its romantic past. Theories arose attempting to explain the apparent lack of progress of the Oglala Sioux. The true issue – white control of the reservation – was overlooked completely. Instead, every conceivable intangible cultural distinction was used to explain the lack of economic, social, and educational progress of a people who were, to all intents and purposes, absentee landlords because of the Government policy of leasing their lands to whites.

One study advanced the startling proposition that Indians with many cattle were, on the average, better off than Indians without cattle. Cattle Indians, it seems, had more capital and income than did noncattle Indians. Surprise! The study had innumerable charts and graphs that demonstrated this great truth beyond the doubt of a reasonably prudent man. Studies of this type were common but unexciting. They lacked that certain flair of insight so beloved by anthropologists. Then one day a famous anthropologist advanced the theory, probably valid at the time and in the manner in which he advanced it, that the Oglala were "warriors without weapons."

The chase was on. Before the ink had dried on the scholarly journals, anthropologists from every library stack in the nation converged on the Oglala Sioux to test this new theory. Outfitting anthropological expeditions became the number-one industry of the small off-reservation Nebraska towns south of Pine Ridge. Surely, supplying the Third Crusade to the Holy Land was a minor feat compared with the task of keeping the anthropologists at Pine Ridge.

Every conceivable difference between the Oglala Sioux and the folks at Bar Harbor was attributed to the quaint warrior tradition of the Oglala Sioux. From lack of roads to unshined shoes, Sioux problems were generated, so the anthros discovered, by the refusal of the white man to recognize the great desire of the Oglala to go to war. Why expect an Oglala to become a small businessman, when he was only waiting for that wagon train to come around the bend? The very real and human problems of the reservation were considered to be merely by-products of the failure of a warrior people to become domesticated. The fairly respectable thesis of past exploits in war, perhaps romanticized for morale purposes, became a spiritual force all its own. Some Indians, in a tongue-in-cheek manner for

The workshop anthro is thus a unique creature, partially self-created and partially supported by the refusal of Indian young people to consider their problems in their own context. The normal process of maturing has been confused with cultural difference. So maturation is cast aside in favor of cult recitation of great truths that appear to explain the immaturity of young people.

While the anthro is thus, in a sense, the victim of the Indians, he should, nevertheless, recognize the role he has been asked to play and refuse to play it. Instead, the temptation to appear relevant to a generation of young Indians has clouded his sense of proportion. Workshop anthros often ask Indians of tender age to give their authoritative answers to problems that an entire generation of Indians is just now beginning to solve. Where the answer to reservation health problems may be adequate housing in areas where there has never been adequate housing, young Indians are shaped in their thinking processes to consider vague doctrines on the nature of man and his society.

It is preposterous that a teen-aged Indian should become an instant authority, equal in status to the PhD interrogating him. Yet the very human desire is to play that game every summer, for the status acquired in the game is heady. And since answers can be given only in the vocabulary created by the PhD, the entire leadership-training process internalizes itself and has no outlet beyond the immediate group. Real problems, superimposed on the ordinary problems of maturing, thus become insoluble burdens that crush people of great leadership potential.

Let us take some specific examples. One workshop discussed the thesis that Indians were in a terrible crisis. They were, in the words of friendly anthro guides, "between two worlds." People between two worlds, the students were told, "drank." For the anthropologist, it was a valid explanation of drinking on the reservation. For the young Indians, it was an authoritative definition of their role as Indians. Real Indians, they began to think, drank; and their task was to become real Indians, for only in that way could they recreate the glories of the past. So they *drank*. I've lost some good friends who drank too much.

Abstract theories create abstract action. Lumping together the variety of tribal problems and seeking the demonic principle at work that is destroying Indian people may be intellectually satisfying, but it does not change the situation. By concentrating on great abstractions, anthropologists have unintentionally removed many young Indians from the world of real problems to the lands of make-believe.

As an example of a real problem, the Pyramid Lake Paiutes and the Gila River Pima and Maricopa are poor because they have been systematically cheated out of their water rights, and on desert reservations, water is the single most important factor in life. No matter how many worlds Indians straddle, the Plains Indians have an inadequate land base that continues to shrink because of land sales. Straddling worlds is irrelevant to straddling small pieces of land and trying to earn a living.

Along the Missouri River, the Sioux used to live in comparative peace and harmony. Although land allotments were small, families were able to achieve a fair standard of living through a combination of gardening and livestock raising and supplemental work. Little cash income was required, because the basic necessities of food, shelter, and community life were provided. After World War Two, anthropologists came to call. They were horrified that the Indians didn't carry on their old customs, such as dancing, feasts, and giveaways. In fact, the people did keep up a substantial number of customs, but they had been transposed into church gatherings, participation in the county fairs, and tribal celebrations, particularly fairs and rodeos. The people did Indian dances. But they didn't do them all the time.

Suddenly, the Sioux were presented with an authority figure who bemoaned the fact that whenever he visited the reservations, the Sioux were not out dancing in the manner of their ancestors. Today, the summers are taken up with one great orgy of dancing and celebrating, as each small community of Indians sponsors a weekend powwow for the people in the surrounding communities. Gone are the little gardens that used to provide fresh vegetables in the summer and canned goods in the winter. Gone are the chickens that provided eggs and small bureaus, projects, institutes, and programs that are designed to harvest grants for the university?

The effect of anthropologists on Indians should be clear. Compilation of useless knowledge for knowledge's sake should be utterly rejected by the Indian people. We should not be objects of observation for those who do nothing to help us. During the critical days of 1954, when the Senate was pushing for termination of all Indian rights, not one scholar, anthropologist, sociologist, historian, or economist came forward to support the tribes against the detrimental policy. Why didn't the academic community march to the side of the tribes? Certainly the past few years have shown how much influence academe can exert when it feels compelled to enlist in a cause. Is Vietnam any more crucial to the moral stance of America than the great debt owed to the Indian tribes?

Perhaps we should suspect the motives of members of the academic community. They have the Indian field well defined and under control. Their concern is not the ultimate policy that will affect the Indian people, but merely the creation of new slogans and doctrines by which they can climb the university totem pole. Reduction of people to statistics for purposes of observation appears to be inconsequential to the anthropologist when compared with the immediate benefits he can derive – the acquisition of further prestige and the chance to appear as the high priest of American society, orienting and manipulating to his heart's desire.

Roger Jourdain, chairman of the Red Lake Chippewa tribe of Minnesota, casually had the anthropologists escorted from his reservation a couple of years ago. This was the tip of the iceberg. If only more Indians had the insight of Jourdain. Why should we continue to provide private zoos for anthropologists? Why should tribes have to compete with scholars for funds, when their scholarly productions are so useless and irrelevant to life?

Several years ago, an anthropologist stated that over a period of some 20 years he had spent, from all sources, close to $10,000,000 studying a tribe of fewer than 1000 people. Imagine what that amount of money would have meant to that group of people had it been invested in buildings and businesses. There would have been no problems to study.

I sometimes think that Indian tribes could improve relations between themselves and the anthropologists by adopting the following policy: Each anthro desiring to study a tribe should be made to apply to the tribal council for permission to do his study. He would be given such permission only if he raised as a contribution to the tribal budget an amount of money equal to the amount he proposed to spend on his study. Anthropologists would thus become productive members of Indian society, instead of ideological vultures.

This proposal was discussed at one time in Indian circles. It blew no small number of anthro minds. Irrational shrieks of "academic freedom" rose like rockets from launching pads. The very idea of putting a tax on useless information was intolerable to the anthropologists we talked with. But the question is very simple. Are the anthros concerned about freedom – or license? Academic freedom certainly does not imply that one group of people has to become chessmen for another group of people. Why should Indian communities be subjected to prying non-Indians any more than other communities? Should any group have a franchise to stick its nose into someone else's business?

I don't think my proposal ever will be accepted. It contradicts the anthropologists' self-image much too strongly. What is more likely is that Indians will continue to allow their communities to be turned inside out until they come to realize the damage that is being done to them. Then they will seal up the reservations and no further knowledge – useless or otherwise – will be created. This may be the best course. Once, at a Congressional hearing, someone asked Alex Chasing Hawk, a council member of the Cheyenne Sioux for 30 years, "Just what do you Indians want?" Alex replied, "A leave-us-alone law."

The primary goal and need of Indians today is not for someone to study us, feel sorry for us, identify with us, or claim descent from Pocahontas to make us feel better. Nor do we need to be classified as semiwhite and have programs made to bleach us further. Nor do we need further studies to see if we are "feasible." We need, instead, a new policy which Indians are justly famous, suggested that a subsidized wagon train be run through the reservation each morning at nine o'clock and the reservation people paid a minimum wage for attacking it.

By outlining this problem, I am not deriding the Sioux. I lived on that reservation for 18 years and know many of the problems from which it suffers. How, I ask, can the Oglala Sioux make any headway in education when their lack of education is ascribed to a desire to go to war? Would not, perhaps, an incredibly low per-capita income, virtually nonexistent housing, extremely inadequate roads, and domination by white farmers and ranchers make some difference? If the little Sioux boy or girl had no breakfast, had to walk miles to a small school, and had no decent clothes nor place to study in a one-room log cabin, should the level of education be comparable with that of Scarsdale High?

What use would roads, houses, schools, businesses, and income be to a people who, everyone expected, would soon depart on the warpath? I would submit that a great deal of the lack of progress at Pine Ridge is occasioned by people who believe they are helping the Oglala when they insist on seeing, in the life of the people of that reservation, only those things they want to see. Real problems and real people become invisible before the great romantic and nonsensical notion that the Sioux yearn for the days of Crazy Horse and Red Cloud and will do nothing until those days return.

The question of the Oglala Sioux is one that plagues every Indian tribe in the nation, if it will closely examine itself. Tribes have been defined; the definition has been completely explored; test scores have been advanced promoting and deriding the thesis; and, finally, the conclusion has been reached: Indians must be redefined in terms that white men will accept, even if that means re-Indianizing them according to the white man's idea of what they were like in the past and should logically become in the future.

What, I ask, would a school board in Moline, Illinois – or Skokie, even – do if the scholarly community tried to reorient its educational system to conform with outmoded ideas of Sweden in the glory days of Gustavus Adolphus? Would they be expected to sing "*Ein' feste Burg*" and charge out of the mists at the Roman Catholics to save the Reformation every morning as school began? Or the Irish – would they submit to a group of Indians coming to Boston and telling them to dress in green and hunt leprechauns?

Consider the implications of theories put forward to solve the problem of poverty among the blacks. Several years ago, the word went forth that black poverty was due to the disintegration of the black family, that the black father no longer had a prominent place in the home. How incredibly shortsighted that thesis was. How typically Anglo-Saxon! How in the world could there have been a black family if people were sold like cattle for 200 years, if there were large plantations that served merely as farms to breed more slaves, if white owners systematically ravaged black women? When did the black family unit ever become integrated? Herein lies a trap into which many Americans have fallen: Once a problem is defined and understood by a significant number of people who have some relation to it, the fallacy goes, the problem ceases to exist. The rest of America had better beware of having quaint mores that attract anthropologists, or it will soon become a victim of the conceptual prison into which blacks and Indians, among others, have been thrown. One day you may find yourself cataloged – perhaps as a credit-card-carrying, turnpike-commuting, condominium-dwelling, fraternity-joining, church-going, sports-watching, time-purchase-buying, television-watching, magazine-subscribing, politically inert transmigrated urbanite who, through the phenomenon of the second car and the shopping center, has become a golf-playing, wife-swapping, etc., etc., etc., suburbanite. Or have you already been characterized – and caricatured – in ways that struck you as absurd? If so, you will understand what has been happening to Indians for a long, long time.

In defense of the anthropologists, it must be recognized that those who do not publish perish. Those who do not bring in a substantial sum of research money soon slide down the scale of university approval. What university is not equally balanced between the actual education of its students and a multitude of

# 12

# Here Come the Anthros

## *Cecil King*

*N'dahwemahdik giye n'weehkahnisidok g'dahnamikohnim meenwa dush g'meeget-chiwinim geeweekomiyek monpee nongo weenashamigabwitohnigok djigeegidotamah manda enjimowndjidihying. -N'geekahwe bigossehnimah dush Wo kinah gego netawtot, weebi-weedji-yawyung, weemeezhiyung nihb-wakahwin meenwa dash nah gihnihgehn-nah n'dahkidowinan djiminokahgohwing, mee gahzhi bigossehndahmah* ... My sisters and my brothers I greet you and also I thank you for inviting me to speak to the topic at hand. As I was preparing my thoughts, I begged the Maker of all things to be among us, to give us some wisdom and also maybe to make my works be as a medicine for us all – those were my thoughts.

We, as Indian people, have welcomed strangers into our midst. We have welcomed all who came with intellectual curiosity or in the guise of the informed student. We have honored those whom we have seen grow in their knowledge and understanding of our ways. But unfortunately, many times we have been betrayed. Our honored guests have shown themselves to be no more than peeping toms, rank opportunists, interested in furthering their own careers by trading in our sacred traditions. Many of our people have felt anger at the way our communities have been cheated, held up to ridicule, and our customs sensationalized. Singer Floyd Westerman (Dakota), for example, expressed this anger in his 1969 recording of "Here Come the Anthros."

We have been observed, noted, taped, and videoed. Our behaviors have been recorded in every possible way known to Western science, and I suppose we could learn to live with this if we had not become imprisoned in the anthropologists' words. The language that anthropologists use to explain us traps us in linguistic cages because we must explain our ways through alien hypothetical constructs and theoretical frameworks. Our *ezhibe-mahdizowin* must be described as material culture, economics, politics, or religion. We must segment, fragment, fracture, and pigeonhole that which we hold sacred. The pipe, *d'opwahganinan*, becomes a sacred artifact, a religious symbol, a political instrument, a mnemonic device, an icon.

We have to describe our essence, *d'ochichaugwunan*, to fit academic conceptual packages, and we have become prisoners of what academics have done to our words to verify their words. We want to be given the time, money, luxury, and security of academic credibility to define our own constructs from within our own languages and our own worlds and in our own time.

We struggle as contemporary Indian, Metis, and Inuit peoples to unlock the classificatory

Cecil King, "Here Come the Anthros," pp. 115–19 from Thomas Biolsi and Larry J. Zimmerman (eds.), *Indians and Anthropologists: Vine Deloria, Jr., and the Critique of Anthropology* (Tucson: University of Arizona Press, 1997). © 1997 The Arizona Board of Regents.

from Congress that acknowledges our intelligence, and our dignity.

In its simplest form, such a policy would give a tribe the amount of money now being spent in the area on Federal schools and other services. With this block grant, the tribe itself would communally establish and run its own schools and hospitals and police and fire departments – and, in time, its own income-producing endeavors, whether in industry or agriculture. The tribe would not be taxed until enough capital had accumulated so that individual Indians were getting fat dividends.

Many tribes are beginning to acquire the skills necessary for this sort of independence, but the odds are long: An Indian district at Pine Ridge was excited recently about the possibility of running its own schools, and a bond issue was put before them that would have made it possible for them to do so. In the meantime, however, anthropologists visiting the community convinced its people that they were culturally unprepared to assume this sort of responsibility; so the tribe voted down the bond issue. Three universities have sent teams to the area to discover why the issue was defeated. The teams are planning to spend more on their studies than the bond issue would have cost.

I would expect an instant rebuttal by the anthros. They will say that my sentiments do not represent the views of all Indians – and they are right, they have brainwashed many of my brothers. But a new day is coming. Until then, it would be wise for anthropologists to climb down from their thrones of authority and pure research and begin helping Indian tribes instead of preying on them. For the wheel of karma grinds slowly, but it does grind fine. And it makes a complete circle.

Humanities and the American Anthropological Association to regulate researchers by guidelines or codes of ethics. However, for most of us, these efforts are part of the problem. For we must ask: Whose ethics? In this era of aboriginal self-government, it is not for the outsider to set the rules of conduct on our lands and in our communities. It is our right and responsibility as aboriginal nations to do that. It is the right and responsibility of researchers to respect and comply with our standards. The dictates of Western science and the standards of behavior enshrined by associations of researchers dedicated to the advancement of social science may or may not be compatible with the code of ethics of our aboriginal communities.

Creative approaches must be discussed and debated by aboriginal communities, academic institutions, and individual researchers to reach a working relationship that neither constricts the advancement of knowledge nor denigrates the aboriginal communities' legitimate authority over the integrity of their own intellectual traditions.

Let me close with a story. I had a dream that all the peoples of the world were together in one place. The place was cold. Everyone was shivering. I looked for a fire to warm myself. None was to be found. Then someone said that in the middle of the gathering of Indians, what was left of the fire had been found. It was a very, very small flame. All the Indians were alerted that the slightest rush of air or the smallest movement could put the fire out, and the fire would be lost to humankind. All the Indians banded together to protect the flame. They were working to quicken the fragile, feeble flame. The Indians were adding minuscule shavings from small pieces of wood to feed it.

Suddenly, throughout the other peoples, the whisper was heard: The Indians have a fire. There was a crush of bodies stampeding to the place where the flame was held. I pushed to the edge of the Indian circle to stop those coming to the flame so that it would not be smothered. The other people became abusive, saying that they were cold too and it was our responsibility to share the flame with them. I replied, "It is our responsibility to preserve the flame for humanity, and at the moment it is too weak to be shared, but if we all are still and respect the flame it will grow and thrive in the caring hands of those who hold it. In time we can all warm ourselves at the fire. But now we have to nurture the flame or we will all lose the gift."

Those are my words. *Meegwetch.*

chains choking our dynamic languages and growing, changing lives. How can we learn how our language is structured, how our world of languages was created, if we still must parse, analyze, and chop them up to fit the grammar of other languages? How can we define who we are, what we see, and what we think when the public, politicians, and policy makers have accepted the prepackaged images of who we are, as created by anthropologists?

I am an Odawa. I speak Odawa, but anthropologists have preferred to say I speak Ojibwe. My language is an Algonquin language, I am told, and it is structured by describing things as animate or inanimate, so I am told. English definitions of the terms "animate" and "inanimate" lead people to think of things being alive or not alive. Is this how our language is structured? I think not. In Odawa all so-called inanimate things could not be said to be dead. Does animate then mean having or possessing a soul? Is this a sufficient explanation? I think not. Is the animate-inanimate dichotomy helpful in describing the structure of my language? I think that it is limiting, if not wrong outright. For in Odawa anything at some time can be animate. The state of inanimateness is not the denial or negation of animateness as death is the negation of the state of aliveness. Nor can something have a soul and then not have a soul and then acquire a soul again. In Odawa the concept of animateness is limitless. It can be altered by the mood of the moment, the mood of the speaker, the context, the use, the circumstances, the very cosmos of our totality. English terms imprison our understanding of our own linguistic concepts.

Having to define ourselves from the start with inappropriate English terms is not sufficient for our understanding. It is confining, and it is wrong. It seems that we must first defend ourselves against scholarly categories. We must find a way to break out of these cages. That takes a lot of unnecessary, unproductive time and energy and money.

In the last twenty years, Indian, Metis, and Inuit peoples have moved from reservations and isolated communities into places of greater visibility, but they are seen through the images built out of anthropological studies of them. We have been defined as "poor folks," members of a "minority" or "less sophisticated cultures"; we have been called "tribal," "underdeveloped," "nomadic," "less fully evolved." Therefore, real Indians are poor. You have provided us with the cop-outs: "Indian time" if we are late, "It's not the Indian way" if we don't want to do something. Employers have acquired cop-outs for not hiring Indians: Indians don't like competition, Indians don't like to work inside, Indians like seasonal employment. Teachers excuse the lack of Indian graduates. Indians themselves find excuses for their lack of employment, education, or dignity.

Now, we as Indian, Metis, and Inuit people want self-determination. We want self-government. When will anthropologists become instrumental to our ambitions, our categories of importance? How helpful is it to be called tribal or primitive when we are trying to negotiate with national and provincial governments as equal nations? Anthropological terms make us and our people invisible. The real people and the real problems disappear under the new rhetoric. Indian, Metis, and Inuit problems defined incorrectly lead to inappropriate solutions, irrelevant programs, and the reinforcement of the status quo. The real problems remain unresolved, and the Indian, Metis, and Inuit are again redefined.

The cumulative effects of all this are now evident. We have been redefined so many times we no longer quite know who we are. Our original words are obscured by the layers upon layers of others' definitions laid on top of them. We want to come back to our own words, our own meanings, our own definitions of ourselves, and our own world. Now scholars debate among themselves the ethics to be used in working in our communities and homes. It is as if they are organizing the feeding schedule at the zoo. We want to escape from the zoo. We want to be consulted and respected as not only human beings, at the very least, but as independent nations with the right to determine what transpires within our boundaries. We want to say who comes to our world, what they should see, hear, and take away. Most important, we want to appraise, critique, and censure what they feel they have a right to say about us.

We acknowledge, with gratitude, the attempts by the National Endowment for the figures such as teachers, nurses, and social workers, since patients were apprehensive of their hostile and dismissive attitude toward such "primitive methods."

In due course, I broadened the scope of my observation to include the operation of the other social services, in particular the welfare and educational agencies. The findings revealed a predominantly passive population supported by a variety of social services. Moreover, this population demanded greater practical and financial support from the services than that provided for by the formal regulations and criteria (e.g., a demand for a full subsidy for children attending a summer camp rather than the 75 percent subsidy that had been offered). By contrast, in the realm of folk medicine – a sphere of activity whose existence is not recognized by the authorities – people acted independently, with initiative and even mutual assistance.

After the publication of my book I was interviewed by a young journalist from a small left-wing newspaper. Her review grossly distorted facts and displayed a complete misunderstanding of the book's contents. According to this journalist, the people of Kiryat Shmona were depicted by me in a venomous manner, as being cruel and generally obnoxious. I quote from her article:

> Upon reading the book, only a masochist would want to visit the place. Kiryat Shmona, according to Greenberg's description, is a horrible town. The residents' outlook on life and their attitudes are fundamentally flawed, they are selfish and evil, public office holders are corrupt, the doctors and teachers are the worst imaginable.... During the five years she spent in Kiryat Shmona, Greenberg found innumerable negative traits among the town's residents, which she proceeds to enumerate one by one. First and foremost is the irritating tendency to change one's place of work. As if possessed, they leap this way and that, until at times it seems as though all of Kiryat Shmona is in a mad spin. (*Hotam*, November 10, 1989, p. 8)

She continues: "They don't know how to organize their meals or their lives so as to ensure that all the family is satisfied and healthy." In support of each such claim a quote is produced from the book, removed from its context and often distorted, as in the following phrase: "the greengrocer, a cunning and avaricious young man." This did not appear in the book, and neither did the following supposed quote: "The shoppers slice the cheese themselves with their filthy hands."

This journalist referred to a 1949 newspaper report about North African immigrants. (I have not checked its authenticity.) This report claimed that "we have here a people primitive in the extreme; their level of education is pitifully low, and worst of all, they are unable to absorb anything of intellectual value." She then added her own observation: "Forty years on, Greenberg does the same thing, the only difference being that she cloaks her argument in rational explanations." Purporting to quote from her interview with me, the journalist records a question she did not ask, and an answer I did not give: "One gathers from reading the book that the residents are worse than the establishment?" ... "That is what happens during the initial period there, they behave atrociously towards outsiders." ... "It seems that you have difficulty accepting the residents of Kiryat Shmona because you feel superior to them."

In her article, the journalist somehow connected the presentation of "the bad people of Kiryat Shmona" with the analysis of weakening family commitment. She is scornful of my analysis of the effect of social factors on the behavior of the members of the community, maintaining that I am suggesting that their inhuman behavior stems from their evil character. In fact, my analysis of the disappearance of family commitment does not imply any criticism of the local people. In the book I present in a neutral way the social conditions (in particular the proliferation of welfare services) that induce the general passivity of town residents toward their lives, expressed here in the evasion of family responsibilities and the expectation that the authorities will intervene. This pattern of behavior constitutes a surprising finding, considering that we are dealing with a traditional community characterized by binding family ties in its country of origin and during the initial period of settlement in Israel.

The journalist concluded her review by portraying me as an arrogant, privileged indivi-

# 13

# When They Read What the Papers Say We Wrote

*Ofra Greenberg*

In October 1989 my book *A Development Town Visited* was published in Israel (in Hebrew). The book contains an account of my experiences in Kiryat Shmona, a community where I lived and conducted research for five years. Kiryat Shmona was founded on Israel's northern border in 1950, largely as a result of a government policy promoting the geographical distribution of the population. It was settled by new immigrants from North Africa, Iraq, and Hungary, and was given the status of "development town" on the strength of its location and the composition of its population. This entitled both the municipality and its citizens to various government dispensations.

Within a few years, most of the inhabitants of European origin (Ashkenazis) had moved to the center of the country. By the 1980s, 90 percent of the population of 15,000 was of oriental origin, characterized by a low educational level (half the population had only an elementary education), negligible professional training, and low average income. During the period under study, families there were receiving welfare support at a rate 50 percent above the national average, partly a result of the high proportion of families with four or more children. Most of the inhabitants defined themselves as being religious, or at least as adhering to Jewish tradition.

At the outset of my research I was interested in the utilization of traditional medicine by immigrants from oriental countries. While attempting to investigate the extent and nature of this activity, I also looked into the characteristics and quality of the established medical services. Since Kiryat Shmona is situated far from the center of the country and has a predominantly low-income population, modern, private medicine was all but nonexistent. Virtually all medical services were provided by Kupat Holim, a medical insurance organization belonging to the labor federation.

I discovered early on that a large segment of the population was utilizing the services of practitioners of traditional medicine. Residing in the town were several folk healers who employed a variety of techniques which included charms and combinations of herbs. They treated a diversity of physical, emotional, and social complaints. A considerable proportion of their patients simultaneously sought help from conventional physicians for ailments beyond the scope of the folk healers, such as heart problems or cancer. Some were treated by both folk and conventional medicine for the same complaint (e.g., depression, infertility), with the conventional practitioners utterly unaware that their patients were receiving treatment from folk healers. Use of folk medicine was concealed from establishment

Ofra Greenberg, "When They Read What the Papers Say We Wrote," pp. 107–18 from Caroline B. Brettell (ed.), *When They Read What We Write: The Politics of Ethnography*. (Westport, CT: Bergin & Garvey, 1993).
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local journalist who had since moved to Jerusalem. The latter told me that after receiving a copy of *Maida Shmona* from his brother, he had immediately bought the book and read it in a day. He was calling to convey his support, as he could imagine how hurt I must feel. In his opinion, the book portrayed a true, if in part painful, picture of the place. He found in it considerable affection for the local people, and considered the "review" to be blatantly unfair both to the book and to me. He also mentioned that he had persuaded his brother to read the book, and his brother subsequently wrote a letter of support that was published in a different local paper.

In addition to telephone calls, I also received a number of letters, including one from the chief rabbi of Kiryat Shmona, with whom I was personally acquainted. He wrote with sympathy and support that "it is sufficient to photocopy the book's pages, conveying their love; there is no simpler or more relevant evidence. To my mind, the article in the newspaper ... was an expression of the feeling that 'an outsider cannot understand us,' and A.'s aggressive review was a faithful expression of Kiryat Shmona's complexity, which you have portrayed with a craftsman's hand."

I received another letter from a member of Parliament who had lived in Kiryat Shmona for a while and to whom I had sent a copy of my book. He wrote: "A wave of memories swamped me upon reading the book, and the characters you describe so well – with sympathy, understanding, compassion and warmth – appeared before me again.... I believe that this little book, which contains so much warmth and beauty, will find a path to the hearts of Kiryat Shmona's people." This member of Parliament, an active and well-known public figure in Israel, also wrote to the national newspaper.

My husband telephoned two or three friends who worked in the public service in Kiryat Shmona (the director of the urban renewal project, the coordinator of programs at the local community center) to enquire about their opinions. They reported that they had found it very difficult to accept the article in *Maida Shmona*, and that they wished to read the book. Once they had done so (I sent copies), they called back, expressing support and inviting us to visit them. They added that many people in Kiryat Shmona were now hostile toward me. The community center coordinator told us that there had been a number of lively discussions about the book among center employees. After people had read it a staff meeting was devoted to further debate about it.

## *Indirect reports*

News of reactions reached me indirectly as well as directly. From a neighbor whose brother lives in Kiryat Shmona I received a report of the uproar that broke out after the local publication. The brother added that he would advise me to keep well away from the town. A friend who works near Kiryat Shmona, and who comes into contact with town residents, told of their anger and of the reaction of one man to a suggestion that he read the book. "I've read the article, and that's good enough for me." Another friend who teaches in the Department of Social Work at Haifa University reported a furious outburst from a student from Kiryat Shmona when my book was mentioned during a lesson. She had not read the book.

There were also responses in the local press. The first letter came from a lawyer, a Kiryat Shmona resident of many years' standing who was also a personal acquaintance of mine. He asked me to send him the book and wrote to the paper after he had read it. I quote from his published letter at length.

> The reader will find a serious, balanced and fair work of research into the structure of Kiryat Shmona's community, and the institutions that are supposed to serve this community. The author does not refrain from criticism, but likewise does not fail to record the positive aspects of this community. If the research is not entirely objective, this is due to the sense of pain born of love, which is evident in the book.
>
> A copy of Ofra's book is to be found in the library of the college at which A. works, but he has not bothered to read it, preferring to criticize the book, and what is more, also the author (whom he knew personally during

dual who observes the "natives" from a superior viewpoint, "like the traditional European anthropologist who investigated the native Africans, and who enjoys describing their backward and cruel world." She emphasized my affiliation with the veteran sector of Israeli society, my European origins, my secularity, and my academic education, and underscored the extent to which these attributes make me quite different from the people I studied. It was thus easy to characterize me as a European colonialist, invading the lives of the natives out of sheer curiosity.

This newspaper article was soon brought to the attention of the people of Kiryat Shmona, and they reacted angrily. I was attacked in the local newspaper and received several telephone complaints. In fact, the furor in the town eventually caught the attention of the national radio. A publication that would otherwise have reached only a circumscribed and mainly professional readership became, with the help of media exposure, a news item broadcast throughout the country. The controversy died down only when the editor of the local newspaper curtailed the publication of further letters. However, it had gone on long enough in print to ensure that almost everyone in the town had heard about the issue.

In this chapter I describe and analyze the reactions of the investigated population to the book's contents, paying particular attention to the role of the press in mediating the reading of my ethnography as well as in defining me, a native anthropologist, as a foreigner and an outsider.[1]

## How the Community Responded

### *Direct responses*

The stream of reactions was not monolithic. People responded differently to the publication, in general according to their position in society. In addition to the townfolk, responses came from friends and acquaintances, from public figures and politicians, and from professionals. Some responses were direct and others were indirect. The first direct response came through a phone call from an indignant acquaintance from Kiryat Shmona. Born in the town, she worked for the welfare services with senior citizens. "Ofra, what have you done to us?" she exclaimed. "How could you sling mud at us like that, after we accepted you so warmly?" I was surprised and embarrassed. Although the journalist had promised to send me a copy of the article as soon as it appeared, she had failed to do so. I did not know how to reply to this caller. Once I had managed to obtain a copy of the newspaper from friends living on a nearby kibbutz, I was appalled by what I read.

Some days later, the local Kiryat Shmona paper, *Maida Shmona*, printed an article written by the leader of the municipal opposition. This man also had an administrative position in the academic institution where I had taught, and he knew me personally. In the article he wrote: "Ofra, or rather Dr. Ofra Greenberg, who honored us by her presence, resided in Kiryat Shmona . . . and collected incriminatory material about the locals . . . the measure of evil, contempt, hatred, and in particular racism exhibited by one person towards an entire population, is equivalent to a lethal dose of cyanide." Later in his article, he admitted not having read the book, claiming, "The [newspaper] article is good enough for me."

From this point onwards, the episode rapidly developed into a full-scale public furor. People used different mechanisms to direct their criticisms at me. Personal acquaintances used the telephone. A friend who lived in the vicinity of Kiryat Shmona and who had worked with me at the local college was among the first to call. I explained the distortion in the newspaper account and suggested that she read the book, which she promised to do. She advised me to reply to a letter in the local paper, as silence on my part would be regarded as an admission of guilt. I took her advice and my letter appeared. It elicited no direct response.

Several other people called to express their support. These included a secretary at my former college who had spoken to the friend mentioned above; a former student who asked me to send him the book (unavailable in Kiryat Shmona); a volunteer teacher; and a former

## Making Sense of the Responses

Perhaps the most interesting aspect of the reaction of members of the community where the research was conducted is that it was by no means monolithic. People both accepted and rejected the contents of the publication as well as the choice of action taken (or not taken) by government agencies as a result of the conclusions drawn. Their position was influenced by a number of factors including their public role, their level of education, issues of self-image, and their personal acquaintance with the researcher.

Certain local politicians used the event to further their own interests, that is, to achieve popularity by identifying with the "humiliated" public and by berating the "stranger." For example, the mayor of the town, who had his eye on a career in Parliament, added his comment to the original article when approached by the journalist. His reaction was given without reading the book and without checking her version with me. Quoting a Hebrew idiom, he spoke of the good people of the town being maligned by career-minded outsiders who "spat in the well from which they had drunk."[2]

The leader of the municipal opposition, an ambitious contender for the mayorship, also made no attempt to clarify the facts with me. His theme was ethnic prejudice and the lack of integrity on the part of educated Ashkenazis like myself. This theme, a recurring one in Israeli party politics, has been successfully invoked by the major right-wing party to win the votes of oriental Jews. Vidich and Bensman (1958b:4) observe that in Western society no reaction is to be expected when the ethnography deals with marginal or minority groups. In Israel, however, the poor and uneducated oriental population is somehow protected from public criticism, which can easily be construed by interested politicians as ethnic or even racial prejudice.

A few people read the book in order to form their own opinion, but the great majority of the public relied solely on information gathered from newspapers and formed their opinions on that basis. It appears that the higher a person's level of education, the greater was his ability and inclination to examine the facts independently (in this case by reading the book). The more educated individuals in the community were less likely to accept the newspaper account as the truth and more likely to take action beyond the confines of their immediate surroundings – by writing to the local or national press, for example.

There is a powerful relationship between self-esteem and a tendency to defend oneself and protest against criticism. In Israel, the correlation between oriental ethnic origin, low educational level, and low income has given rise to a paternalistic attitude on the part of the establishment toward the oriental communities. One of the by-products of this attitude has been low self-esteem among many of the uneducated oriental Jews who have no confidence in their ability to exert influence and to bring about change, and who therefore took no action with regard to my book even when they could have responded. This pattern of inactivity was evident in many aspects of life, some of which are discussed in *A Development Town Visited*. The general lack of active response upon reading the article in the local paper is symptomatic of this syndrome.[3]

On the other hand, when those suffering from low self-esteem imagine that their weakness is being pointed out, they are apt to react strongly. In the development town under discussion, many of the public officeholders had little formal education. The leader of the municipal opposition, who held a senior administrative position in the college although lacking a complete academic education, was quick to ridicule my doctor's title as well as my discipline as part of his defense of himself and his community.

While responses differed depending on factors such as public role and level of education, there were nevertheless a few issues around which objections were fairly uniform. One of these was my discussion of family ties and obligations. Not every issue will arouse the same intensity of objection, and they will vary from one culture to the next. In Israel, the family is clearly a sensitive topic.[4]

The role of the family in the national myth has, over the years, undergone several changes that have been bound up with the complex her five year stay in Kiryat Shmona), on the strength of a tendentious publication in the supplement "Hotam." This was A.'s first "foul." His second mistake was in failing to elicit the author's response. These two transgressions led A. to produce an amazing collection of unfounded and malicious slander, to which the term "character assassination" may be applied as an understatement.

No less serious than the damage caused by A.'s slanderous attack is the general outlook from which it derives. Unfortunately, this outlook is not peculiar to him . . . and is shared by most of those who represent and form public opinion in Kiryat Shmona. According to this outlook, mere residence in the town endows its inhabitants with rights devoid of obligations; the residents, and in particular their leaders, are above all criticism. Should the mirror reflect an ugly image, or the thermometer indicate the existence of a disease, they must be broken! (November 20, 1989, p. 20)

The local paper published three other responses, two of which expressed support from people who had read the book, while the other, from someone who read only the article, was angrily antagonistic. I received a long letter in favor of my book and castigating A.'s behavior, which the editor of *Maida Shmona* chose not to publish on the grounds of lack of space.

## How the National Newspaper Responded

The discussion in the national newspaper *Hotam* was of a more academic and professional nature than that in the local paper *Maida Shmona*. In one camp were the author, a member of Parliament, a university anthropology lecturer, and the publisher's editor, all of whom criticized the journalist's abuse of press freedom in the form of distortion of the facts. The respondents were the journalist and the editor of *Hotam*.

Whereas the correspondence in the local paper revealed emotions such as anger and hurt, and dwelt upon personal and emotive issues, such as the author's character and qualifications and the accuracy of descriptions of the town, the discussion in *Hotam* focused on general questions, such as the role of the press, or the anthropologist, as a detached and balanced observer. For example, the university anthropology lecturer wrote:

> The contents, style, and spirit [of the article] constitute, in my opinion, a crass and disgraceful exploitation of the sacrosanct ideal of press freedom, in the name of which such an article can be published. . . . This is not merely a matter of quoting out of context, or of sarcastic comments with no basis whatsoever in the book, but mainly of the selective and tendentious manner in which the journalist has chosen to convey her own messages. . . . Finally, as a social anthropologist, I protest at the irreversible damage caused by the journalist to the reputation of the discipline. . . . The residents of Kiryat Shmona and of other development towns may, with justification, regard your article as an incitement to a renewal of inter-ethnic conflict. Had Ms. X done justice to the book, she may have discerned its constructive aspects, and by so doing may have encouraged political and social action designed to further the welfare and interests of the residents, who are deprived by the establishment and paid venomous and hypocritical lip-service by the media, of the sort exemplified by the article. (*Hotam*, December 1, 1989)

The journalist replied by alleging that the chapter containing descriptions of interesting and sympathetic characters had been added under pressure from the publisher (a complete fabrication), and that the complaint about quoting out of context was a routine and devious defense.

Finally, the publisher's editor responded by claiming that what infuriated him about the article was "not only its distortion of the book's contents, but also the malice evident between the lines. . . . The journalist from *Hotam* has missed the point of the book. She makes only brief mention of the governing institutions' responsibility for Kiryat Shmona's condition. Ironically, the establishment and its failings are let off extremely lightly by the crusading journalist" (*Hotam*, December 1, 1989).

express his own subjective opinions. Many journalists thus find it difficult to understand the complex approach of the anthropologist, who can respect the people he or she researches while at the same time describing and analyzing their behavior from a neutral perspective. The anthropologist does not make judgments, whereas the journalist in many cases does. The work of the anthropologist is thus evaluated according to journalistic criteria. This results in misunderstanding.

The press is often perceived by the public as a representative of society and its product as a reliable reflection of reality. The question of whether a certain event really did take place (in our case, whether the book actually contains derogatory material) becomes largely irrelevant. Most people will not bother to read the original book or article, as they "know what is written there already." The information that sticks is that put out by the media.

The media's version of the "truth" becomes the issue under discussion as the publication galvanizes an aggregation of individuals into a community under attack, seeking to defend its honor. The debate takes on a life of its own beyond the control of the ethnographer, and around issues (in this case the role of the family in Israel and the status of various ethnic groups) that may not even have been central in the original text.

What happened after the publication of *A Development Town Visited* is yet another instance of how the press shapes reality. As early as the 1920s Lippmann (1922) was aware of the communication media's ability to create their own version of reality. Consequently, people do not react to objective reality, but to an environment perceived through the media. More recently, theoreticians (including neo-Marxists) have discussed the "constructing of reality." According to them, the mass communication media necessarily present a nonobjective picture of reality and thus affect the moral and ideological perception of what is "really going on." For example, if a certain unusual phenomenon is presented from the conservative viewpoint of social consensus, the coverage will tend, indirectly, to denigrate the deviant phenomenon, thereby affirming the existing consensus (Cohen 1972). This "spiral of silence" theory (Noelle-Neumann 1974) maintains that the communication media not only form the image of reality, but actually intervene to play a part in forming reality itself by urging people to action or to passivity on the strength of distorted information. They thus bring about a result that would otherwise not have occurred (for example, people who do not bother to vote because they have been led by the press to believe that theirs is a lost cause). The press, in short, is a powerful factor influencing what happens when the people we write about read what we write.

pattern of relationships between Ashkenazi and oriental Israelis. In the initial period of statehood, emphasis was placed by official ideology on the individual's commitment to society, whereas the importance of family attachment was considerably muted.

Later years brought with them a change in official attitudes toward the culture of oriental Jews in general, and toward the place of the family in that culture in particular. This change was due to complex sociopolitical processes. An accumulation of sociological knowledge stressed the importance of social continuity for the successful absorption and social adaptation of immigrants, and explained the central role played by the stability of the community and the family in the process of social integration. In a parallel development, political changes brought the "attitude toward oriental Jews" to the forefront of party political struggles, with the main right-wing party successfully turning it into a major election issue. The primary argument was that the time had come to respect once again the neglected honor of oriental Jews by, among other means, fostering their special culture, and by respecting their traditions.

As a result, there has been a revival in the value attached to the family. Whereas during the early years an effort was made to "modernize" the oriental Jews, recent years have seen a glorification of the "good qualities" of oriental ethnic groups (as a result of both social consciousness and political manipulation). Among the components that form this romantic-nostalgic idealization is the extended, warm, supportive family. This image plays its part in the newly acquired ethnic pride among some of the orientals. To question its validity in public discussion is unacceptable, although in private many orientals admit that reality falls far short of the ideal type. Nevertheless, a description perceived to be critical of an important component of the recently established community pride (in this case also ethnic pride) causes an emotion-laden response.

As Brettell points out in the introduction to this volume, sensitivity on the part of an investigated community to its public image has been noted by other anthropologists following reactions to the publication of their research. Scheper-Hughes, for example, reports that people do not object to the distortion of reality, but rather to seeing it in print. They are willing to accept her writing about them – even if it is critical – as long as it is not widely published, even though this is intended to benefit them.[5] Conversely, criticism of public institutions such as schools or a health clinic is readily accepted because responsibility is easily assigned to outsiders, in particular to government offices and officials. It is, in short, the violation of self-image, that brings about the stormiest reactions.

## Conclusion: The Press as Mediator

Newspapers played a leading role in shaping the response to the publication of *A Development Town Visited*. Most of Kiryat Shmona's residents would not have heard about the book's publication had it not been for the appearance of the article. Without the journalist's subjective and tendentious interpretation there would have been no public outcry.[6]

Anthropological literature provides us with several examples of the manner in which the press magnifies the resonance of a research report, selectively publishing certain sections in an endeavor to provoke the interest of readers. Renato Rosaldo (1989:63) describes the reactions to his research among the Ilongot in the Philippines, while Gmelch (1992) had a similar experience in Ireland. The account extensively reported in the introduction to the present volume of the Italian media representation of Schneider's work strikes a familiar note, as does Wrobel's (1979) account of his treatment by the Detroit press.[7]

The conflict between the anthropologist and the journalist arises out of a discrepancy of interests and a different professional ethic. First, the journalist is guided by a goal of drawing maximum attention to his product. In some cases this is achieved by distorting the facts in order to create a more sensational effect. Second, his professional ethic allows him to make value judgments on the material he presents. Some journalistic ideologies (e.g., "new journalism" [DeFleur and Dennis 1981] go so far as to encourage the journalist to rose early on winter mornings and went down to the sea to gather crannach, dilisk, carageen and other native edible seaweeds, half-freezing in his shirt-tails and warming himself by beating his sturdy arms across his chest. This, mind you, accomplished before the *real* work day of the farm had begun.

When Martin was still a very young man an older and more robust brother was sent off to America to make room for Martin, one of the younger and more vulnerable sons, to take up the family farm. Although primogeniture was then still customary, the father-patriarch had the freedom to choose his primary heir among his sons, according to his perceptions of his sons' skills, personalities, aptitudes and needs, as well as his and his wife's needs as they grew older. And the Da had settled upon Martin. But during the man's life-time, farming ceased being an enviable way of life and sibling jealousy had turned to sympathy toward those who were left behind to till the small 'rock farms' of An Clochan. And Martin's diasporic siblings had fared exceedingly well, numbering among them college teachers and clergy.[1]

Aine, the older sister, scowling while drying a plate and peering over Martin's shoulder, came out of the back of the house to give me a scolding: 'Who made you such an authority? You weren't such a grand person when you and your family came to live in our bungalow. You could hardly control your own children. Why don't you go home and write about your own troubles. God knows, you've got plenty of them, with school children shooting each other and US planes bombing hospitals in Kosovo. Why pick on us?'

Martin interjected: 'Admit it. You wrote a book to please yourself at our expense. *You ran us down, girl, you ran us down.* You call what you do a science?' And before I could deny that I did, he continued, 'A science, to be sure, the science of scandals. We warn our village children before they go off to the university in Cork or Dublin to beware books about Ireland written by strangers.' Seeing that his words had found their mark and tears were coursing freely down my cheeks, Martin softened his stance a bit, but not his sister who roundly rejected my apologies: 'You say you are sorry, but we don't believe it. Those are crocodile tears! You are just crying for yourself.'

Breaking the mood, Martin turned to my adult son, Nate, who was busy trying to hide himself in a thick hedge near the barn. Martin's words were soft and courtly: 'You are a fine looking lad to be sure, Nate, and I'm sorry to be talking to your mother like this in your presence.' Then, he returned his gaze to me: 'Sure, nobody's perfect, nobody's a saint. We all have our weaknesses. But you never wrote about our strengths. You never said what a beautiful and a safe place our village is. You never wrote about the vast sweep of the eye that the village offers over the sea and up to Conor Pass. You said nothing about our fine musicians and poets, and our step dancers who move through the air with the grace of a silk thread. And we are not such a backwater today. There are many educated people among us. You wrote about our troubles, all right, but not about our strengths. What about the friendliness of neighbors? What about our love for Mother Ireland and our proud work of defending it?' When I protested that I could not have written about those radical activities for fear of reprisals from outside against the village, Martin replied: 'Ah, you were only protecting yourself.' 'Is there *anything* I can do?' I asked. 'You should have thought about that before. Look, girl, the fact is that ya *just didn't give us credit.*'

## Homecoming

Twenty-five years had elapsed since a young and somewhat brash anthropologist and her off-beat, counter-cultural family – shaggy-haired, gentle 'hippie' husband and their three rambunctious babies and toddlers – stumbled somewhat dazed and almost by default into the relatively isolated, rugged mountain community of 'Ballybran' just over the spectacularly beautiful Conor Pass through the Slieve Mish mountains past the Maharees and nestled on the shores of Brandon Bay, a cul de sac on the eastern end of the Dingle Peninsula in West Kerry.

It was late spring 1974 and we had reached the end of the line, figuratively *and* literally. We had spent several weeks in a rented car canvassing villages in West Kerry and West Cork in search of an Irish-speaking (though

# 14

# Ire in Ireland

*Nancy Scheper-Hughes*

Fulingeann fuil fuil I ngorta
ach Ni fhuilingeann fuil fuil a dortadh
[A man can tolerate his own blood starving to death, but he won't tolerate his blood attacked by a stranger] (local proverb)
'A Hundred Thousand Welcomes' (Board Failte, *Irish Tourist Board*)

'Well, I am sorry to tell you, Nancy, but you are not welcome. No you are not. Have they let you a place to stay down in the village?' I was standing awkwardly in the once familiar doorway of 'Martin's' sturdy country house in a ruggedly beautiful mountain hamlet of An Clochan, a bachelor's outpost of some nine or ten vestigial farm households. Once, we had been good neighbors. During the summer of 1974, Martin had warded off the suspicions and dire warnings of his wary older sisters and had befriended us so far as to feel out my political sympathies toward various activities of the local IRA in which he and his extended family were involved. 'Ah, I should have listened to Aine', Martin said.

Over the past quarter of a century, some memories in An Clochan were engraved in stone like the family names of the Moriartys and the O'Neills carved over the smallest village shops in West Kerry signifying that *this* public house, *this* name, *this* family are forever. And what was remembered in this instance was a slight (in village terms, a slander) committed by me against the good name of the community. Ever the proud nationalist, Martin warned me to stay clear of village institutions: 'You'll not be expecting any mail while you are here', he said rather ominously.

Martin still cut a dashing, if compact, figure, now sporting a pair of gold wire-rimmed designer eye glasses and dressed on that afternoon in an impeccably starched white shirt. A shiny new sedan was parked outside his door. Martin's bachelor household, shared on the odd weekend with an older sister who works in the city, had clearly prospered over the past 2 decades. But all traces of active engagement with the land are gone. There was no sign of the haying that should have been going on during those precious few warm and sunny days in mid-June. No symmetrical mound of soft, boggy turf stood in front of the farm house. A quick side-long glance to the right showed the barn standing empty and swept clean. Above all, the neat row of newly laundered clothing strung across the outdoor line included no work-a-day overalls or denim shirts. What was once an active and viable farm had become a gentlemanly country home, a far cry from the days of Martin's youth when his beloved 'Da', the patriarch of large household,

Nancy Scheper-Hughes, "Ire in Ireland," pp. 117–40 from *Ethnography* 1:1 (2000). Copyright © 2000 SAGE Publications.

harm if he sits up all night in the barn singing to the cows?' Mihal would never see the walls of St Finian's madhouse. But there would be no excuses made for Seamus, the reluctant 44-year-old bachelor who expressed *his* frustration at a parish dance by leaping to the stage and drunkenly exposing himself to a crowd of village girls. He, of course, was quite mad.

Central to my thesis was the image of a dying and anomic rural Ireland resulting from the cumulative effects of British colonization, the Great Famine (1845–9), and various 20th-century development and 'modernization' schemes that tied the economy of rural western Ireland to Great Britain and then, with Ireland's belated entry into the European Community in 1973, to western Europe, as a whole. Throughout the process, the final vestiges of a subsistence-based peasant economy were destroyed to make way for capitalist modes and relations of production. The symptoms of malaise that I was observing in the mid-1970s were many: population decline in the coastal western villages resulting from out-migration and permanent celibacy; widespread welfare dependency of young, displaced farmers, shepherds and fishermen; depression, alcoholism and episodes of madness pushing up the Irish psychiatric hospitalization rates into first place worldwide.

Beneath the quaint thatched roofs and between the thick, clay walls of the rural farm households what was going on was an extraordinary emotional drama of labeling and denial that allowed some Irish county children (especially daughters and first-born sons) to achieve full adult status, education and eventual emancipation from the family, while consigning other children (especially latter-born sons) to the status of the 'leftover', worthless and pathetic '*aindeiseoir*' of the family. Every rural family seemed to have its high-achieving first-born pet sons and its under-achieving, last-born backward and painfully shy bachelors and its hopeless and stigmatized black sheep. Parental aspirations for achievement and status rested with the first-born, and everything was sacrificed to improve his life chances. In the 'old days' when farming was still a valued and productive way of life, he would have inherited the farm. But with Ireland's entry into the European Economic Community, the prized first-born was being reared for export, for emigration.

The rural Irish parents were faced with a new problem: how to keep back at least one son for the farm and to care for them in their old age. The task involves a certain amount of psychological violence: a cutting down to size and a crippling of the aspirations of the designated farm heir. In collaboration with village teachers, shopkeepers and the parish priest, farm parents tended to create a 'sacrificial child', oddly enough not in the form of the disinherited and dispossessed child, but in the more lethal and ambiguous form of the farm heir. From the time of his birth the heir is labeled 'the left over', 'the last of the litter', 'the scraping of the pot', 'the runt', 'the old cow's calf', a child who could never survive beyond the tolerant and familial confines of the village. 'Blessed are the meek', it is written, 'for they shall inherit the earth' ... and with it (I wanted to add) a life of involuntary celibacy, poverty, obedience and self-negating service to the old ones.

Through shaming and ridicule the farm heir eventually grows to fit his reduced role and life chances, and he comes to think of himself as only good enough for the farm and for the village, places generally thought of as not very good at all. Often enough the boy is able to make the necessary accommodations to his role. I have always been struck, even early in my anthropological career, by the enormous resilience and elasticity of the human spirit despite the violence that culture and society so often visit upon it. And there *were*, at least in the case of rural Ireland, some compensations and rewards: the boy who stays behind is praised as the dutiful, loyal, 'saint' of a son.

Some farm heirs never adjusted to the demands made of them and they aged poorly, becoming angry, isolated, bitter individuals, cut off from the flow of human life. Some became the depressed and alcoholic bachelor farmers who populated the several pubs that cater to a population of just 400 and some villagers. Others became eccentric hermits, and still others who deviated too far from the straight and narrow of village life became psychiatric patients at St Finian's Mental bilingual) community kind enough to accept our presence for a year of live-in fieldwork. We would begin our tentative inquiries about securing housing with the local post mistress or the resident curate or parish priest only to be told that people living in this or that community would not much fancy being observed by a live-in stranger. Ethnographic fieldwork was still a new and alien concept for a country people known both for their spectacular hospitality and for their fierce family loyalty and privacy. Tourists who came and went for the brief salmon-fishing season on the Dingle Peninsula were one thing, and bothersome enough in their own way, but a resident writer-anthropologist was something else again. In a country dedicated both to the banning of books *and* to revering the written word, any writer learns to tread lightly and to have a quick exit plan.

On arriving for the first time in 'Ballybran' I introduced myself and my family to the young curate of the spectacularly beautiful 'half-parish' with some trepidation. My official documents failed to dazzle this down-to-earth curate. What *did* make a difference were letters from our local university chaplain vouching that Michael and I were 'good enough' Catholics, if perhaps a bit wayward in our post-Vatican II enthusiasms for the transformation of Mother Church, and an almost illegible note from an older friend and informal mentor, the late Canon Law scholar, David Daube, stating that we were trustworthy and decent people. And so, ironically, with the sponsorship and blessing of the same Irish Catholic Church that I would take to task in the pages of my book, we settled into Ballybran a few weeks before the feast of Corpus Christi in June 1974 and we stayed until late spring the following year.

## A Fine Touch of Irish Madness

I arrived in Ballybran with a starting set of altogether alien and 'outsider' questions. Why did the Irish claim the highest rates of hospitalized mental illness in the world? Why was schizophrenia the primary diagnosis used in mental hospitals there? I believed that by studying 'madness' I could learn something about the nature of Irish society and culture as a whole. Deeply influenced by the early writings of Michel Foucault, I believed that a society revealed itself most in the phenomena it excludes, rejects and confines. Irish madness, I hypothesized, could be seen as a projection of specifically Irish conflicts and themes.

What was going on in remote, supposedly bucolic, western Ireland that was overproducing so many young psychiatric cases? Who were the likely candidates for mental hospital? What were the events that led up to a psychiatric crisis? Did the Irish actually have *more* mental illness than elsewhere, or were they simply more likely to label a village nonconformist as mad? Was the straight and narrow of Irish country life so rigid that it led to a straitjacket for some? What was going on in Irish farm families, and in the public spaces of village life, schools, pubs and church?

The book that emerged, *Saints, Scholars and Schizophrenics: Mental Illness in Rural Ireland* (1979), was a blend of old and new approaches: child rearing and adult personality, TAT tests, and reflexive/interpretive anthropology. Theoretically eclectic, it applied insights from Freud, Erikson, Durkheim, Gregory Bateson, R. D. Laing, and Michel Foucault to a tiny population of Irish-speaking farmers, shepherds and fishermen. Using the heterodox field methods of a qualitative and interpretive ethnographer, I amassed a great deal of circumstantial evidence supporting the pathogenicity of certain aspects of rural Irish social relations, especially those between the sexes and between parents and children. Rural Ireland, I concluded, was a place where it was difficult to be 'sane' and where 'normal' villagers could appear more 'deviant' than those institutionalized in the County Kerry mental hospital.

Madness was, I argued, a social script and there were appropriate and inappropriate ways of 'going' and 'being' mad in rural Ireland. Extreme eccentricity was allowable, even coddled, if it could pass as harmless 'foolery' or if it came wrapped in the mantle of Irish spirituality. 'Mihal, bless him, hasn't been quite right since the death of his mother, but what

The approach I was developing – a form of cultural critique – was seen as 'biased' and ethnocentric. Admittedly, my approach deviated from the usual anthropological manners which determined that we describe only what was 'good' and 'right' about a given society and culture. One was *not* to use anthropology in order to diagnose the ailing parts of the social body as a cultural pathologist of sorts. Why, I was asked, did my description of unhappy and conflict-ridden rural life depart so radically from Conrad Arensberg's (1937) classic and almost loving portrait of *The Irish Countryman*? In part, perhaps, because my ethnography was told, not from the perspective of the old men seated comfortably at the pub and at the hub of Irish country life, but from the perspective of their thwarted middle-aged sons. These were the 'young lads' and boy-os who would have to wait until their 50s, if lucky, to come of age and into their own, and even then they would have to wait, hand and foot, on the old ones who had retired to the 'west room' of the household and who, unlike their fathers before them, would most likely never marry, given the demographic imbalance [village girls had long since begun to desert the village lured by the promise of relative freedom that out-migration represented] or have a family and therefore a power base of their own.

*Saints, Scholars and Schizophrenics* offered a counter-hegemonic view of Irish country life, but one that struck some sensibilities as 'anti-Irish', 'anti-Catholic', or 'anti-clerical'.[2] In her incisive review of my book for the progressive Catholic journal, *Commonweal*, Sidney Callahan (1979: 311) charged me with religious bias suggesting that I was 'strangely insensitive to the religious idealism of the people' and that 'my hostility to the sexual repressiveness of Irish Jansenism, a hostility always to be encouraged [presumably by secular humanists such as myself], had made [me] tone deaf in [my] interpretation of religious phenomena.' Where I had seen needless self-sacrifice, Callahan questioned whether some 'repressions weren't worth the price' and she suggested that 'wit, learning, music, the work ethic, and altruistic sacrifice for family and high ideals' might also flourish in Ireland exactly because sex, aggression, and individualism were so severely curtailed. If the rural Irish values of self-discipline and mortification of the flesh contribute to the isolation, celibacy, depression, madness and alcoholism of bachelor farmers, they might also account for the extremely low incidence in the Republic of Ireland of physical assault, rape, adultery and divorce.

Another Irish-American critic, Eileen Kane (1982), described *Saints* as 'unethical' in its violation of the 'privacy' of the community and its right to maintain its 'community secrets'. These refer to the 'best-kept and worst kept secrets' (Bourdieu, 1977: 173), the ones that everyone in the community must keep in order to ensure the complicity of all in the collective forms of bad faith that make social life possible, such as, in this instance, the symbolic violence against the farm heir masquerading as concern and generosity toward the poor, inept last born sons of the village. In my various responses I denied that anthropologists had a responsibility to honor *community* secrets, especially those protecting what Sartre (1956) meant by 'bad faith' relations.

In *From Anxiety to Method*, George Devereux (1977) observed that in the field, as on the couch, the dynamics of transference and counter-transference can shape the ethnographer's perceptions and the resulting analysis. Indeed, the field can loom as a large Rorschach test for the naive anthropologist. Lacking sufficient critical distance and reflexive insight, the result can be distortion in the form of glaring omissions, editing, ambiguous descriptions and so forth. Ethnographers may use the field to work out their own neurotic conflicts and anxieties about attachment, power, authority, sanity, gender or sexuality. Here, confrontation and projection, rather than avoidance and denial, can lead to distortion in the form of a highly subjective interpretation that does violence to the natives' own understanding of the meaning of their culture and social relations.

From time to time, Devereux cautioned, the ethnographer should pause to analyse the nature of the object relations in the field and at home throughout the process of data analysis and writing. The goal of such ethnological self-analysis was to expose and to strip away the layers of subjectivity and bias that get

Hospital in Killarney. Many of these men were preoccupied with paranoid fears of bodily encroachment or obsessed with unfulfilled sexual and generative needs and fantasies.

Why didn't they escape? Some would have if they could, but too often they conceded to the prevailing view of themselves as incomplete men, lacking something, a bit too soft. To his face I've heard it said of a dedicated stay-at-home son: 'Sure our Paddy is a big old slob of a man, soft and sentimental, full of *dutcas*' (i.e. referring to warm, almost maternal, fellow feeling) while the man in question would nod his head in agreement. Hence, the rural Irish 'double bind' – two contradictory injunctions – on the one hand: 'You're worthless, you can't live beyond the farm; sure, if you had any guts you would have been out of here years ago', and, on the other: 'We need you – you're all we have; how could you even think of leaving your poor old Da? You're the last hope we have!' A third injunction prevented any escape from the horns of the dilemma: Stay, but you are forever a boy-o; leave, but you are guilty of filial disloyalty. A powerful ideology in the form of a puritanical and authoritarian version of Catholicism bolstered the symbolic violence contained in the exploitative social and family systems.

I had reinterpreted Gregory Bateson's (Bateson et al., 1963) double-bind hypothesis of schizophrenia within a larger social context to show that not only individual families, but entire communities can participate in patterns of distorted communication that can harm the individual while rescuing the social system. Scapegoating, collusions, family myths, and 'bad faith relations' are found not only in diseased or 'weak' families but in vulnerable communities as well. Social and economic situations can be double-binding, so that hard pressed farm families are forced to engage in unfair tactics for self-preservation at the expense of the designated child, and the whole community can come to accept and reinforce such distorted 'family myths'. It was not my intention to 'blame' village parents, but rather to shed light on an aspect of the rural Irish collective unconscious so that, once recognized, the emancipation and liberation of the generative scapegoat – the 'good, stay-at-home' son – might be possible.

## The 'Native' Reaction: Ethnography on the Couch

Ironically, no sooner was I notified in early 1980 that I was to receive the Margaret Mead Award from the Society for Applied Anthropology, honoring a book that 'communicated anthropological ideas and concepts to a broadly concerned public', than *Saints, Scholars and Schizophrenics* became embroiled in a large and lively trans-Atlantic controversy. The first critics of the book suggested that 'Ballybran' did not exist at all, and that it represents a 'composite', made up of bits and pieces of dozens of rural communities, both real and imagined. But in the fall of 1980 a columnist from the *Irish Times*, Michael Viney, headed out along the Dingle Peninsula, peddling his 10-speed mountain bicycle, buffeted by awesome gale winds and pelting sheets of rain in search of what he later described in one of his columns as the 'mythical valley of Ballybran'.

After a few false starts and cases of mistaken identity, Viney (1980) rejoiced on finally reaching his desired goal as he slipped inside the snug materiality of Peg's Pub. 'Yes', said the publican identifying herself, '*I* was one [in the book] who didn't believe in sociological statistics!' 'Mrs Scheper-Hughes had sat here often', Viney mused with a pint of Guinness in his hand, 'as I was doing now, with the rain hosing down from the mountains beyond the open door.' In a subsequent column (1983), Viney pictured himself as he thought the anthropologist might have seen him:

> Sometimes – cycling over the hill to the post office, past the rusty, crumpled bracken and the lichen-crusted walls – I look down at the little houses (which are for my writerly purposes crouched in Atlantic mist) and wonder what the anthropologist would make of our community (or indeed, of *me*, a squinting, unkempt figure in black oilskins and dripping cap, alienated, irretrievably from his own urban peer group, the epitome of *anomie* on wheels). Would she decide that our remote half-parish ... have a whole new perspective on [its] right or ability to exist?'

Both the scholarly and the popular Irish and Irish-American communities were up in arms.

poignant, more circumspect ethnographies, a high price for any writer to pay. But our version of the Hippocratic oath – to do no harm, in so far as possible, to our informants – would seem to demand this. Additionally, a hermeneutics of (self-) doubt could temper our brutally frank sketches of other people's lives as we see them, close-up but always from the outside looking in and 'through a glass darkly'.

As for the selectivity of my observations, what I had left out and what I might have said about An Clochan in the mid-1970s was that the village offered an extraordinary glimpse of a closed corporate rural community in which social hierarchy and social difference were successfully curtailed, where 'putting on airs' was spurned in the interests of *communitas* and where, despite the general rule of farm family patriarchy, girls were reared to be high achievers, women did *not* have to marry, and single women could raise sheep, drive cows, manage a village pub, run a primary or secondary school, scold the local gombeen man, or boss the local curate till he 'cried uncle' and gave in on a particular theological or political point. Rural women could choose to marry young or they could wait and marry late in life and then marry men much younger than themselves. Alternatively, especially in a family of daughters, they could refuse several marriage proposals in order to remain at home and inherit their father's fields and his favorite pipe or their father's pub and his celebrated goat-skin drum. Moreover, married women kept their maiden names *and* their pre-marital social and self identities.

Perhaps nowhere else in the world were women so free to walk country roads at night without fear of either physical assault or malicious gossip. Nowhere else have I seen women and men banter with each other in public without every source of humor reduced to a double-entendre. And nowhere else were bachelors and spinsters accepted as normal and unremarkable members of society, able to lead autonomous, if lonely, lives. No eyebrows were raised at the bachelor who not only planted and harvested but also cooked his own spuds, who not only raised his own sheep but was quite capable of knitting his own socks and sweater. How distant this was from Ivan Illich's (1982: 67) description of the woeful state of bachelors in those parts of traditional Europe more characterized by gender 'complementarity':

> You could recognize the bachelor from afar by his stench and gloomy looks. . . . Solitary men left no sheets or shirts when they died. . . . A man without a wife, sister, mother, or daughter had no way to make, wash, and mend his clothes; it was impossible for him to keep chickens or to milk a goat.

In An Clochan at the time of my study social life was not confined to couples. Dress for both sexes was casual and the sturdy figure ahead of you on the road wrapped in layers of trousers, woolen vests, long coat and shod in muddy green Wellington boots, and waving a stick, was just as likely to be that of a woman 'driving' her small herd of cows. I may have misread important aspects of social life in a community where *gender* and *sibling* bonding was as or more important than the sexual or the erotic bond. If marital relations were problematic it was in part because marriage interrupted and intruded upon other competing and equally valued affections and loyalties. Surely any anthropologist practicing today would not wish to suggest a hierarchy of appropriate affections such that life-long friendships, brotherly and sisterly in nature, would somehow count for less than conjugal relations.

If psychiatric hospitalization rates were high, rape and sexual assault were unknown at that same time. Theft was so rare that one definition of an eccentric was a person who was overly preoccupied about the safety of his property, while a case of 'paranoid schizophrenia' could be diagnosed on the grounds of having accused one's neighbors of wanting to steal one's sheep or cows or having shifted the stone boundaries that mark off one field from another. And 'Brendan the rapist' who I interviewed at the county mental hospital in Killarney had sinned only in his thoughts and was by his own account a virgin, unlucky in sex. So, as a young married woman in An Clochan, I could hail a ride on the back of Morris's motorbike without any hint of in the way and distort the perception of an objective ethnographic reality. To the end Devereux remained an empiricist dedicated to a belief in the perfectibility of objective anthropological facts, data and interpretation.

In the aftermath of the Irish controversy, I found Devereux's solution less than satisfying. For, as I saw it, the real dilemma and contradiction was this: How can we know what we know other than by filtering experience through the highly subjective categories of thinking and feeling that represent our own particular ways of being – such as the American Catholic-school-trained, rebellious though still ambivalently Catholic, post-Freudian, neo-Marxist, feminist woman I was in my initial encounter with the villagers of Ballybran.

Both the danger *and the value* of anthropology lie in the clash and collision of cultures and interpretations as the anthropologist meets her subjects in a spirit of open engagement, frankness and receptivity. There was, I concluded, no 'politically correct' way of doing anthropology. Anthropology is by nature intrusive and it entails a certain amount of symbolic and interpretive violence to the 'native' peoples' own intuitive, though still partial, understanding of their part of the world. The question then becomes an ethical one: What are the proper relations between the anthropologist and her subjects? To whom does she owe her loyalties, and how can these be met in the course of ethnographic fieldwork and writing, especially within the problematic domain of psychological and psychiatric anthropology where the focus on disease and distress, difference and marginality, overdetermine a critical view.

## Getting Over: Crediting An Clochan

Over the past two decades, 'Ballybran' has been host to a small but steady stream of anthropologists and sociologists from Europe and North America – little red paperback of *Saints, Scholars and Schizophrenics* in hand – searching among the dispersed mountain hamlets for some of the key protagonists of the book. And so, the drama of hide-and-seek played between villagers and their various defenders, unabashed curiosity seekers and global interlocutors continues to this day.

Today, of course, neither 'Ballybran', anthropology, nor the ethnographer are what they were in the mid-1970s. The Ballybran that I describe here is barely recognizable. The last of the real thatched farm houses have been razed and modern suburban ranch style homes have appeared in their place. The only 'thatched cottage' in evidence is Nellie Brick's former tea-rashers-butter-and-bread shop now being renovated as a snug and romantic pub for the pleasure of tourists. The interior is rustic English countryside and the thatch has been imported from Poland. But the thatchers, at least, are from Killarney even if they learned their 'traditional' trade courtesy of a development grant from the European Union. Still, the thatch smells as sweet and inviting as ever, and some kind soul had thought to stick a cardboard sign on a window sill indicating 'Nellie's window', the vantage point from which the wonderful old wag had once kept tabs on the village world.

Still, were I to be writing the book for the first time and with hindsight, of course there are things I would do differently. I would be inclined to avoid the 'cute' and 'conventional' use of pseudonyms. Nor would I attempt to scramble certain identifying features of the individuals portrayed on the naive assumption that these masks and disguises could not be rather easily de-coded by villagers themselves. I have come to see that the time-honored practice of bestowing anonymity on 'our' communities and informants fools few and protects no one – save, perhaps, the anthropologist's own skin. And I fear that the practice makes rogues of us all – too free with our pens, with the government of our tongues, and with our loose translations and interpretations of village life.

Anonymity makes us unmindful that we owe our anthropological subjects the same degree of courtesy, empathy and friendship in writing as we generally extend to them face to face in the field where they are not our 'subjects' but our boon companions without whom we quite literally could not survive. Sacrificing anonymity means we may have to write less goodbye that I have held as dear over the many years as this one which had been wrested from the giver with so much difficulty.

The supreme irony is that the anthropologist who has always been in search of a relatively classless, genderless, egalitarian society, had stumbled on to one early in her career without ever recognizing it as such or singing its praises in this regard. This village egalitarianism was expressed as well in the painful decisions that had to be made about inheritance, the argument that was so central to my thesis. While these decisions never came easily to either generation, parents or children, in the end they were decided with a strong commitment to fairness and with attention to correcting the unwitting losses experienced by one sibling at the hands of the other. Unlike rural English patterns of primogeniture based on a 'winner takes all' model, Irish farm families always strived to settle each of their 'disinherited' sons and daughters with some kind of life security – whether through carefully sought after connections with potential patrons in commerce and the trades in the next town (see Arensberg, 1937) or through the Catholic Church and its extensive web of educational and social welfare institutions, or through helpful relatives and former neighbors abroad – so that virtually no 'disinherited' Irish child was sent out into the world to 'seek their fortune' alone as had so many generations of 'disinherited' rural English children (*see* Birdwell-Pheasant, 1998). Consequently, the 'traveling' and diasporic Irish, including over the generations a great many from the little parish of An Clochan, have contributed, disproportionately, to the culture and civilization of the larger English-speaking world (see Hout, 1989: chapter 5; Keneally, 1998). For all these reasons and for whatever it could possibly matter now – all credit to An Clochan.

## Crediting Ethnography

> To begin with, I wanted that truth to life to possess a concrete reliability, and I rejoiced most when the poem seemed most direct, an up front representation of the world it stood in for or stood up for or stood its ground against. (Heaney, 1995:12)

At the heart of the anthropological method is the practice of witnessing, which requires an engaged immersion over time in the lived worlds of our anthropological subjects. Like poetry, ethnography is an act of translation and the kind of 'truth' that it produces is necessarily deeply subjective, resulting from the collision between two worlds and two cultures. And so, the question often posed to anthropologist-ethnographers about the dangers of 'losing one's objectivity' in the field is really quite beside the point. Our task requires of us only a highly disciplined subjectivity. There are scientific methods and models appropriate to other ways of doing anthropological research, but ethnography, as I understand it, is not a science.

Very much like the poet who decides to enter another oeuvre for the purpose of translation – Seamus Heaney, for example, describing his entering the poetry of Dante[3] – the anthropologist sees something in another world that intrigues them. It can be as simple as 'Oh, I like that! Let me see if I can't understand how that particular mode of being and thinking and feeling and sensing the world works, the sense it makes, the logic and the illogic of it, the pragmatics and the poetics of that other way of life.' And so we think, 'Yes, I'll go there for a while and see if I can't come back with a narrative, a natural history, a thick description – call it what you will – that will enrich our ways of understanding the world'. Like any other form of 'translation' ethnography has a predatory and writerly motive to it. It is not done 'for nothing' in a totally disinterested way. It is *for* something, often it is to help us understand something – whether it is about schizophrenia as a projection of cultural themes or about ways of solving perennial human dilemmas around the reproduction of bodies and families and homes and farms.

In referring to his own long-term project of translating the Beowulf, Seamus Heaney (1999) drew on a generative metaphor based on the Viking relationship between England and Ireland, distinguishing between the historical period known as the Viking raids and the period known as the settlement. The raid, he said, is a very good motive for poetic translation. The poet can raid Italian or German poetry and come back with a kind of 'booty' scandal, just as I could sit and talk with the local curate over a mid-morning cup of tea in his living room with the priest still in his pajamas.

House-keeping, gardening and meal-preparation were kept to a minimum, thus freeing both women and men for other voluntary activities and a good deal of leisure time that was spent in fostering friendship and conviviality – for men in one of several village pubs, at local sheep fares and regional markets, and for women in shops, church and school related activities, and for older women and widows in house calls to friends and far-out kin. There was time out for story-telling and time out for play. There was time to gather around deaths, wakes and funerals – a full day was given over for the funerals of each of the 38 villagers who died during 1974–5. Everyone had radios and some owned televisions, but most people still preferred 'live entertainment' and they gathered frequently, especially during the winter, at pubs, church halls and in each others' homes to entertain themselves with their own music, singing, step-dancing, and poetry recitation. Both young and old, male and female, were encouraged to develop their own repertoire of songs, recitations, or 'steps' which they could be called on to perform at the drop of a hat. Though the shyness and modesty of bachelors could be heart-breaking, the institutionalized pattern of 'coaxing' could bring even the most reluctant fisherman or shepherd to perform his 'party piece' and shine before his peers.

The ethic of modesty and deference assured that no one singer ever stood out or sought undue attention. Meanwhile, the reciprocal call and response mode – 'Sing us a song, Paddy'; 'Oh, I couldn't', etc. – allowed for the limited expression of praise and appreciation which could always destabilize into 'codding' – 'Sure, he's the best singer in the village'. Together these promoted a strong sense of community solidarity at the expense of the individual, aimed as they were at suppressing any hint of unseemly arrogance or self-importance. In other words, social equality was fostered through the very same witty games of 'codding', 'giving the mickey' and 'having a crack' which I had described in *Saints and Scholars* as having a decidedly adverse effect on the more psychologically vulnerable individuals who were less able to evaluate and respond appropriately to the 'double-binding' messages they carried. To wit: refuse the praise and you are putting a damper on the high spirits of your companions; accept the praise and you appear the fool for taking it seriously.

Gregory Bateson, who had developed the 'double bind' theory of schizophrenia that I used in my book, understood that human communication patterns were extremely complex and that some double-binding injunctions were damaging to certain individuals while some were beneficial, even therapeutic to others. The verbal duels and interactional challenges so characteristic of rural Irish wit may have contributed to the cognitive dissonance suffered by Irish schizophrenics unable to differentiate literal from metaphorical truth. But just as surely these communication patterns contributed to the development of Ireland's long tradition of saints, poets and scholars as well.

So, while I told the anecdote about the cruel codding in the pub of a shy bachelor who was teased unmercifully about his inability to speak to me without stammering, I failed to tell the anecdote about the day of our leave-taking from the village when I saw out my front window the very same painfully shy man standing under a tree at the bottom of the little path that led up to our cottage. I wondered what he was doing there, 'loitering' for such a long time. I went about my packing and house-cleaning, but each time I passed the window I saw him standing there, so still, hardly changing his posture. Finally, after a few hours, it occurred to me that perhaps he was waiting for me to come down the path on my way to the village after an errand. So, I packed up the babies into strollers and backpacks and we made our way down the path as if on our way to the village post office. As I came close to Paddy, I shyly lifted a finger and crooked my neck at him in the traditional, understated Kerryman greeting at which Paddy came forward and put out his hand which I clasped in both of mine as he said: 'You're leaving us. I just wished . . . wanted . . . well . . . God bless you, Mum. And God bless Michael and the wee ones, too.' In all my many comings and goings as an anthropologist, there was no

Dubh, the crooked one, come back to An Clochan.' Indeed, I *was* beginning to feel very much like Crom Dubh, the pagan force and alter-ego of the village who epitomized everything dark, hidden, secret, and overgrown, tangled among the brambles of the old graveyard – everything that needed to be resisted. My presence was a daily reminder – 'salt in the wound' said one villager – of everything they would like to hide, deny and secret away.

In fact, however, most villagers did not avoid me. Many fell back into the old habit of telling me poignant stories and catching me up on people, events and changes in the parish. At times there seemed to be a pressure, even a hunger to speak. Kathleen shook her head one evening: 'You are like the village analyst and we are all on the couch. We can't seem to stop ourselves from talking.' It made no difference that I was not back looking for secrets, for there was simply no way of escaping them. Since I had no other reason for being in the village except to visit with people, my presence became something of an obstacle, even to myself. In this small world, words were as dangerous as hand grenades or bullets, as much for those who gave as for those who received them.

An older couple took the risk of going about with me in public at considerable social risk to themselves. It was, they said, the Christian thing to do, and never mind what others thought or said. Aiden even appointed himself my colleague in arms and after an afternoon of making house calls together, he commented wearily: 'Ah, but this fieldwork is tiring'. But as the situation grew more prickly I asked the new priest of An Clochan to help me call a parish meeting so that I could apologize in general terms for any pain I caused the community and so that villagers who wished could collectively express their anger. Then, I hoped, naively perhaps, we could clear the air and move forward. I explained how difficult it was to try to do this work of repentance and explanation door to door. The priest was unsure, however. 'Will you be up for it?' he mused. 'And will *they* be up for it? Is this drawing too much attention to an old hurt? Should you apologize? Would this be good thing?' The good priest promised to mull it over with a few confidants in the community and he promised to get back in touch with me. 'But come to Mass this Sunday', he urged. When, a few days later, I approached the Communion line, Father M. held the Host up high and looked about him reciting my name very loudly, indeed: 'Nancy, receive the body and blood of Christ'. But after Mass he said that a parish meeting would be too risky and that I should just continue as I was doing, making my rounds, door to door, the best I could. As I walked home alone from Mass I wondered how much longer I should stay.

The 'drumming' out of the village, when it came, was swift. There were warning signs a few days before that trouble was brewing. Conversation would suddenly stop when I entered a pub, and I would smile weakly and turn on my heels. During an afternoon drive I was taken past a few sites that had been subject to local harassment, including car and house bombings. No one had ever been hurt in these attacks, but the damage to property was considerable and the message conveyed was clear. The parish was controlled, in part, by threats and intimidation by a small but active group of local cultural nationalists. Among the kinds of people 'unwanted' in the village were British landowners, suspected homosexuals, purported drug dealers, 'gombeen men' (local petty capitalists who bought up old farms) and me, that new species of traitor and friend, the anthropologist.

My local friends were shaken by the tide of rejection, and they were understandably conflicted by divided loyalties. On the last evening of my stay in An Clochan I returned to my B&B filled with stories to share. It had been a good day and I had managed to make contact with some dear old acquaintances. My flagging spirits were on the rebound. But as I popped my head into the kitchen to tell B. that I'd be down for tea in a few minutes, she turned from the stove with a face that was flushed by more than the gas burners. 'I have some terrible new', she blurted. 'Is something wrong at home?' I asked, clutching at my throat. 'Did something happen to Michael or one of the children?' 'No, no, not that. But, Nancy, you have to leave. Right now. This evening. You can't eat here. You can't sleep here anymore.'

'Did I do something wrong?' I asked. 'Did I offend someone in the village today?' It was called 'imitations' of Homer or Virgil, for example. Or, alternatively – as Heaney did with the Beowulf translation – the poet can approach the translation through 'settlement', that is, entering the oeuvre, 'colonizing' it, taking it over for one's artistic purposes. In settling in with the work, you stay with it a long time, identify with it in an imaginative way. You change it and it changes you.

Similarly, there are 'raiding' and 'settlement' ways of doing the work of anthropological translation, although granted both these metaphors play on our discipline's worst nightmares. Neither raiders nor settlers have much currency in the parts of the post-colonial world where most of us still work. In our vocabulary, 'raiding' is what Margaret Mead sometimes did – going in and after a culture in order to raid an idea, a practice that could be useful to young mothers in Boston or to adolescents in Los Angeles. Another form of raiding is the kind of 'quick and dirty' research we sometimes conduct with a specific goal, such as evaluating an AIDS prevention program in Botswana or a child survival program in Northeast Brazil for a governmental or international agency. Quick and dirty – a raid, if you will – but necessary at times and valuable in its own right.

And, then, there is ethnography and participant observation – the settlement metaphor par excellence. Here we enter, settle down, and try to stay for as long as people will tolerate our presence. As 'travelling people' we are at the mercy of those who agree to take us in as much as they are at our mercy in the ways we represent them after the living-in and living-with is over. Anthropologists are a restless and nomadic tribe, hunters and gatherers of human values. Often we are motivated by our own sense of estrangement from the society and culture into which we were existentially thrown. I went to rural Ireland, in no small part, in search of better ways to live and I found these especially among some of the old ones with whom I spent the greater part of my days and long winter evenings in An Clochan and who, perhaps, biased me toward an overly critical view of village life in the mid-1970s.

## Rabbit Run: Taking Leave

The fateful visit with Martin spelled the beginning of the end of my return to An Clochan. By the next day I was beginning to feel the weight of social censure closing in, not so much on me personally, as on those in the village who had taken me in – in the village vernacular who had 'fed me and kept me' – or had taken me under their wing. When S., for example, arrived to meet me for breakfast the next morning, she was in a state of considerable agitation. She had not slept well the previous night. 'I was awakened', she said, 'by a terrible nightmare. Oh, it was an awful sensation, as if my house was being invaded by a dark force, an ill-wind, or an alien invader.' She looked hesitantly to me for a clue to her ominous dream. I replied only that houses were often symbols of the body and of the self and left it at that.

But that night it was my turn to be awakened by a ghostly visitation, a hooded creature who pointed a long skinny finger over and beyond my head and toward the sea. Like Scrooge, I was happy to find myself unchanged in the morning and I suppressed the urge to hug the wooden bedstead promising: 'I am *not* the woman I was, I am not the woman I was'. But I knew this to be untrue in certain fundamental ways. And I took out my little notebook – the one that would ultimately prove to be my undoing – and jotted down a few ragged thoughts.

Shaken, I continued my daily rounds of the village, by now heavy of heart, and uncertain of step. I waved to a solitary hay-maker, the first one I'd seen in several days. He did not recognize me and he stopped to take a break. Making small talk I asked why the man took such care in making several small little hay cocks rather than larger haystacks. 'Because the hay is much sweeter this way and it pleases the animals more', he replied, tipping his cap as I walked along. After the visit with Martin I began to walk the country roads with my head bowed, practicing a government of my eyes so as not to elicit an automatic greeting from those who might later regret it. And I took to announcing myself at the open door of older friends and acquaintances: 'It's Crom *really* sorry.' In their view this would mean nothing less than a renunciation of self and of my vexed profession, a move I could not take. *Saints* was written from a particular perspective at a particular moment in time and by a particular sort of anthropologist-ethnographer. And time, as they say, is a great healer. There is no such thing as everlasting ire anymore than there is undying love. Anything can change. A sense of proportion and a sense of humor may eventually replace injured pride. And in the meantime, as the Tailor of Ballybran would have said, 'just leave that there.' The next 25 years may pass even more swiftly than the last. And, God willing, by then *both* Crom Dubh and I will have found a way to return to 'our' village.

evening, I was dog-tired, and my feet were sore. I had no transportation. Was it even possible to call up a taxi from distant Tralee at this hour? 'Is there anyone else who can put you up tonight?', B. asked. 'Let me think', I said stupidly, 'while I go upstairs to pack.' In the little attic room I moved slowly as in a dream, folding my few things into the suitcase pulled out from under the bed. I hadn't eaten since morning and I had missed dinner the evening before. So I was hungry as well as tired. But where could I go? Who would be safe from whatever intimidation B. had gotten? And what was she told? 'Get that woman out of here immediately before someone gets hurt'? I sat on the edge of the narrow bed and jotted down a few thoughts to clear my head. But they were so scrambled I tore out the page, crumpled it into a small wad, and tossed it carelessly into the wastepaper basket.

Outside night was falling. The closest home where I thought I might be able to stay was a mile away and I walked there quickly. My reception was kind but wary, and my new friend let me know, at last, that indeed the community as a whole had closed down where I was concerned. 'It's not fair', he said, 'But I can't not tell you that it hasn't happened. It's really not very good right now for *anyone* to be seen with you.' Nonetheless, he kindly insisted that I spend the night, or even the week, if I wished. He refused, he said, to be intimidated. 'Well, I'll go back and get my bags, but I will only stay until morning. And I'm so sorry for putting you in this situation.' 'It's only a book', he said. 'And people here will tell you on the side that it has made them rethink a thing or two, for example, about how to raise and treat one's children.' And he laughed. "The young mothers, here, they now go all out of the way to nurse their babies, and they are forever hugging them. Just to show you, I sometimes think.'

By the time I walked back to my 'guest' house to pick up my bags, my older friend and village sponsor was already waiting for me in the parlor.

'Where have you been? We've been worried. We've worked out a solution', he said glumly. 'You can spend your last night here – I'll see to it that no one blames B. – and I'll be back to fetch you first thing in the morning. Be completely ready. I'll carry you as far as Limerick and from there you'll take the bus to Dublin. No, don't argue; I insist. We can at least see you off to the next county. And we can use the extra time to talk.'

The next morning as I crept quietly down the creaking stairs I found a good strong bowl of tea and a plate of toast waiting for me in the 'guest room'. Ah, I thought, it's the *Lon na Bais*, the custom of the last meal that was left out just before an old one dies.[4] The family of the house had gathered around the long table in the kitchen for a breakfast that was taken in almost monastic silence. I tried to be equally silent in the next room. In taking my leave finally from B. she confronted me with my crime: 'All that time you spent in your room upstairs. You weren't just reading – you were writing! You left a trail in the wastepaper basket. People *said* you were writing. They saw you scribbling into your note book outside the pub in Brandon.' 'I won't deny it', I said. 'But was it such a grave sin? I needed to write my way through my own confusion and loneliness.' Then, B. gave me a quick hug and whispered in my ear, 'I'm so sorry for this. Ignore them. Keep up the good work.'

Then, the *Lon na Bais* ritual continued as my village mentor took me on our final rounds together of the village, this time to feast my eyes for the last time. Like a local funeral procession, he drove me slowly past all the sites that were dearest to me. 'Take a good look', he said. 'There's your Brandon Head. And there's your creamery, what's left of it. And here is your primary school. In a few hours the children will be lining up to march inside. And here's your Peg's pub, your Tailor Dean's house, and your old widow Bridge's cottage overgrown with brambles.' Then, as we turned the final curve past the abandoned little hamlet of Ballydubh, with the village almost out of sight, he forced me to turn around and take in the full sweep of the mountains and the sea. 'And there', he said, 'is your An Clochan. You had best say good-bye, now.'

In the end perhaps we deserve each other – well matched and well met, tougher than nails, both of us. Proud and stubborn, too. *Unrepentant* meets *Unforgiving*. So, in a way villagers were right to say 'We don't believe you are *really* sorry.' In their view this would mean nothing less than a renunciation of self and of my vexed profession, a move I could not take. *Saints* was written from a particular perspective at a particular moment in time and by a particular sort of anthropologist-ethnographer. And time, as they say, is a great healer. There is no such thing as everlasting ire anymore than there is undying love. Anything can change. A sense of proportion and a sense of humor may eventually replace injured pride. And in the meantime, as the Tailor of Ballybran would have said, 'just leave that there.' The next 25 years may pass even more swiftly than the last. And, God willing, by then *both* Crom Dubh and I will have found a way to return to 'our' village.

# Part V

# Fieldwork Conflicts, Hazards and Dangers

*Jeffrey A. Sluka*

Fieldwork is not free of tensions, dilemmas, conflicts, hazards, dangers, and even lethal hostility. Awareness of such risks is of life-saving importance to students embarking on their first fieldwork experience, and research troubles, even failures, are a source of ethnographic knowledge in themselves. In this part of the reader, these concerns are exemplified and addressed in June Nash's account of her experience of living through a strike in a Bolivian mining community, where she was forced to take sides with the strikers against the government at considerable personal and professional risk; a chapter on "Human Hazards of Fieldwork" from Nancy Howell's study of occupational safety and health in anthropology; Carolyn Nordstrom's reflexive account of her research on "War on the Front Lines" in Mozambique; and Jeffrey Sluka's article on managing danger in fieldwork, based on his research on the conflict in Northern Ireland. The selections by Nordstrom and Sluka both come from *Fieldwork Under Fire: Contemporary Studies of Violence and Survival* (Nordstrom & Robben 1995), a collection of essays that explore the dynamics of sociopolitical violence, written by anthropologists who have conducted fieldwork with victims, perpetrators, and survivors in war zones and other sites of conflict. One of the main concerns the volume addresses is the distinct research problems and experiences of ethnographers who study situations of violence, and the theoretical issues that emerge from studying topics that involve personal danger (Nordstrom & Robben 1995:4).

June Nash's fieldwork in a Bolivian tin-mining community in the early 1970s led to the publication of her outstanding ethnography, *We Eat the Mines and the Mines Eat Us: Dependency and Exploitation in Bolivian Tin Mines* (1979). In "Ethnology in a Revolutionary Setting," she recounts some of the difficulties, suspicions, and dangers she experienced while conducting her fieldwork, during a time of industrial unrest and political disorder. Nash was one of the first modern anthropologists to seriously address the issue of neutrality in fieldwork, and she concluded that "In Bolivia it was not possible to choose the role of an impartial observer and still work in the tin mining community . . . The polarization of the class struggle made it necessary to take sides or to be cast by them on one side or the other. In a revolutionary situation, no neutrals are allowed" (1976:150). In her research, the support and approval she received from the mine management aroused suspicions among the workers; when a state security agent investigated her work, she began to worry about the danger her notes and tapes could bring to her informants; and she eventually had to respond to their growing suspicions that she was a CIA agent. These difficulties threatened her ability to continue her research and her personal safety. In the end, she was able to maintain relations with the mine management, the union, the tin miners, and the political authorities all at the same time, and convince the community she was not a CIA agent or threat. She concludes that, in revolutionary or violent settings, the traditional scientific attitude of impersonal objectivity is inappropriate and that "We can no longer retreat to the deceptive pose of neutrality" (1976:164).

Nancy Howell's *Surviving Fieldwork* was the first, and so far only, comprehensive study "about anthropologists, specifically, about the fieldwork of anthropology, the risks that are taken, and the prices that are paid for doing fieldwork in the ways we do" (1990:1). At Howell's instigation, in 1986 the Board of Directors of the American Anthropological Association established an Advisory Panel on Health and Safety, and invited Howell to head it and produce a report on occupational safety and health in the discipline. This report showed that anthropology can be dangerous and that hundreds of anthropologists have failed to protect themselves from dangers and been victims of fieldwork. Field accidents and illnesses are a serious component of professional work in anthropology. From snakebites and truck crashes to severe sunburn and diarrhea, fieldwork is regularly interrupted by crises and problems. Few fieldworkers escape harm entirely. Many are hospitalized, lose fieldwork time, or suffer long-term or permanent disabilities. At least sixty people died from fieldwork mishaps during the 1980s alone. Howell's study details the kinds of threats to health and safety experienced by anthropologists in the field, with frequencies estimated from an anonymous random sample, and illustrated by accounts from named volunteers who tell of their own problems.

*Surviving Fieldwork* was intended to help fieldworkers anticipate the dangers they will face and prepare for preventing and responding to them, and includes chapters on various hazards, including those presented by humans and animals, exposure, injury, accidents, parasitic, infectious, and degenerative diseases, mental health and illness, families in the field, practicing medicine in the field, and making fieldwork safer. In the chapter "Human Hazards of Fieldwork," selected for this reader, Howell observes that "many anthropologists suffer interpersonal attacks during the course of their fieldwork" and provides descriptions and discussion of the "hazards of human hostility and conflict" (1990:89), including physical violence; rape and attempted rape; murder, suicide, and other mysterious deaths; political hazards, such as arrest and suspicion of spying; and political turmoil, such as military attack, factional conflict, hostage-taking, and assassination.

Carolyn Nordstrom is one of the leading anthropologists of war and armed conflict. In "War on the Front Lines," based on her fieldwork in the 1980s on the front lines of the war in Mozambique, she explores three interrelated themes of chaos, the "crisis of reason," and creativity, and is particularly struck by how creative people are in surviving and adapting to contexts of "dirty war" where civilian populations are targeted for terror tactics; "what may be the most powerful aspect of studying war is not merely the deconstructive violence that attends to it but the creativity the people on the frontlines employ to reconstruct their shattered worlds" (1995:131).

Nordstrom was interested in the war and Mozambicans' experience of it, and she argues that this research question required different field techniques than those normally associated with ethnographic studies set in specific locales or communities. She developed her own approach which she terms "ethnography of a war zone," in which the theme of war, rather than a specific locality, situates the study, and a focus on process and people supplants place as an ethnographic site. Rather than living and conducting research in one locale or community, she traveled the country extensively, visiting villages, towns, and provincial capitals, and following the war from urban centers to rural outposts – from "the plush offices of powerbrokers to the crumbling embers of villages in the far reaches of the country" (1995:144). In particular, she visited locations on the front lines of the war to interview local people about their experiences, and she depended primarily on air travel to get there. She became a master of catching rides, mainly on cargo planes taking emergency supplies to war-devastated areas, and dubbed this "runway anthropology" (1995:140).

In the last two decades or so, an increasing amount of research has been carried out by anthropologists working in war zones and sites of sociopolitical unrest and violence. Nordstrom's article exemplifies some of the difficulties, hazards, and dangers of fieldwork in such contexts.

In 1990, the same year Howell's *Surviving Fieldwork* was published, I was the first anthropologist to publish an article specifically on managing danger in fieldwork as a methodological and subjective issue (Sluka 1990). (More recently, sociologists have begun to write about danger in fieldwork; see Lee 1995, Lee-Treweek and Linkogle 2000.) The selection here is an updated and elaborated version of that 1990 original article, incorporating subsequent fieldwork I conducted on the conflict in Northern Ireland. I begin by observing that "anthropological fieldwork is more dangerous today than it was in the past" and "few anthropologists will be able to avoid conflict situations and instances of sociopolitical violence in the course of their professional lives" (1995:276). I argue that while special ethnographic, methodological, theoretical, and ethical sensitivities are required when working on and in sites of sociopolitical conflict and violence, these dangers can be mediated through foresight and planning. I present a reflexive account of my fieldwork with Irish nationalist militants, and make practical recommendations on how to enhance personal safety when conducting ethnographic research in dangerous or violent social contexts. These recommendations are intended as "a starting point from which others considering fieldwork in dangerous contexts can map out their own strategies for conducting fieldwork safely" (1995:290).

One of the most important conclusions is that one of the main sources of danger for anthropologists in the field is the nearly universal suspicion that they may be spies. This is an old concern in the discipline that goes back at least as far as the Franz Boas "Scientists as Spies" controversy in 1919. I note that:

> Because the most common suspicion that research participants have about anthropologists is that they are spies, and it is difficult to find an anthropologist who has done fieldwork who has not encountered this suspicion, this danger deserves special mention. Being defined as a spy [let alone actually being one, since death is the usual wartime penalty for spies] is inherently dangerous, and the link between anthropology and war-related research has exacerbated this danger. Anthropologists have been involved in war-related, particularly counterinsurgency, research, others have had their research used or "applied" by governments, militaries, and intelligence agencies to help plan

> military operations, and spies or intelligence agents of various sorts have used the cover that they were anthropologists. As a result, people in many parts of the world have come increasingly to believe that anthropologists, even those engaged in "innocent" (or in Boas' terms "honest") research, are actually or potentially dangerous to them. Many nations and peoples are therefore justifiably suspicious of anthropologists and will not allow them to do research, and fieldwork has become more dangerous today than it was in the past. (1995:283)

In her article on "Human Hazards of Fieldwork," Howell also highlights the danger that being suspected of being a spy has represented for many anthropologists, and in her article Nash describes how she faced and overcame a serious threat of just this sort.

This issue is of particular contemporary relevance, because in 2005 the already considerable danger represented by anthropologists being suspected of being spies was exacerbated by the establishment of a new CIA scheme to sponsor trainee spies through American university courses. In the aftermath of the September 11, 2001 terrorist attacks on the United States, the Pat Roberts Intelligence Scholars Program was launched to improve US intelligence-gathering capabilities. Armed with $4 million in scholarships, it pays anthropology students, whose names are not disclosed, up to $50,000 a year to use their postgraduate training and fieldwork to gather political and cultural details on other countries, and, after graduation, they are expected to go on to work directly for the CIA (Price 2005). This development was met with anger and concern among American and British anthropologists, and, in particular, John Gledhill, president of Britain's Association of Social Anthropologists, called the scholarships ethically dangerous and divisive, and warned that they could foster suspicion within universities worldwide and cause problems in the field (BBC 2005).

In 1919, Franz Boas, who for two decades had dominated American anthropology, was formally censured by the American Anthropological Association (AAA) for publishing a letter in *The Nation* condemning anthropologists who used their profession as a cover for spying during World War I:

> Sir: In his war address to congress, President Wilson dwelt at great length on the theory that only autocracies maintain spies; that these are not needed in democracies. At the time that the President made this statement, the Government of the United States had in its employ spies of unknown number ... The point against which I wish to enter a vigorous protest is that a number of men who follow science as their profession, men whom I refuse to designate any longer as scientists, have prostituted science by using it as a cover for their activities as spies.
>
> A soldier whose business is murder as a fine art, a diplomat whose calling is based on deception and secretiveness, a politician whose very life consists in compromises with his conscience, a business man whose aim is personal profit within the limits allowed by a lenient law – such may be excused if they set patriotic devotion above common everyday decency and perform services as spies. They merely accept the code of morality to which modern society still conforms. Not so the scientist. The very essence of his life is the service of truth ... [Any person] who uses science as a cover for political spying, who demeans himself to pose before a foreign government as an investigator and asks for assistance in his alleged researches in order to carry on, under this cloak, his political machinations, prostitutes science in an unpardonable way and forfeits the right to be classed as a scientist.

> By accident incontrovertible proof has come to my hands that at least four men who carry on anthropological work, while employed as government agents, introduced themselves to foreign governments as representatives of scientific institutions in the United States, and as sent out for the purpose of carrying on scientific researches. They have not only shaken the belief in the truthfulness of science, but they have also done the greatest possible disservice to scientific inquiry. In consequence of their acts every nation will look with distrust upon the visiting foreign investigator who wants to do honest work, suspecting sinister designs. Such action has raised a new barrier against the development of international friendly cooperation. (1973:51–2)

It is ironic that in 2005, at the same moment in history when the AAA membership was voting overwhelmingly in favor of a resolution to rescind Boas' censure in 1919 for condemning anthropologists who served as spies, the largest ever government-sponsored program of recruiting anthropologists as spies emerged. This disturbing development promises to exacerbate the already considerable difficulties and dangers all fieldworkers face of being suspected of being spies, particularly CIA agents. This represents a serious threat both to anthropologists' ability to gain access to fieldwork sites, because it undermines our ability to gain the trust, rapport, and consent of research participants, and to the "occupational safety" of anthropologists in the field.

# 15

# Ethnology in a Revolutionary Setting

*June Nash*

There is a growing gulf between the anthropologist's two roles, that of field researcher and that of analyst. In the first role, we share the lives of the people we study and identify with them in the conflicts they face (Gough, 1968:4; Henry, 1966) as we "try the intimate experience of another upon ourselves to test our hypotheses" (paraphrasing Lévi-Strauss, 1969:51). In the second, we must objectify and distill our experiences. Ever since we discovered that secrecy was a defense against the dominant culture, we have been increasingly aware that our data may be used against those whose lives we have shared. In the period of decolonization, as Maquet (1964:48) has shown, the anthropologist has come to be classified with the enemy. Even where national independence is established, our material can be and is being used to counter popular uprisings. The people we study are often cut off from the data we publish by a language or literacy block. Without our knowledge, our material may be fed directly to the "man in the field with the civic action program; working with a military establishment . . . the person in psychological operations who has the basic fundamental studies that give him understanding of the masses" (in the words of Dante B. Fascell, chairman of the House of Representatives Subcommittee on Inter-American Affairs; see US Government, 1969). Since we have no official audience with statesmen or policy makers, we do not know how or whether our publications influence policies that will affect the lives of the people we study. Lacking control over the product of our research, we have lost the basis for social responsibility.

The issues raised by Project Camelot and the publication of the Thailand counter-insurgency research reveal the need to set ethical standards within the profession (Jorgensen, 1971, Wolf and Jorgensen, 1970). Berreman (1968) has gone beyond the issue of professional standards to signal the danger of leaving the use of our data to others: "politicians and journalists," "madmen and scoundrels," as well as "statesmen and benefactors." Stavenhagen (1971) has called for "decolonizing the profession."

In order to work out an understanding of the role we can begin to play, we need accounts of concrete field experiences such as those that Maquet (1964), Henry (1966), and Jones (1971) have given us. This report provides a comparative instance based on my recent field experience in the revolutionary setting of Bolivia. In my previous fieldwork with the Maya of Chiapas, Mexico, the impact of modern change was only indirectly felt. The old power structure of curers and diviners who controlled the supernatural was breaking down, and men who had been protectors of

June Nash, "Ethnology in a Revolutionary Setting," pp. 148–66 from Michael A. Rynkiewich and James P. Spradley (eds.), *Ethics and Anthropology: Dilemmas in Fieldwork* (New York: John Wiley & Sons, 1976).
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the community were being killed as witches. Hostility was turned inward, as a rising incidence of homicide within the community indicated. The defensive insulation of the community against the outside world protected me from the kinds of issues that arise in studying groups in the mainstream of change. People did not involve me in the witchcraft conflict that was the central struggle in their lives.

In Bolivia it was not possible to choose the role of an impartial observer and still work in the tin mining community of Oruro, where I had gone to study ideology and social change. The miners, who spoke Quechua and or Aymara as well as Spanish, had entered the modern industrial sphere and were demanding power in it. The polarization of the class struggle made it necessary to take sides or to be cast by them on one side or the other. In a revolutionary situation, no neutrals are allowed.

In the 146 years since Bolivia's independence, there have been 186 uprisings, resulting in more than 150 changes of government. Only one of these movements resulted in the formation of a legitimized, democratically elected succession of leaders seeking structural changes that would warrant its being called a revolution: the uprising of the National Revolutionary Movement (MNR) on April 9, 1952, when the people fought for the right to seat president-elect Victor Paz Estenssoro after he had been refused power by the oligarchy of tin barons. Eventually Paz lost the confidence of the masses who had supported him as he turned to a false "development" based on loans and increasing external control. In the convulsive spring (our fall) of 1964, workers' strikes and student protest led to his withdrawal. Under the banner of the "revolution of reconstruction," Rene Barrientos Ortuno, a general who had become Paz's vice-president under pressure from the army, took advantage of the rebellion. Reneging on the promises he had made to labor, he instituted four years of the worst repression Bolivia had suffered since the days of the "Butcher" Mamerto Urriolagoitia, installed in the presidency by the tin oligarchy in 1948.

When I arrived in La Paz in July 1967, Che Guevara was still fighting in the tropics of Santa Cruz. Barrientos's troops had massacred 87 men, women, and children in Siglo XX Catavi on June 23. The massacre was precipitated by the Congress of Miners' Unions planned for the following day and was possibly an attempt to discourage workers from supporting Che Guevara's guerrilla movement. I took a bus to the old mining center of Oruro, where I found the San José mine paralyzed by the reorganization of the mines according to plans proposed by the Inter-American Development Bank as a condition of its loan to the Nationalized Mining Corporation of Bolivia. The corporation had just fired over 200 women who had worked in the concentration of metals and replaced them with men and machines. I spoke to only a few people on my first visit, a teacher in the company school, who sympathized with the workers and told me some of their problems; a woman in *chola* dress (the sign of transition from an Indian culture to urban life) selling candy and fruit punch to miners as they came off their shift; and a gatekeeper who was no longer able to work inside the mines due to silicosis, the "professional illness" of miners, but all of them spoke bitterly of the government and the nationalized administration. I read the writing on the walls of all the company buildings calling for the "fight against imperialism" and death to the military assassins and parasites," signed with the initials of the various political parties and union federations. In large red letters, dripping from the hastily executed inscription, the word LIBERACION dominated the walls of the company store. It was as though the cry of the French Revolution for "liberty, equality, and fraternity" had been reduced to its minimal demand – liberty to work out their own destiny. I was determined to return to this place that revealed, even in such a short visit, the turbulence of a society holding on to a precarious niche in an industrial empire at the same time as it was trying to come to grips with an imposed system of exploitation.

I returned to Bolivia for a summer field session in June 1969 to study the ideology of tin-miners. Barrientos had died in an air crash two months before, and his vice-president, Luis Adolfo Siles Salinas, had scheduled an election for May, 1970. Most of the mines were running, as they had for several years, at a loss, decapitalized by inefficient management and the transfer of capital into military equipment. Pensioned workers were not receiving subsistence checks that, even when they arrived, could not cover food costs. Government-employed teachers had not been paid for months. Union leaders were in hiding or exile, and "yellow" unions were serving as spies for the management. Mining police received more pay than miners. Their job was to catch *jucos*, usually employed miners, who entered deserted shafts at night and "stole" what they considered to be their national riches. Many of the older engineers felt that productivity would increase and costs decrease if the police were fired and the company bought the metal the *jucos* extracted.

I spent an afternoon in the local office of the mine management, waiting for permission from La Paz to go ahead with my study. I had already spoken to some of the officials in La Paz, but the administrator of the San José mines wanted direct communication from them. When the assistant to the manager of industrial relations finally received a telegram approving the project, he went far beyond what I had requested in the way of cooperation and arranged for me to conduct interviews in the anteroom of his office. Furthermore, he ordered the superintendent of mines to send miners to be interviewed when they finished work. I felt I couldn't refuse without arousing suspicion, so I started interviews on work conditions under these trying circumstances, where the assistant manager could hear everything that was said and where the men, tired after a day's work, were compelled to talk with a strange *gringa*.

One of the first miners I interviewed demanded that I explain what my study was about before he replied to my questions. I liked his forthright attitude and explained in detail. He seemed satisfied and spoke with interest and involvement. Another of the interviewees who impressed me in these early interviews was a watchman who, like all the others working in nonskilled jobs at the surface, could no longer work inside the mine because of silicosis. Since his duties were not pressing, he came by twice in the following week. He was more relaxed when I asked questions about ritual and folklore than when I questioned him about conditions in the mine. Once he brought up the massacres that had taken place in the mines, his tone turned from bitterness to compassion for the dead and he wept as he spoke of the massacre of December 21, 1942 in Siglo XX Catavi. Tears came to my eyes as he spoke of "our history" and of how Maria Barzola, a woman worker in the concentration of metal had seized the Bolivian flag during the march to the administrative offices to demand "more daily bread" and had been shot along with other men, women, and children by the soldiers called to the defense of the mine. Soon after that he and some of the other miners I had met in the course of my interviews invited me to their homes, and I could avoid the restrictive atmosphere of the administrative office. I think I had passed some kind of test that allowed me to go beyond a barrier to communication, a barrier that might never have been withdrawn, if I had remained a "stranger" (however desirable some ethnologists believe this to be [cf. Jarvie 1969]).

I was visiting in the mine one day when I heard that an agent of the Department of Criminal Investigation (DIC) had come to investigate what I was doing. When I returned to the house where I rented rooms, I found all my notes and tapes removed. I later learned that my student had taken them in a laundry sack to a friend's house in the mining community, but I had a few bad hours reflecting on the danger my notes could bring to my informants. The agent returned, and (remembering U Nu's "tension-releasing lunches" with his cabinet from my Burmese days of fieldwork), I invited him for breakfast the following day. That afternoon I received a call from one of my miner friends, who had begun an autobiography. He had heard the DIC was after me and was curious to know what had happened. I assured him that I would not let the DIC see any of his work, and said that I would burn it first if there was any danger. He protested against such drastic action; it made me feel good that he had confidence in me and was committed to the work we were doing. That evening we gave all the notes to a student who was traveling to La Paz retaining only a few myths and folk tales to show the DIC. The agent arrived promptly the following morning and waited for us to return from the anniversary mass of a friend's deceased mother. After we had chatted about folklore and rituals, he went on to tell us about his student days at the University of Wisconsin, where he said he had had a scholarship to study "counterinsurgency," and about his friendship with an American CIA agent working in Oruro. Then he left, after asking only to see our passports, although on the previous day he had demanded that we show him all our notes.

Political campaigns for a presidential election were underway when I left Bolivia in September. The Mayor of La Paz, Armando Escobar Uria, was gaining popularity as a candidate. I was not surprised to read shortly after that Alfredo Ovando, who had helped Barrientos attain power and who had little reason to believe that he could win a democratic election, had seized power on September 26. Desiring to outdo Siles, who had been promising to revise the oil code to increase the national share of the wealth of the American holdings in Santa Cruz, he nationalized the oil company. Economically, it would have been preferable to await the installation of a gas line to Argentina's markets, but the move was dictated by the political urgency of stabilizing a weak military coup. Yielding to pressure from intellectuals and workers, Ovando freed some of the jailed union directors, who returned to work in the mines. When I returned to Bolivia in January, 1970, I sensed the uncertainty that Ovando's moves had created among the miners. Some called for a position of *acerquismo*, getting closer to the center of power, by supporting Ovando and trying to influence policy. Others distrusted the attempts of what they felt was an opportunistic regime, still dominated by the military, to gain support among the masses. Wages and contracts cut in half by Barrientos in 1965 in a "temporary" austerity measure, remained at the same low level, a little under a dollar a day.

Labor had begun to rebuild its shattered organization. I visited the new secretary-general and presented my credentials and plan for a study of the mining community. (I had avoided the former representative because of the low esteem the miners had for him and their suspicion that he was a spy for management.) In April I attended a weeklong congress of the Federation of Mine Workers' Unions at Siglo XX Catavi, where old leaders and new gathered for the first time in five years to plan a program of action and elect a directorate. I was permitted to attend all of the sessions and to tape record the proceedings, except for those of the political commission. The regular attendance of myself and two assistants became something of a joke. It seemed too obvious a stunt for the CIA to pull – having a "blond" *gringa* sitting in front of a nearly all-male audience with a large paisley-covered tape recorder. For those with lingering doubts about my presence, it might even have been appealing that they had their own specially assigned agent bugging them. I felt that immediate feedback was essential to justify my presence to the miners, and so I wrote some of my impressions in an article published in *Temas Sociales* entitled "El XIVe Congreso y Después." In the months that followed, the union leadership concentrated on regaining the ground lost in the Barrientos period. The directors formulated a plan for reinstating wages at the pre-Barrientos level without producing inflation that had crippled the MNR government before currency stabilization in 1956. Their plan involved eliminating many of the bureaucratic and technical posts that had accumulated as the army invaded the administration and abolishing the mining police. Only one strike was called, and that was to demand replacements for machinery and tools in the mines. By the fall (our spring) of 1970, Ovando's government was beginning to swing farther to the right. The promised wage increase was not forthcoming and increased expenditures were made for armaments. One of the few left-of-center critics in the government, Marcelo Quiroga Santa Cruz, resigned from the Ministry of Mines and Petroleum in protest on May 18th. It wasn't just protest directly on mines wages but on a whole swing to right. He later revealed Ovando's intention of replacing all the civilian ministers with military men (*Presencia*, July 7, 1970). The only other civilian minister of the left, Kidrich Bailey, was forced to resign soon after. The union leaders alerted the workers to this swing to the right in the cabinet and called for an antiimperialist demonstration.

In the middle of July university students sent out a call to mobilize the National Army of Liberation, the guerrilla movement left over from the Che Guevara period, in Teoponte, and scores of students went there in the guise of literacy brigades. The 68 guerrillas were quickly defeated. As they were surrendering to the army, they were shot. Prisoners, some of them wounded, were killed with machine gun fire and hand grenades. Ovando refused to hand over the bodies of the victims to their families, perhaps, as rumor had it, because the thoroughly destroyed bodies revealed the brutality of the military operation. Not only relatives of the dead and political sympathizers, but many of the Bolivian people were outraged by this callous behavior. Other sectors of the middle class were alienated by Ovando's imprisonment and expulsion of priests in the middle of September.

A series of demonstrations in La Paz by university students protesting the government's bizarre handling of the dead guerrillas culminated in a march on September 21, the Day of Students. Usually a day for celebration of youth, when students crown their queens and dance, that year it was a day for rebellion against a regime that was falling into the old pattern of ineptness and repression of the resulting discontent. Union directors joined the students in a symbolic funeral of the dead guerrillas. Although the miners had rejected the movement when it was active, they proclaimed its martyrs when they were no longer a threat to their trade union aims. The streets for blocks around the university were still filled with the tear gas used to break up the demonstration when I passed by several hours later. My taxi driver said bitterly, "Each one of those bombs costs us $U.S. 10, and look what they do with them!" As I waited to meet a foundation representative in the lobby of one of the few luxury hotels in La Paz, I was appalled to overhear some of my compatriots, attending a medical convention, joking about this latest of Bolivia's revolutions. I wondered how anyone could laugh at people prepared to die rather than continue selling their lives everyday in a market over which they had no control.

The weeks following were a time of near anarchy. Ovando's government had lost its legitimacy. The big question was: When will it end?

The union in San José and other mining centers built up its antiimperialist campaign in these weeks of guerrilla action and student protest. The campaign came to focus more and more on *Yanqui* imperialism. Finally, I became the target. On October 3, Doris Widerkehr, who was beginning her dissertation research, went to tape a union meeting, as we had been doing for a special study of the rhetoric of worker organizations. She came back shaken and upset. One of the former leaders, who was himself accused by some of being a CIA agent had asked why we were allowed to tape the sessions and why we were doing the study. (Since I had had several conversations with him about the work we were doing, this seemed a tactic for diverting suspicion from himself.) The question had opened a general discussion of our role in the mining center. Three strangers from Argentina, one of whom claimed to be an anthropologist had said that they never used tape recorders in their work and that furthermore anthropologists need only 3 months for a field study and we had already been there 10 months. Doris had been asked to leave.

We discussed the events of the meeting with my compadre, a retired miner, during lunch. He advised me not to get angry when eating because the bile would burst and I might die. I tried to control my anger until the afternoon, when I went to see the secretary-general. Fortunately he was not there. Since I had not yet worked out a plan of action, I then went to see another of my compadres, who was a delegate with the union. He consulted with a compadre who had many years of experience in labor struggles, and they decided that I should draft a letter explaining my problem and methods of investigation in detail and distribute copies to all the delegates as well as to the secretary general, asking for an audience at the next meeting. I had, of course, discussed my work with the union before and had given the secretary-general copies of the articles I had published in Spanish, but changes in the union and in the political scene seemed to have made a reevaluation necessary. The strategy they outlined for me was to involve all of the men responsible for the operation of the union in the discourse, avoiding personal commitments to a single individual, who could then be suspected of being in complicity with me. I drafted the letter and revised it in accordance with comments from my compadre. The following day I passed out copies of the letter to the delegates as they were leaving a meeting concerned with an attempted coup in La Paz. The coup had begun on the morning of October 4, while Ovando was in Santa Cruz, with a radio-broadcast mandate signed by 64 officers calling for his renunciation of the presidency.

The following seven days have been called "The Week of the Generals" (Samuel Mendoza, *Presencia*, November 15, 1970). In the course of the week, six presidents entered and left the "Burned Palace." The contest became something like a football game between sectors of the armed forces, with the people listening to the radio with consternation, dismay, and a wild sense of the absurd as generals kicked the football of power from one to another.

Because I had not yet cleared up the question of whether I was an agent for the CIA, I did not go out except for brief trips to the plaza to see if any of the student demonstrations announced on the radio were taking place. A new group of DIC agents had taken possession of one of the benches in the plaza. I discovered their identity when I was about to take a photograph of soldiers massing near the Cathedral to block a scheduled student demonstration and one of the plainclothes agents rushed over to stop me, saying I had no right to photograph secret agents.

On Monday, October 5, as a result of the only "election" in two years, held in the Miraflores barracks, officers of the army called for Ovando (who had by now returned to the Palace) to renounce the presidency "for having defrauded the hopes of the people." General Rogelio Miranda, who had led the coup could not muster strong support even within the army, so he named a triumvirate to take control. Students and workers rallied behind General Juan José Torres, as the least imperialist, least Fascist, and least reactionary of the lot, to oppose the trio. When a strike was threatened for the following day, the tide turned against the triumvirate. Torres was proclaimed president on the afternoon of October 6. He promised to form a government based on *campesinos* (agricultural workers), miners and factory workers, and university students, with the support of the army, the various ministries to be divided among these four sectors of the population. The representatives of the Bolivian Workers' Central (COB) at first rejected the plan of coparticipation, on the basis of their experience with the MNR government and criticism of the compromises resulting from that episode, but later agreed to accept the posts of Housing, Mines, and Labor. Torres's attempts to find a popular base for his new government threatened a rebellion if labor were to have as much participation in government as Torres had promised them. Fearing the seizure of power by the rightist wing, the Political Command of the COB agreed to leave the new president at liberty to choose his cabinet without including them.

Meanwhile, Doris and I had made preparations in case of an attack on the Anglo-American-owned apartment building in which we lived. I took my daughter to stay at the house of a friend. We took suitcases of notes to the house of another friend and copies to still another. Then we shuttered the windows and listened to the radio. That night it looked as though the crisis was over, and we celebrated the assignment of ministerial posts assigned to labor leaders, some of whom we knew.

The Workers' Central of the Department of Oruro had planned a strike and an anti-fascist demonstration in Oruro on October 7, with participation by miners from other centers as well as San José. From the window of a dentist's office that morning, I watched a crowd of young men converging on the offices of the DIC. The guards ran up to the roof with machine guns, but they did not fire on the crowd below, whether because of cowardice or good judgment I do not know. The youths came out carrying rifles. (We later discovered that the guns had no firing pins, but they looked menacing at the time.) Not far away, students were assaulting the U.S. Information Service building, where the doors of the library were bombed open and books and materials carried off or burned.

When I returned home, our cook told me that she had just bought a can of oil in the market when one of her neighbors pointed a rifle at her and tried to seize it. He did not recognize her until she screamed, "Don Roberto, what are you doing?" and then he let go of the can. Shortly after, one of the university students came with news of the assault on the

USIS. He brought tear gas bombs and a vomit bomb, plainly marked US Army, that he had liberated from the DIC office and gave them to us in case the Anglo-American school, as the only symbol of US imperialism left, should be the next target. I was beginning to get irritated at some of the "revolutionary" tactics of the mob, and I didn't want to leave without at least some show of resistance. I felt that, while I had to demonstrate that not all Americans were imperialists, I also had to make them realize that not all actions in the name of revolution were revolutionary.

There was nothing to do but sit in the shuttered living room listening to the transistor radio, with the bombs close at hand. Radio Universidad announced the arrival of truckloads of miners from Machacamarca. Others from Catavi and Siglo XX were expected to join them for the afternoon demonstration. Suddenly a volley of shots was fired into the unarmed crowd in front of the central barracks. Whether it was triggered when civilians approached the door of the barracks, as some said or when a woman, hit by an orange peel thrown at the guards, screamed, is not verified. For the next eight hours there ensued a useless battle with random sniping, resulting in about 20 deaths and 100 injuries. Between desperate calls for a return to sanity radio announcers broadcast the lists of wounded and dead.

The teenage son of a miner came to visit us. He wanted to go out in the streets to see what was happening, but I made him help me bake a cake. When it was done, I wanted to invite Doris, who lived across the street, to join us for tea, but the moment we opened the door a volley of bullets from a sniper sitting on a nearby hill discouraged us. I made the teenager stay overnight for fear that he would be shot going home to the mine. In the morning his parents arrived looking for him. As I had feared they had thought he was in the hospital or the morgue and had made the rounds early in the morning and seen the dead and suffering. However, they agreed that the precaution had been worth their night of anxiety. In the afternoon of the following day, we went to the wake of eight students and youths in the university auditorium and then to the mine office, where three workers who had died were on view. Thousands came to pay their last respects and then attended the mass funeral on the following day. The speakers tried to make of the deaths a noble sacrifice for the revolution, but the people knew that they were due to nothing more than the stupidity and ineptness of the command of the armed forces.

In the following days people waited and watched the new officials. The DIC office was still occupied by students, who had put a likeness of Che Guevara in guerrilla fatigues on the pillar in the entryway. They soon acquired a reputation of being as arbitrary in their handling of cases as their predecessors. One of the miners told us that a fight had broken out in his family and everyone was detained in the cold cells without cots or plumbing facilities, even his 15-year-old daughter (who, as a minor, should not have been imprisoned). In a country that lives with the constant expectation of revolution, there is little preparation for a successful outcome. During this period, there was relative freedom of expression in the press and on the radio. Union leadership was given full liberty to pursue the plans for the restitution of wages. There was no evidence of restraint or recrimination against any of the combatants.

A week after my accusation, I went to ask the secretary-general for time to explain my work to the delegates. He agreed to give me some time in the next meeting. In my presentation, I stressed that the tape recorder was a tool to get more accurate data and not an instrument of espionage. One of the delegates told me that the suspicion had arisen when I had lent the tape recorder to one of the delegates to a COB conference. Though the title of the tape on the machine had been erased, the tape itself had not and it contained accusations by one of the members against some of the directors. The delegate had played the tape for the other delegates to the conference and let it be known that it was mine. I was dismayed at what I had let happen after all my care, and assured them it was an error of stupidity and not evil intention. They seemed prepared to believe this. Despite their hatred of the CIA, they had a very high regard for the agency's performance, and this blunder did not fit the image. I felt that they had accepted my continued stay in the community when the directorate's intellectual advised me how I could improve and amplify the study by investigating work conditions of the women on the slag pile.

The episode, although disturbing and threatening the very possibility of continuing my work, yielded some ethnographic benefits. Enemies of the leadership became friendly to me when I was cast as an enemy of their enemy. They told me of the leaders' attempts to gain favor with the administration and described the circle of *llunkus*, men who curry favor with those in power, that surrounded each of them. They spoke more frankly of their own fear of being deceived by their leaders, the ever-present fear of the powerless. I learned of the corrupt union leader's technique of taking issues from the management, and introducing them as union demands, such as constructing additions to social service buildings (projects that meant a lot of graft), I discovered that I had been under almost constant surveillance by a neighbor whose husband worked in the mine, and that her report of my visitors' being only workers from the rank and file reassured the people that my interest in the mine stemmed from genuine sympathy. In the week of my own "suspension" I came to understand more fully the insecurity that robs working people of their revolutionary zeal.

Several days after my meeting with the union delegates, I saw the jeep station wagon put at the disposal of the union by the company raising a cloud of dust behind me as I walked down the road. Still not sure how they had taken my defense, I jumped into the ditch to avoid being run down; much to my surprise, the driver pulled up to me and the secretary-general asked me if I would like a ride. I was too overcome to think of an excuse, so I agreed, but I recovered my defense enough to decline a drink of chicha (the fermented drink of the workers). At that point I was afraid of being seen with him and his *llunkus*. The next time I showed up at a union meeting, two of my compadres came over and greeted me, using the formal address of *comadre*, thus establishing the relationship publicly. Later, when a newcomer who was trying to gain footing in the union tried to intercept my taking a photograph of two children listening in to the discussion at a meeting, two other friends came over and asked him what he thought he was doing. After the meeting a man invited me to be comadre of the soccer team. I could hardly refuse, although it meant buying socks for the 13 members.

Torres visited the San José mines in December, just after the announcement that the wage raises would go into effect in January. The crowd of 200 or so miners who came to the stadium to listen to him applauded his speech but saved most of their *vivas* for the working class and the martyrs of the union struggles. The president called for confidence in his government and the good will of the armed forces, and he pleaded for peace to work out a program for economic improvement. "You have fought enough," he told the miners, "in the war of Nancahuasu and when Maria Barzola marched and sacrificed her life asking for more bread for the workers"; the miners responded with a call for arms for the workers. Miners have learned that the words of presidents have little value unless they have confidence enough in the workers to give them weapons.

I left in December feeling somewhat optimistic about the future of the mines. When I returned in July, 1971, to make a film based on the miner's autobiography mentioned earlier, there was a new secretary-general, formerly a leader in Siglo XX who had been jailed during Barrientos's term of office. When he had been elected in December, one of the superintendents expressed relief that he had won over the more Marxist-revolutionary candidates, since he had a good reputation as a foreman concerned with production. He had already aroused some criticism from the rank and file for getting employment in the mines for members of his extended kin group. Furthermore, newspaper articles implicated him in the torture and slaying of a union leader during Ovando's period of office. The last thing he needed was a *gringa* working in the mines. He agreed to let me show the Super-8 film we had made the year before. I had looked forward to this as an opportunity of telling people in the mining community about the 16-mm film we proposed to do. When I arrived at the union hall, the order to permit the showing of the film had not been given. I sensed that I was going to have some opposition from official union sources in making the film and continuing my work, although my friends and compadres were cooperative throughout.

There had been a marked shift to the left in the Torres government. Two or more attempted coups in January (one by Hugo Banzer, who was to carry out the successful coup in August) had been put down with the help of miners. The right was on the defensive or in flight. The Popular Assembly, a kind of forum of union leaders, *campesinos*, and left politicians, opened in June. Ideologies and programs of the left were aired in what *Presencia* (August 6, 1971) referred to lyrically as a "symphony for the revolution." The main business of the Assembly became the working out of the details of coparticipation of workers in the administration of mines and factories. Workers in the mining center were doubtful about the program, since they felt that in the coparticipation phase of the MNR regime the union leadership had lost its revolutionary aims as it learned to participate in the spoils of the company. At an August meeting in San José mine, the director was unable to secure a quorum, and those present began to whistle and protest that the leaders had not come to advise them about the plan for coparticipation. The director turned to me and ordered me to go, thereby diverting the workers from their protest and eliminating a witness of the breakup of the meeting for lack of a quorum. After the meeting, rank-and-file members told me resentfully that they were again being used as "steps" in the rise of opportunistic labor leaders.

During July and August there were invasions of agricultural and business enterprises. Miners seized private holdings in Colquecharca, Postosi, and Catavi. Peasants seized the homes of the *hacendados* (estate owners) for whom they worked. Some of the presses and radio stations were taken over by the men who worked in them or by popular pressure groups and were turned into cooperatives. There was an air of apprehensiveness and expectation; people stood in their doorways, watching to see the next development, just as they did in the week of Carnival waiting for the dancers to come in. When I went to the University of Oruro's library on the morning of August 7, pensioned miners who had been on a hunger strike to gain their subsistence money surged into the building, which had once been the residence of a tin baron. As the men and women pressed into the main hall of the baroque mansion, shorter than I by a head, stunted by years of malnutrition and shaken with the racking cough of silicosis, I felt the full impact of the revolutionary pressures in an economy hedged in by foreign powers that did not have to yield to their demands.

In the early days of August, rumors of military plots originating in Santa Cruz led to the demand for arms for the people to "defend their revolution and take positive steps toward socialism" (*Presencia*, August 8, 1971). On August 14, union directors in Santa Cruz advised Torres of subversion in their capital. Torres failed to act, but the military began to prepare themselves; on August 16, students in the military college were assigned to the central barracks without any official explanation. The union leaders of Santa Cruz sent more urgent messages, and both union leaders and the Popular Assembly in Cochabamba asked for arms for the workers. On August 20, Hugo Banzer and 38 coconspirators were imprisoned. After a day of demonstrations in favor of his release, Banzer was set free and began to mobilize rebel forces.

In Oruro, miners were mobilizing from Siglo XX, Catavi, Huanuni, and Santa Fe to fight the rebels. On the morning of the 10th, union leaders of San José called for a work stoppage and a united demonstration of miners from the other centers. The call was broadcast on the union radio until 11 A.M. At 11:20 A.M. 14 truckloads of miners arrived in Oruro. The union leaders were no longer to be found. The mayor and government leaders had left their posts. At 12:45 P.M., the military guard of Oruro yielded to the insurgents. The demonstration was called off. On the following day, reinforcements arrived from Santa Cruz by air. Radio Pio XII, the Oblate mission station in Siglo XX, called for a withdrawal of miners still in the area to prevent bloodshed. At midnight the radio broadcast a speech, said to be by Victor Paz Estenssoro (whose MNR party, along with the Social Phalanx of Bolivia [FSB], was behind the rebellion), calling for their dispersal. (Those familiar with his voice say that it was difficult to recognize it because of the poor transcription.) Some of the miners left the city in response to his plea.

The miners were still under contradictory orders from their leaders on the following day.

Over 1500 miners from Siglo XX and Huanuni, resolved to take the Oruro airport and hold it against the insurgents, were repelled by heavily armed forces, and 8 were killed. According to 2 miners taken prisoner, their leaders had told them that they would be joined by military forces under the command of President Torres (*Presencia*, August 23, 1971). The prisoners reported that the directors of the union escaped in some of the vehicles, leaving the dead and wounded without help (*Patria*, September 22, 1971).

The 280-day presidency of Juan José Torres ended after 3 days of fighting in the capital cities of Santa Cruz, Oruro, Cochabamba, and La Paz departments. Torres had given the country over nine months of freedom — freedom for workers to reorganize the unions, for students to march in protest against imperialism, and for politicians of the right as well as of the left to formulate positions and seek alliances. For some, this freedom meant only anarchy, but in a country that had lived in a state of dependency and subjugation to outside economic and political interests, it was a time to assess who Bolivians were as a people and where they were going.

Throughout his term, there had been persistent rumors of intervention by the United States. That the US Embassy knew that a coup was about to take place is established by the warning to stock up on food supplies 48 hours before the coup in La Paz, reported by Cuban correspondent Ernesto Gonzales Bermejo. Reports indicate that Torres had himself reserved 60 places for exiles in the Chilean Embassy. The reported association of Major Robert J. Lundin with Banzer prior to and during the coup (*Washington Post* on August 29, 1971) has been denied as having had a serious impact on the movement of the rebels in Santa Cruz by General Remberto Iriarte (*Presencia*, August 31, 1971), but those who were in Bolivia during the coup attest to the importance of a network of radio communication linking the activities of rebels and demoralized government armed forces.

Despite the US denial of military involvement, officials did not conceal their satisfaction at the success of the coup (*Presencia*, August 30, 1971). The financial support immediately offered the Banzer government indicated to Bolivians which side the United States was on. On August 28, *Presencia* reported a US loan of $2,500,000 for cotton agriculture. On September 7, *Presencia* announced in large headlines US offers of $100,000,000 credit with $3,000,000 earmarked for construction of three new markets. The Bank of America announced a $12,000,000 loan to the Nationalized Mining Corporation of Bolivia September 11, 1971. Victor Siracusa, US Ambassador, promised special financing to Bolivia that would offset any problems with the proposed law restricting imports (*Presencia*, September 14, 1971). Brazil and Argentina added to US promises loans totaling $10,000,000.

The new government is relying on military strength to hold on to what it has gained. The university and mines were occupied by troops until September. Recently armed tanks were delivered to Oruro, where the barracks are situated right next to the mining encampment. The two parties that backed the rebellion, the Social Phalanx of Bolivia and the National Revolutionary Movement, still maintain an uncertain alliance, but the old left-of-center supporters of the latter have urged Paz Estenssoro to denounce the government of Banzer for its treatment of students during the raid on the University of San Andres.

Reading the newspaper reports of the aftermath of the coup, I felt that I was back in 1954 in Guatemala, when Castillo Armas entered the country with 200 rebel troops equipped by the United Fruit Company and backed by promises of support from the US government. I was living in an Indian town in the western highlands and saw trucks loading campesinos with nothing more than machetes to "defend their revolution" the night that Jack Purifoy, special representative of the US government, maneuvered the ouster of President Arbenz. The coup also has a parallel in the US invasion of the Dominican Republic, when President Johnson sent in Marines to take back control from a government considered too far left.

Recalling the consequences of our intervention in these countries, I began to reflect on the role we social scientists are called on to play. Do anthropologists go to these countries just to write epitaphs for the movements that are cut down when they go beyond the limits the US government sets for them? Those of us who are concerned with the welfare of the people we study must reveal what we know about the US involvement and what it means to them. In the months I worked in Oruro, I came to realize the CIA symbolized the American presence in Bolivia. Their agents act in secrecy and are protected by the State Department, while we, as American citizens, must bear the burden of guilt for their actions. It seems a corollary of this that we must dedicate ourselves to eradicating their influence on our government's policy.

The role of the participant-observer in a revolutionary setting has a special dynamic. Just by being there, threatening the existing role-structure and hovering in the conflict of identity, I became an instrument in the research. The attacks directed against me gave information of the inner conflicts of a people who had suffered "in their very flesh" the presence of the United States and the abhorred CIA agents. In evaluating my own role in the community, I realized that the CIA agent is not entirely different from the witch in the Maya community. The difference was that, while I despised the activities of the CIA of my own country, I felt neutral about witches in another country, and while I became one of the targets of accusation in Bolivia my cultural distance protected me from becoming part of the witch hunt in the Maya community. This breakdown of my carefully cultivated "cultural relativist" position forced me to realize that it was premised on a colonialist attitude. I did not judge the witchcraft institutions because I felt removed from and impervious to them. In Bolivia I was no longer able to maintain this pose, because the CIA agents and I were part of the same historical continuum. I realized more fully the implications of Maquet's (1964) rejection of the scientific attitude of impersonal objectivity as inappropriate for the kind of research in which we by our very presences are instruments of that research. The world is no longer our laboratory, as Berreman has remarked (1971:100), but a community in which we are coparticipants with our informants.

Anthropologists are now at the crossroads in defining a participation-observer perspective more adequate to the load that revolutionary stress is putting on their role in the field. We must begin to specify the "degree of indeterminancy" (Heisenberg, quoted in Mannheim, 1936) arising from our own perspective. We can no longer retreat into the deceptive pose of neutrality (Henry, 1966). Science advances only by honest declaration of the convictions that influence our data gathering and analysis. It is a paradox that the physical sciences cast aside the pose of neutrality decades before the social sciences, with their presumably greater humanitarian orientation. In Bolivia I became convinced that part of our professional task as anthropologists is to attack the multifarious ways in which the US State Department operates to destroy the independence movements of the countries that supply it with raw materials. Lévi-Strauss (1969:52) announced prematurely that "our science arrived at maturity the day that Western man began to see that he would never understand himself as long as there was a single race or people on the surface of the earth that he treated as an object." We have yet to reach the goal he envisioned of becoming "an enterprise reviewing and atoning for the Renaissance, in order to spread humanism to all humanity".

# 16

# Human Hazards of Fieldwork

## *Nancy Howell*

If we were thinking of the hazards of humans as disease and accident vectors, many of the hazards experienced in the field could be put in this section. Venereal disease, for instance, is a "contact disease" of humans, but we will classify it as an infectious disease and confine our attention here to the hazards of human hostility and conflict.

More than other hazards, interpersonal assault and threat are touchy subjects for anthropologists, calling into question the relationship that the fieldworker is supposed to establish with colleagues and with the subjects of his or her study, as a matter of professional competence. Nevertheless, these failures do happen, and whether the anthropologist is in some sense to blame or is blameless, many anthropologists suffer interpersonal attacks during the course of their fieldwork.

Some anthropologists, when reporting these kinds of problems during fieldwork, are quick to point out that the same kinds of problems of criminal attack might occur in the home society – and, indeed, some are sure that the field is much safer than home in these respects, even if there is still some danger.

We start by looking at the frequency of the kinds of events that we consider "criminal": theft, fighting, assault, rape, and murder. Table 1 gives the overall table of numbers reporting one or more interpersonal problems, to self and to other members of the research group. Note that the percentages given are cumulative, so that the percent under "others" includes those that reported the hazard for self. And the categories are nested, so that if an event occurred to oneself and to others in the group it is coded as happening to self. Political problems, stemming from governmental or political movements such as arrest and military threat are not included in Table 1 and will be considered later in this chapter.

We note that the majority of those who work in Africa report one or more kind of interpersonal hazard, and that those who work in North America have the best record of avoiding these problems.

We also note that most of those who report any such problem report it as having occurred to themselves rather than to others of the group. Generally speaking, common events show this pattern of being reported primarily for self, while rare events are more often reported for others than for self. Criminal hazards seem to be highly variable by area. Where they are common, they are reported primarily for self, and in other areas they are not reported for either self or others in the group. A similar pattern would be expected if some

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*Table 1* Criminal Interpersonal Hazards, Combined Rate

| | *N. America* | *Europe* | *L. America* | *India* | *Africa* | *Pacific* | *Total* |
|---|---|---|---|---|---|---|---|
| Total | 61 | 17 | 63 | 17 | 23 | 23 | 204 |
| Had condition, self | 19 | 6 | 25 | 8 | 12 | 9 | 79 |
| | *31%* | *35%* | *40%* | *47%* | *52%* | *39%* | *39%* |
| Others in group had it | 1 | 0 | 4 | 0 | 1 | 0 | 6 |
| | *33%* | *35%* | *46%* | *47%* | *56%* | *39%* | *42%* |

*Table 2* Robbery (including theft)

| | *N. America* | *Europe* | *L. America* | *India* | *Africa* | *Pacific* | *Total* |
|---|---|---|---|---|---|---|---|
| Total | 61 | 17 | 63 | 17 | 23 | 23 | 204 |
| Had condition, self | 5 | 2 | 13 | 4 | 4 | 7 | 35 |
| | *8%* | *12%* | *21%* | *24%* | *17%* | *41%* | *17%* |
| Others in group had it | 3 | 0 | 7 | 0 | 3 | 0 | 13 |
| | *13%* | *12%* | *32%* | *24%* | *30%* | *41%* | *24%* |

respondents reported while others denied interpersonal hazards.

About one third of those who work in Latin America, Africa, and the Pacific report having been robbed during fieldwork (Table 2). In South Asia, the rate is about one quarter. In North America, despite the American reputation for lawlessness and incivility, only about 13% report having been robbed in the field. Overall, about one in four report one or more robberies during fieldwork, and some have been robbed many times. Countries with particularly bad reputations for robbery are Kenya, Peru, Colombia, and New Guinea. Maintaining the distinction between criminal and political acts is often difficult in these parts of the world. Many of the robberies reported occurred in national capitals or other big cities on the way to fieldwork but were not directly a part of the fieldwork experience.

During traveling to and from the field, one is at risk of robbery because one is burdened with carrying luggage while being disoriented, tired, and unsettled. Pickpockets may lift a wallet, and luggage, cameras, and backpacks are frequently stolen in transit, in airports, while getting a taxi, checking in to a hotel, and so on. Shoulder bags are especially common targets in some parts of the world, where the experience of having one's bag grabbed by someone on a passing motorbike is frequently reported. The difficulty of replacing a stolen passport or travelers' checks is both common and disheartening.

Hotel rooms are another frequent site of theft for travelers, by hotel employees or by outside thieves who might be ex-employees, ex-residents in the hotel, or just ordinary thieves. Some travelers bring along a wedge-shaped doorstop for use in hotel rooms, which will not eliminate the danger of theft but at least will prevent break-ins while one is in the room. Some people leave passports, tickets, and travelers' checks in the hotel safe while visiting a dangerous area. (Good advice on managing the risks of theft is given in Hatt 1985.)

Circumstances of robbery are likely to vary by areas of the world. In some places, anyone walking around who does not look like a local is at high risk, whereas in other areas it is the knowledge of the tempting money and goods that might be taken that seems to pose the greatest risk.

A matter of great concern under such circumstances is the keeping of guns. Some researchers want to have a gun in order to prevent or retaliate against robbery, but the knowledge that there is a gun in camp may enormously increase the probability of being robbed, as a gun is a very valued possession in many areas. Knowledge that one keeps a gun may also lead robbers to come armed with guns, increasing the danger to everyone.

One may also be robbed by informants or employees. Many anthropologists have reported that they have lost valuable possessions in the field, but few are willing to describe the incidents "on the record." It is easier to talk about thugs in the capital or bandits who sweep down into the village, but hard to talk about breakdowns of reciprocity at the field site, especially for social-cultural anthropologists. For all robberies combined, 20% of archaeologists report robbery, while 27% of social-cultural anthropologists report the same thing.

Many examples can be cited. Napoleon Chagnon (1974) tells of a number of instances where temporary employees or village bullies stole trade goods, food, or research equipment from him, some of which were later recovered. When Louis and Mary Leakey were newly married, they were working at Hyrax Hill in Kenya. Local bandits broke in and stole everything from their tents. In response they got a dog, and have kept dogs with them in the field ever after (Leakey 1984:71). Dian Fossey was robbed of a case containing her money, checkbook, passport, and car papers (Cole 1975:345).

In Nairobi, Kristen Hawkes and Jim O'Connell (University of Utah) had their entire truckload of supplies for an expedition to the Hadza stolen. Not only camping gear, but notes, cameras, and research supplies were taken, leaving only a case of bully beef. This delayed the research considerably and almost threatened to end it, since the materials were essential and they could not easily replace them. Again in Nairobi, Hawkes and O'Connell were robbed by four men on foot, while they were walking from the hotel to dinner. Her jewelry and money were taken, and his watch, wallet, and pocket knife. A passerby in a car saw the robbery going on and stopped and rescued them, but the robbers were not caught. This crime was reported to the local police and embassy.

Mark Owens (Owens and Owens 1984:107), who worked with his wife in the Kalahari, reports:

> Later, when we unloaded in camp, we found that our three months' supply of flour and sugar had been stolen from the truck, along with some other grocery items. We were furious. On our limited budget, there was no going to Maun until our next regularly scheduled trip, so for three months we were without bread, an important part of our diet. Since most of the door and window locks on the old Land Rover were broken, there seemed to be no defense against being robbed, other than for one of us to watch our goods every minute while in the village.

Owens also reports (1984:107) that he protected his goods in the truck after that disastrous theft by killing a couple of large and feared mamba snakes and draping their bodies over the fresh supplies in the back of the Land Rover. He says that even after the locals realized that the snakes were dead, no one touched the goods, probably because the bizarre sight of them suggested witchcraft to locals.

## Physical Violence

The three categories – assault, fighting, and beating – are similar in that they all refer to interpersonal struggles in which physical injuries were inflicted, but they seem to differ in the locus of the responsibility being assigned. Note that fighting and beating are more frequently reported for others in the research group, while assault is much more commonly reported for self. In may be that "assault" is a word we use to describe circumstances where the victim is held to be innocent of blame, whereas "fighting" implies more aggression, and being beaten implies defeat. In interviews, it has emerged that the "others" referred to in these responses are typically male graduate students and junior colleagues.

There are many known instances of assault to anthropologists, and they are found in all parts of the world. In Papua New Guinea, Robert Welsch, then a student at the University of Washington and now at the Field Museum in Chicago, was assaulted and stabbed in a robbery attempt while staying in a university residence for researchers in Port Moresby. There had been an outbreak of robberies around the university in 1980. Welsch was sleeping in his room one hot night, when seven of these "rascals" broke into the housing

*Table 3* Assault and Physical Violence

| | *N. America* | *Europe* | *L. America* | *India* | *Africa* | *Pacific* | *Total* |
|---|---|---|---|---|---|---|---|
| Total | 61 | 17 | 63 | 17 | 23 | 23 | 204 |
| Assault | | | | | | | |
| Had condition, self | 5 | 2 | 4 | 1 | 4 | 4 | 20 |
| | *8%* | *12%* | *6%* | *6%* | *17%* | *17%* | *10%* |
| Others in group had it | 0 | 1 | 6 | 0 | 0 | 0 | 7 |
| | *8%* | *18%* | *16%* | *6%* | *17%* | *17%* | *13%* |
| Fighting | | | | | | | |
| Had condition, self | 1 | 3 | 0 | 1 | 1 | 1 | 7 |
| | *2%* | *18%* | *0%* | *6%* | *4%* | *4%* | *3%* |
| Others in group had it | 4 | 1 | 6 | 0 | 3 | 0 | 14 |
| | *8%* | *24%* | *10%* | *6%* | *17%* | *4%* | *10%* |
| Beating | | | | | | | |
| Had condition, self | 0 | 1 | 0 | 1 | 1 | 0 | 3 |
| | *0%* | *6%* | *0%* | *6%* | *4%* | *0%* | *1%* |
| Others in group had it | 3 | 0 | 3 | 0 | 0 | 1 | 7 |
| | *5%* | *6%* | *5%* | *6%* | *4%* | *4%* | *5%* |

unit. Three entered his room, armed respectively with a baseball bat, an ax, and a long kitchen knife. Welsch resisted and was stabbed between the ribs in the struggle. They fled without taking anything of his, although others in the residence were robbed that night. He was taken to the hospital by colleagues, underwent surgery, and recuperated in the hospital 12 days, and then in the home of Mac Marshall. Welsch partially blames himself for the attack, for sleeping with his door open on a hot night. Afterward, the university installed a simple whistle in each room as a low-tech alternative to burglary alarms.

Lorraine Sexton reports that one time she was driving with Mac Marshall and his wife and child near her field site in Papua New Guinea when they were approached by ax-wielding "highwaymen' attempting to rob them. Marshall adds to her account that when he realized the threat "I sped up and aimed right for the guy with the ax. When he finally realized that I would hit him, he flung the ax to one side and ran, looking very scared."

Charlotte Ikels, Case Western Reserve, writes that when she was interviewing in Hong Kong she was robbed at knifepoint in the stairwell of a building.

Napoleon Chagnon (1974:4) tells of many instances among the Yanomamö of threats of violence and occasional hits or planned attacks, especially when the locals had been taking hallucinogenic drugs. On one occasion, Chagnon tells of one of the village men, under the influence of drugs, coming toward him with an arrow aimed directly at his chest. Chagnon believed that the purpose of the behavior was to make the anthropologist turn and run, exposing his rear as a target, and he decided that he would stare the aggressor down, despite the dangers of provocation. As it happened, this dangerous gamble was rewarded with success.

A recent encounter with violence occurred in Philadelphia, in April 1988, when an employee attacked four archaeologists with a knife. According to the *New York Times* of April 12, 1988, Glynn W. Sheehan, the director of a salvage archaeology project on the Schuylkill River, and his wife, Anne Jensen, were attacked when they attempted to come to the rescue of two women being raped by the employee, Arthur Faulkner, who was arrested in New York City the next day. Sheehan and Jensen were stabbed, seriously injured, and were apparently left for dead. Sheehan crawled for help to the nearest house and got an ambulance and the police.

## Rape and Attempted Rape

Before Sheehan and Jensen arrived, the employee, Faulkner, had cornered Clarice J.

*Table 4* Rape and Attempted Rape

| | *N. America* | *Europe* | *L. America* | *India* | *Africa* | *Pacific* | *Total* |
|---|---|---|---|---|---|---|---|
| Total | 61 | 17 | 63 | 17 | 23 | 23 | 204 |
| Had condition, self | 0 | 0 | 2 | 0 | 1 | 2 | 4 |
| | *0%* | *0%* | *3%* | *0%* | *4%* | *9%* | *2%* |
| Others in group had it | 1 | 1 | 5 | 0 | 1 | 0 | 8 |
| | *2%* | *6%* | *11%* | *0%* | *8%* | *9%* | *6%* |

Dorner, of Elk Grove, Illinois, in the barn that was used for the headquarters of the archaeological excavation on the banks of the Schuylkill River outside Philadelphia. He was in the process of raping her when another archaeologist, Annaliese H. Killoran, of Lynn, Massachusetts, arrived and tried to stop the attack. According to the account in the *Times*, he raped and killed both women.

Rapes occur among many occupational groups, and in many parts of the world. The special risk factors in this case seem to have been the relatively isolated location of the site and the need for manual labor rather than any features of the archaeology itself. Generally speaking, rape seems to be a special danger to women who are unknown to the local power structure, which protects most women most of the time. Women anthropologists may be outside protective networks, even if only temporarily, and may be unaware of the signals of challenge and deference with which local women protect themselves.

Rape, including attempted rape, is a difficult topic to study, since many women do not want to discuss the topic. Table 4 shows that rape was reported by 2% of all fieldworkers, 7% of the women in the study. Some of them are motivated by the fear that women who have been raped lose status and respect and are seen as pawns in the male game of aggression and competition. Other women are more concerned about issues of employment and research opportunities that could be denied to them on the excuse (or perhaps the true motive) of wanting to protect women from the risk of rape in the field. All in all, it is a touchy subject, and one that I am quite sure is underreported in this sample.

In all, four anthropologists in the sample reported that they personally had been raped in the field, but two of these are women reporting an attack on themselves and two are men reporting an attack on their wives. Eight researchers, in addition, reported that someone in their study group other than self or spouse was raped (or experienced an attempted rape) during fieldwork in which the respondent had participated. Overall, about 6% reported a problem with rape in their fieldwork.

While the data are uncertain, it seems to be the case that the threat of rape is highly localized: in some areas, at some periods of time, there is little or no risk, while in other areas it is hard to avoid. North Africa, localized parts of East Africa, New Guinea, and parts of Latin America (Peru stands out in the accounts) have been cited as particularly dangerous, while Europe seems to be relatively safe. Nader (in Golde 1986:111) states that threats to women in the Middle East have been highly exaggerated, and cites customs of protection of certain categories of women ("sisters of men") that should make foreign women safe. Other informants, however, disagreed that fieldwork was safe for women working alone in Muslim areas. Generally speaking, knowing the area and knowing people in the area seem to be factors in safety, whereas being alone, tired, and disoriented seem to be factors in danger.

## Murder, Suicide, and Other Mysterious Deaths

Murder attempts against themselves were reported by five anthropologists in the sample (two Latin Americanists, two Asianists, and one North Americanist), and another four reported a murder attempt against a member of the research group (two in Latin America, two in Europe). One researcher reported a suicide attempt by a member of the research group in India. Another in the sample was arrested and tried for the murder of a member of his research group.

A famous old case of an anthropologist killed by the people he went to study is that of the death of William Jones, who was at the Field Museum in Chicago until he was killed by the Ilongot of Northern Luzon in the Philippines around 1908. There have been other cases – in Latin America, New Guinea, and Africa – and there have been cases where anthropologists simply disappear and no one ever knew what happened to them.

Another well-known murder case in the history of anthropology is the death of Henrietta Schmerler, a graduate student at Columbia who died during fieldwork at the Fort Apache Reservation in Arizona in 1931. Schmerler, a student of Ruth Benedict, was described by Morris Opler, who was a member of the research group, in the following way:

> According to Goodwin, Miss Schmerler, who greatly admired Margaret Mead, was determined to duplicate her South Seas work in the Apache context and especially to gather material about Apache sex life. This is a subject about which Apache elders do not speak easily to virtual strangers, and they refused to cooperate. The youth who slew her interpreted her emphasis on sex in her research as a sign of looseness and invited her to ride behind him on his horse, something that young people of opposite sex among the Apache do not do unless they are courting. Miss Schmerler, unaware of this, accepted. When he made advances and was rebuffed, the young man was angered at what he perceived as enticement and then rejection; the struggle, assault and death followed. (Opler 1987:3)

The Schmerler case is almost a litmus test of whether anthropologists assume that they have to take the blame for everything that goes wrong in the field, or whether others are seen as independent agents who might be held responsible. The possibility that women might not be permitted to do fieldwork because of their susceptibility to rape and murder came close to the surface in this situation. Franz Boas wrote to Ruth Benedict when he heard the news:

> I cannot tell you how shocked and also worried I am by the fate of Henrietta. I am trying to imagine what may have happened and cannot conceive of anything that should have induced nowadays an Indian to murder a visitor. [In another letter after receiving a detailed report on the events:] It is dreadful. How shall we now dare to send a young girl out after this? And still. Is it not necessary and right? (Mead 1959:408–10)

It is still true that women hesitate to speak out about rape or threats of sexual assault for fear that their freedom of action will be restricted.

No doubt the most famous case in recent years of an anthropologist killed in the course of her work is the death of Dian Fossey, hacked to death with a *panga* knife in her research hut in Rwanda, during the night of December 27, 1985. Fossey had been working with the mountain gorillas of that area for some 15 years, and had been in frequent conflict with local residents and local officials who resented her high-handed ways. No one knows who broke into her cabin and killed her, as was shown in the recent film of Fossey's life (*Gorillas in the Mist*), but Mowat's book (1987) entertains several hypotheses. Local authorities charged one of the graduate students and one of her long-time local employees. These two people were convicted of the murder, *in absentia*, and the case is considered closed by the Rwandan government, although primatologists familiar with the case are convinced that neither was guilty.

Not all murder attempts are successful. Napoleon Chagnon (1974: 178–80) tells of being in a hostile village among the Yanomamö with a severe allergic reaction. Throughout the night, as he tossed and turned in itching and discomfort, he shone his flashlight around the village to orient himself whenever he woke up. Later he learned that the village leader and his two brothers, carrying axes, had crept up close to his hammock intending to crush his skull while he slept, but just then he had shined the light on them, and they had stopped in fear of his knowledge and his powerful gun. Hearing this story, Chagnon decided to avoid work with this group for the foreseeable future.

In some cases of mysterious death in the field, how the death occurred is not known, and perhaps never will be known.

A German anthropologist, Harold Herzog, from the Max Planck Institute near Munich, was found shot with his own gun in the jungle of Venezuela where he was doing fieldwork with the Yanomamö. Despite the reputation of the Yanomamö as the "fierce people" there is reason to believe that his death was due to accident or suicide rather than murder, although his entry to his daily diary gave no hint of despair that day. The Yanomamö reported the death to local authorities and said that they had just found the body near the gun (Polly Weissner, personal communication, 1986).

In the early 1980s, Melanie Fuller, a graduate student in the biology department at the University of Chicago, and a student of Stuart Altmann, was in East Africa attempting to get started on dissertation research when she ran into difficulties. She broke off her agreement with her supervisor and spoke of establishing another topic with another supervisor. Then she was not heard of for some weeks. Her parents got in touch with local authorities in Kenya and investigators found her body in the bush, and identified it by dental records. The cause of her death is unknown; her money and documents had not been taken.

## Political Hazards

It is often difficult to distinguish during fieldwork between the kinds of threats that stem from criminal impulses on the part of others, and those that arise from political motives and circumstances. What feels like irrational rejection by a segment of the village may follow ancient cleavages of the local power structure, and the arrival of hostile army officers with questions and demands to inspect papers may arise from racial hatred, a desire for bribes, the presence of the organized guerilla movement aiming to overthrow the government, or the suspicion that the researcher has broken a law of the land.

We tend to assume that our colleagues are innocent of offenses, but we can probably all think of instances where a colleague broke local laws.

Don Johanson tells (Johanson and Edey 1981:158–9) of needing a human knee joint for comparison with a fossil when they found important new *Australopithicine* bones in Ethiopia. In the excitement of an important find, he compelled a reluctant graduate student to help him take a bone from a modern Afar burial mound, although he had been warned that any approach to the modern burials would be considered an outrage to local feelings. Some researchers feel that consequences of that act have had negative implications for all foreign research in Ethiopia.

Whether or not the individual was guilty of law breaking, about 5% of our sample of fieldworkers report having been arrested in the field, and about 9% experienced an arrest in their group (Table 5). Some of the charges were minor, but one was the arrest of the group leader on murder charges, and two were arrests on drug smuggling charges in South America.

## Military Attack

Like arrest, involvement with the military in other countries can be a frightening event. The hazard was described as "military attack," but apparently respondents included harassment

*Table 5* Arrests in the Field

| | *N. America* | *Europe* | *L. America* | *India* | *Africa* | *Pacific* | *Total* |
|---|---|---|---|---|---|---|---|
| Total | 61 | 17 | 63 | 17 | 23 | 23 | 204 |
| Had condition, self | 1 | 0 | 6 | 1 | 2 | 2 | 11 |
| | 2% | *0%* | *10%* | 6% | *8%* | *8%* | *5%* |
| Others in group had it | 3 | 0 | 3 | 1 | 0 | 1 | 8 |
| | 7% | *0%* | *14%* | *12%* | *8%* | *13%* | 9% |

and questioning by the military, not just armed attack. Table 6 shows the experiences reported with the military.

In areas of guerilla warfare, anthropologists may be suspected by the military of helping or supplying the rebels. The rebels may see anthropologists as allies of the government or as competitors for resources or people. In Peru, guerillas have been known to retaliate against villagers who sold food to an expedition. We recall that 31% of our informants reported having been unable to work somewhere when they wished to because of political instability.

Another difficulty that anthropologists sometimes have in the field is being suspected of spying. Bruce Schroeder of the University of Toronto tells of being arrested by the Syrian Border Patrol while surveying in the Anti-Lebanon Mountains for sites in Lebanon in 1972, apparently due to suspicion of his motives for working so close to the border. His group was taken across the border to several civil and military authorities in small villages until they reached Damascus. They were released at the Lebanese border within a day, but they then faced the difficult task of explaining to the Lebanese border authorities why they had no Lebanese exit or Syrian entry stamps in their passports. Schroeder concedes that they probably did look like spies, "wearing khaki, and encumbered with maps, binoculars and cameras." There were spies in the country, and he stresses that the Syrian authorities did not harass or mistreat them. But they spent a day "riding around Syria with armed troops, machine guns and bazookas at our backs." They eventually decided that it was not possible to work at that site until the political situation was resolved.

An indicator of the degree of trust and good will of the local population is probably found in the frequency with which investigators are accused of spying, a charge that is difficult to defend against when one is there in search of information, and the uses to which it will be put cannot easily be explained to the locals. Overall, 15% of fieldworkers report that someone in the research group was suspected of spying (though only a few of these were arrested) (Table 7).

We note that suspicion of spying is most frequent among those who work primarily in the Pacific and Asia (30%). The rate for those who work in Africa is moderate (17%), and is about 12% for those who work in Europe, Latin America, and India. Among those who work in North America, the suspicion of spying is not frequent, although it has been known (Wolff 1964:240). It is striking that suspicion of spying is much more frequently reported by social-cultural anthropologists (about 25%) than among archaeologists and

*Table 6* Military Attack

| | *N. America* | *Europe* | *L. America* | *India* | *Africa* | *Pacific* | *Total* |
|---|---|---|---|---|---|---|---|
| Total | 61 | 17 | 63 | 17 | 23 | 23 | 204 |
| Had condition, self | 1 | 1 | 2 | 0 | 0 | 0 | 4 |
| | 2% | 6% | 3% | 0% | 0% | 0% | 2% |
| Others in group had it | 0 | 0 | 2 | 1 | 1 | 1 | 5 |
| | 2% | 6% | 6% | 6% | 4% | 4% | 4% |

*Table 7* Suspicion of Spying

| | *N. America* | *Europe* | *L. America* | *India* | *Africa* | *Pacific* | *Total* |
|---|---|---|---|---|---|---|---|
| Total | 61 | 17 | 63 | 17 | 23 | 23 | 204 |
| Had condition, self | 4 | 2 | 8 | 2 | 4 | 7 | 27 |
| | 6% | 12% | 13% | 12% | 17% | 30% | 13% |
| Others in group had it | 1 | 1 | 2 | 0 | 0 | 0 | 4 |
| | 8% | 17% | 16% | 12% | 17% | 30% | 15% |

physical anthropologists (about 10%). And sometimes the suspicion is correct: after the crisis, Louis Leakey openly admitted that he had been working for the Kenyan government, as Mary Leakey put it, "broadcasting propaganda to the loyalists and gathering intelligence about Mau Mau groups and their leaders" (Leakey 1984:111). His fluency in Kikuyu and his knowledge of the people and the history of all participants in the conflict made him invaluable to the government. His love of Kenya made him willing to do whatever he could to preserve a state with room for Europeans as well as Africans.

Another kind of hazard even more frequently reported by fieldworkers is living through a period of political turmoil in the country where the work is being done. These episodes are not necessarily focused on the researchers, but it may be stressful and dangerous at times when one's presence and intentions may suddenly come into question. Examples are periods of revolution, war, or rioting. Naturally, the rate of such problems depends very much upon the countries in which one is working. June Nash (1979), for example, tells of living through a strike in a Bolivian mining community, where she was forced to, and chose to, take sides with the strikers against the government, at considerable personal and professional risk.

We note that about 30% of those who have worked primarily in India and in Africa report experiencing political turmoil during their fieldwork, as do 24% of those who have worked in Latin America (Table 8). North America, and Asia and the Pacific were places of political turmoil to about 13% of the workers, and Europe had the lowest reported rates, at only 6%. There is no striking difference in the rates for social-cultural versus archaeological fieldworkers. We notice that the hazard usually occurs to all or none in the research group, although there are a few cases where it was said to have happened to others in the group but not to self.

Often the events involve several categories of problems at the same time. For instance, Ronald Cohen (University of Florida) tells of having a vehicle accident in Nigeria, during the period of Ibo massacres in northern Nigeria in 1966. People were being killed, and he was in the process of getting his family into the country. Just then, he had the bad luck to hit a cow with his truck. What might have been a minor mishap in peaceful times was extremely stressful during a period of national crisis. Cohen's African advisers helped him to resolve the problems quickly with the owner of the animal, ands he and his family were able to go to the peace of their field site destination.

## Factional Conflict

A milder form of the same phenomenon is the experience of living through acute conflict within the unit being studied, for example, a struggle between factions in the village or organization under study, which might include violence. Overall, about 11% report having had this kind of experience as a part of fieldwork (Table 9).

## Hostage-Taking

A rare but severe form of interpersonal threat, almost always politically motivated (although the desire for ransom may be a motivation), is taking hostages. Five of the 204 respondents (2%) were involved in hostage-taking incidents in the field – two in Latin America, and one each in the Indian subcontinent, Africa, and Asia and the Pacific (Table 10).

*Table 8* Living through Political Turmoil

| | *N. America* | *Europe* | *L. America* | *India* | *Africa* | *Pacific* | *Total* |
|---|---|---|---|---|---|---|---|
| Total | 61 | 17 | 63 | 17 | 23 | 23 | 204 |
| Had condition, self | 8 | 1 | 15 | 5 | 7 | 3 | 39 |
| | *13%* | *6%* | *24%* | *29%* | *30%* | *13%* | *19%* |
| Others in group had it | 2 | 0 | 2 | 0 | 0 | 2 | 6 |
| | *16%* | *6%* | *27%* | *29%* | *30%* | *22%* | *22%* |

*Table 9* Factional Conflict

| | *N. America* | *Europe* | *L. America* | *India* | *Africa* | *Pacific* | *Total* |
|---|---|---|---|---|---|---|---|
| Total | 61 | 17 | 63 | 17 | 23 | 23 | 204 |
| Had condition, self | 6 | 2 | 5 | 1 | 3 | 3 | 20 |
| | *10%* | *12%* | *8%* | *6%* | *13%* | *13%* | *10%* |
| Others in group had it | 2 | 0 | 2 | 0 | 0 | 0 | 4 |
| | *13%* | *12%* | *11%* | *6%* | *13%* | *13%* | *12%* |

*Table 10* Hostage-taking Incidents

| | *N. America* | *Europe* | *L. America* | *India* | *Africa* | *Pacific* | *Total* |
|---|---|---|---|---|---|---|---|
| Total | 61 | 17 | 63 | 17 | 23 | 23 | 204 |
| Had condition, self | 0 | 0 | 1 | 1 | 0 | 0 | 2 |
| Others in group had it | 0 | 0 | 1 | 0 | 1 | 1 | 3 |
| | *0%* | *0%* | *3%* | *6%* | *4%* | *4%* | *2%* |

Perhaps the most famous incident of this kind was the abduction of three Stanford University students and a Dutch citizen by paramilitary kidnappers from the Congo. They were taken in 1975 from the Gombe Stream Reserve, where chimpanzees were being studied in the field by Jane Goodall and her associates. A ransom was paid by Stanford University before the students were released. One of those students, Barbara Boardman Smuts, went on to get a PhD in neuro- and biobehavioral sciences and became a professional anthropologist.

More recently, a woman archaeologist and her four-year-old child were kidnapped by guerillas in Peru and later abandoned in the desert. They managed to get back to safety.

## Assassination

Mowat speculates whether the death of Dian Fossey should be attributed to poachers or whether it was a product of a faction in the resentful power structure of Rwanda. The case reminds us that anthropologists may be killed in the field for motives that are political rather than personal.

Another example is the death of Ruth First Slavo, a South African-born anthropologist who was a professor at Maputo University in Mozambique, where she was killed in her office in 1982 by a mail bomb. It is suspected that the bomb was sent by the South African secret service to end her effective political protests against apartheid. First died on the job, even if she was not at the moment doing research "out in the field." Bridgit O'Laughlin, who works in the same research institute in Maputo, was injured in the same explosion.

Despite the difficulty of distinguishing political from criminal or merely interpersonal causes of dangers from other people, the hazards of political problems can readily be seen to be real and frequent. Overall, 42% of those who worked in the Indian subcontinent reported experiencing one or another of these kinds of problems, as did 37% of those who worked in Latin America, 35% of those who worked in Africa, and 26% of those who worked in Asia or the Pacific. Rates for those who worked in North America (22%) and especially in Europe (12%) were notably lower.

We have seen that interpersonal problems make a substantial contribution to the difficulties of fieldwork. They are not the major cause of death – our next topic, accidents, is that. And very likely the rates of interpersonal violence and difficulty are no higher for fieldworkers than they are for other members of the society under study, and maybe not any higher than in one's own society at home.

Human nature includes variation everywhere, and a person who has no difficulty at home may have a difficult time in the field setting. Conversely, a person with a reputation for difficult interpersonal relations at home sometimes does well in the field. And the best balanced and most socially skilled person may not find it possible to live and work in some circumstances without interpersonal conflict. A field site that is easy to work at one time may be very difficult a decade later. It is part of the nature of anthropology that anthropologists tend to take credit for fieldwork that goes smoothly, and to feel guilty or unprofessional when serious difficulties arise, especially in human relations. While these feelings are understandable, it is likely that many of the elements in the interpersonal equation are not under the control of the anthropologist.

. . . It is important that these problems should not be denied, especially to young people starting to think about becoming anthropologists.

# 17

# War on the Front Lines

## *Carolyn Nordstrom*

War is perhaps impossible: it continues nonetheless everywhere you look.

Sylvère Lotringer (1987)

### Munapeo

*As I wandered up into the town of Munapeo*[1] *from the dirt strip that served as a runway, I noticed the voids in the landscape of village life: the lack of houses and fields – razed, burned, or destroyed. The lack of social flow – well-worn paths empty of men returning from farm plots, women carrying water home, children running in endless games.*

*It was my first visit to Munapeo, but in the year I had just spent in Mozambique, I had seen a number of towns in similar straits. Munapeo had been held by the rebel group Renamo – responsible for the instigation of the war and the majority of terror-warfare practices and human rights abuses*[2] *– for some years. The Frelimo (government) forces had recently retaken the town. And the war was not far: gunshots and shouts from Renamo forces could be heard less than a kilometer away.*

*The sense of eerie abandonment gave way in the town center to an all too common scenario in war-torn Mozambique. Hundreds of people sat, slept, and worked in a clump of humanity, eschewing the few remaining bombed-out buildings in favor of makeshift tents. A limited supply of emergency foodstuffs, flown in on the cargo plane I had hopped a ride with, were being distributed to a surprisingly orderly line. The plane brought food but not cooking pots or fuel, and the ingenious tried to figure out ways to cook their grains in a town long since plundered for its goods and wood.*

*The battle-wise and violence-weary knew that food did not bring peace: a concentration of troops brought a concentration of (starving) civilians, which prompted the delivery of emergency resources, which then provoked renewed Renamo attacks seeking to loot the supplies. The war rolls over the town again.*

*Behind these scenes – the hungry and starving sprawled in the dust and the sun, the bombed-out buildings sporting military graffiti, the wild eyes and careless ranting of someone who "has just seen too much war" – are a host of further tragic realities. Some stories I never got used to: I sat listening incredulously as a soldier explained to me a typical fact of life:*

> *Renamo comes into town and some soldiers enter a hut and grab the woman and begin to rape her. Another soldier forces her husband to stand close by and look on. Usually these husbands do – they are so afraid for their*

Carolyn Nordstrom, "War on the Front Lines," pp. 129–53 from Carolyn Nordstrom and Antonius C. G. M. Robben (eds.), *Fieldwork under Fire: Contemporary Studies of Violence and Survival* (Berkeley: University of California Press, 1995). Copyright © by The Regents of the University of California.

*families that they think they should stay and help in any way they can, and besides, Renamo has threatened them all with their lives if they do not do as they are told. Then we [Frelimo forces] come into town, and if we find out about such rapes, we round up these men. I mean, they must be collaborators (with Renamo), for what kind of man would sit and watch his wife being raped?*[3]

*A mother comes up to me at this point and asks me to accompany her. She takes me to a shade tree where her son of about four years old is quietly sitting, and she draws back a dirty piece of cloth draped over one shoulder and falling to his lap. He has been shot in the groin, and the bullet is clearly still inside the child. Is there anything I can do? she wants to know. I look around at the town – no clinic, no medicines, no nurses, no running water. Even the indigenous healers cannot get outside of town to collect the herbs they need to treat. Other than passing out some antibiotics and some empty words of hope, there is nothing I can do. I sit down next to the child and realize he already knows.*

*These and a hundred other stories fill my head as I walk to the dirt airstrip to catch a ride out with a cargo plane that has come. But most of all, I think about the tragic fact that I can leave. The inhabitants of Munapeo cannot. In the contest for towns and the quest for security, both sides use the civilian population "strategically." When the control of a town shifts hands from one set of troops to another, and when the ability of the troops to hold that area is questionable, civilians are often gathered together around a troop base. Theoretically, this is for security: "unprotected" civilians provide easy labor sources or targets for vindictive enemy troops convinced they are supporters of the "other side." But, in fact, forced relocation provided troops with easily guarded populations who provided not only supplies and labor for the troops but also a buffer zone between the troops and the enemy. In case of an attack, it is the civilians who provide a wall of security. Because they were often forbidden to leave the immediate area, this meant that many were unable to attend their farms, and starvation often set in at an alarming rate. Entire communities were known to die off in this way.*

*It is less than a kilometer to the dirt runway, but no civilians are this far from the town center. I am reminded how close the war is for them when Renamo soldiers in the bush shoot at the plane as the pilot tries to land, something he is completely unaware of because he is landing to a rousing chorus of Aerosmith in his earphones. I think wryly back to the security clearance report I got before leaving for Munapeo: "No problem, safe and secure for travel."*

*When we touch down at the provincial capital, I glance to see if the two Russian twin turbine combat helicopters stationed there are in. One has "In God We Trust" painted on the side over a picture of an American dollar bill, and the other is emblazoned with the wings emblem from Paul McCartney's first album with his band, Wings.*

After more than a year in Mozambique, I was used to days such as this. The layers of conceptual havoc that surround the war had become, in a curious way, a fact of life – almost comfortable in an off-balanced manner. It was not always that way. When I first arrived, I was frequently assailed by what appeared to be sheer chaos. Uninitiated into reading between the lines, I could not figure out why security reports did not match security realities. I was philosophically stalled by listening to a man sympathize with a person for having to watch his wife being raped by enemy soldiers and then targeting him as an enemy for having let this occur. I had no framework with which to deal with a culturally constructed image of war (soldiers on a battlefield) that in reality turned out to be a four-year-old sitting silently under a tree knowing with an uncanny wisdom that he would probably die from a gunshot wound in his groin.

In this chapter I explore the three interrelated themes of chaos, ~~reason~~ (or what Feldman has said may effectively be called a crisis of reason), and creativity. Chaos abounds in war and in fact may be called one of its defining characteristics. It exists as both strategy and effect and permeates the entire war enterprise from perpetrators to victims. War, expanding on Elaine Scarry (1985), "unmakes" worlds, both real and conceptual. Both studying and writing about war call into question some of our enduring notions of reason. But what may be the most powerful aspect of studying war is not merely the deconstructive violence that attends to it but the creativity the people on the front lines employ to reconstruct their shattered worlds.

## Chaos and Camus's Absurdity

*A world that can be explained even with bad reasons is a familiar world. But, on the other hand, in a universe suddenly divested of illusions and lights, man feels an alien, a stranger. His exile is without remedy since he is deprived of the memory of a lost home or the hope of a promised land. This divorce between man and his life, the actor and his setting, is properly the feeling of absurdity. (Camus 1955:5)*[4]

In considering the many towns like Munapeo I observed during my year and a half of fieldwork in Mozambique, I found that understanding the war does not rest on the fact that the war begins to make any more sense as time goes on but that, as Mozambicans showed me, we begin to accept the existence of senselessness. As a Mozambican explained to me, reminiscent of Camus:

*Do you know why, when you meet a phantom on the road, you do not pass it by and look at it? Do you know what is so dangerously bewitching, so lethal, about looking? It is because if you turn around and look behind the phantom, you will discover him to be hollow. This war, it is a lot like that phantom.*

For the vast majority of Mozambicans, war is about existing in a world suddenly divested of lights. It is about a type of violence that spills out across the country and into the daily lives of people to undermine the world as they know it. A violence that, in severing people from their traditions and their futures, severs them from their lives. It hits at the heart of perception and existence. And that is, of course, the goal of terror warfare: to cripple political will by attempting to cripple all will, all sense.

To understand the war in Mozambique is to multiply the small vignette of Munapeo a thousandfold. But to understand Munapeo is not to understand the war. For each person's experience of the war is unique, and the characteristics of the war – the form the conflict takes – varies from village to village, district to province. I could as easily have begun this chapter with the story of the town I saw that was completely burned to the ground, all its inhabitants gone, no one knew where. No one knew where, because no one officially knew the town was destroyed. When I returned to the provincial capital and later to the country's capital, I inquired about the fate of this town. No one had even heard it had been burned out. With a war that has affected one-half of the entire country's population, it is hard to keep track of every casualty, including entire towns.

I could also have started this chapter with the story of any of the hundreds of thousands who have been maimed, displaced, or kidnapped. Stories such as the following are legion in Mozambique. These were the words of a person I spoke with the day after he emerged from the bush after having escaped from Renamo:

*We were under Renamo control for several years. They came in and took everything, including us. We were forced to move around a lot, carrying heavy loads for Renamo here, being pushed there for no apparent reason. People died, people were killed, people were hurt, cut, assaulted, beaten . . . there was no medicine, no doctors, no food to help them. My family is gone, all of them. Only I am here. But the violence and the killing is not necessarily the worst of it. Worst of all is the endless hunger, the forced marches, the homelessness . . . day in and day out a meager, hurting existence that seems to stretch on forever.*

The level of violence in this man's story is considered "normal" in the war. True horror is reserved for stories that combine unbelievable brutality with sheer senselessness.

*The Bandidos Armados [Armed Bandits: Renamo] came into our town. They rounded all of us up who had not been killed in the initial attack and brought us to the center of the village. They took my son, and they cut him up, they killed him, and they put pieces of him in a large pot and cooked him. Then they forced me to eat some of this. I did it, I did not know what else to do.*

The formation of Renamo and the war helps to explain the inordinate amount of terror warfare that has characterized this war. Mozambique's "internal" war was developed and guided externally. The war began when Frelimo (Frente de Libertaçao de Moçambique) came to power in Mozambique after the country achieved independence from Portugal in 1975. Proapartheid governments, first Rhodesia and then South Africa, formed and led the rebel group Renamo (Resistência Nacíonal Moçambicana) in an attempt to undermine the model and assistance that a successful black-majority Marxist-Leninist country offered to the resistance fighters of their countries. While pro-Renamo supporters and opportunists do exist within Mozambique, essentially the rebel soldiers functioned with little popular support. Because destabilization, not coherent political ideology, was the defining factor in Renamo's formation, dirty war tactics – those using terror tactics in the targeting of civilian populations – predominate. The human rights violations have been recognized as being among the worst in the world.[5]

The extent of the violence in Mozambique can be captured in a few statistics. Over one million people, the vast majority noncombatants, have lost their lives to the war. Over two hundred thousand children have been orphaned by the war (some estimates are much higher). Adequate assistance is more hope than reality in a country where one-third of all schools and hospitals were closed or destroyed by Renamo and where a single orphanage operates. Nearly one-fourth of the entire population of 15 million people has been displaced from their homes by the war, and an additional one-fourth of the population has been directly affected by the war. In a country where 90 percent of the population lives in poverty and 60 percent in extreme poverty, the toll has been devastating.

These stories of war, individually and collectively, are distinctly Mozambican. It is their lives, their suffering, their courage, that is on the line. But the war itself is not uniquely Mozambican. In addition to the founding role played by Rhodesia and then South Africa, disaffected Portuguese former colonialists have played a critical role in Renamo's war. As well, Renamo has been aided by numerous Western right-wing organizations and religious groups and assisted by Western military advisers, arms merchants, and mercenaries – placing the war, and its defining strategies, squarely in an international political, economic, and military network. The strategies used in Mozambique have been applied in scores of other wars around the globe, carried through the same international network by the same international cast in search of power and gain (Nordstrom 1994a, 1994b).

This international entanglement of alliances, antipathies, and mercenaries allows the transfer of fundamental strategic orientations and specific tactical practices from group to group across international and political boundaries. Transferred with these are the cultural belief systems: beliefs about what are deemed acceptable, and necessary, processes of war, violence, and control in the quest for power. These wars, which have taken place primarily in non-Western countries, have focused on the use of terror tactics and the targeting of civilians and social infrastructure. They carry the legacy of a cold war that has itself been given over to history.

To understand what is attacked in a dehumanizing war necessitates an understanding of what it is to be human. For Mozambicans, this includes, but is certainly not limited to, the following. Mozambicans are nurtured in the bosom of family, and this is grounded in the skills and behaviors that sustain life – in working, in cultivating, in harvesting, in consuming. As family members, they illuminate the nexus of a time/place continuum: the fecundity of the ancestors has been instilled in them and comes to fruition in the familiar landscapes of home, hearth, and the land they were born to. They thrive as part of a community, and a pattern of friendships, obligations, and shared goals gives tangible substance to their sense of world. Mythological space landscapes geographic space: ritual, ceremony, and belief bring the universal home. The eternal, the social, and the collective are made apparent through the individual and the particular. Cultural process brings "home" the nature of reality through the physical form of the participant's everyday world. They sit in a gathering place in their community, just outside their homes, surrounded by their fields and animals and belongings, supported by their family and acquaintances, and they peer through ceremony's door into the mysteries of the universe until they have made sense of it and it of them. Their community, mythical and physical, takes shape in relation to a landscape of cultivated and wild spaces, within a network of other communities that together follow patterns of exchange, of everything from people and goods to aggressions and innovations.

The words of a Mozambican woman friend of mine poignantly demonstrate the destruction that the war has brought to millions of her compatriots:

*Epah, Carolyn, this war. My youngest son came of age not too long ago, and I felt obliged to take him back to the land of my people to perform the ceremonies that would ensure that he grows into a strong and healthy member of our family. The journey was a heart-stopping one – as you know the roads are so unsafe, and we had to walk a majority of the way to avoid land mines and rogue soldiers. I was so frightened I would lose my son before he could even come of age properly. But when we arrived in my birth home, it was so very disappointing. I remember a house filled with the happy shouts of children, lush farmlands flowing out from its doors, vegetables to pick for food, and our animals dotting the hillsides. Always a fire with food cooking, always a story being told.*

*It is so awful to see it now. My mother is the only one there now: my father, as you know, was killed by Bandidos [Renamo], my grandparents just died of the war: not enough food, medicines, hope. My mother, she will never be the same after all the attacks she has lived through, after seeing her husband slaughtered. The horror of the violence is etched on her face and her soul. The house is dark, decrepit and empty. The Bandidos have carried off everything they could in the innumerable times they have come through. The fields are destroyed, and my mother refuses to replant them, for every time she does, the Bandidos come and raid and then burn the fields. The animals are long gone, killed by the soldiers. The neighbors are few and far between, killed off, run off, starved off. No more laughter, no more stories, no more children. No more home. Even worse, when we arrived there, I found it was going to be really difficult to hold the ceremonies we wanted to for our son. The noise and music of the ceremonies attracts the Bandidos. They hear it and come to attack. We cannot even perform the ceremonies that make us human. We did a ceremony, yes, but a mere skeleton of that which tradition calls for. Skeleton, yes, that is a good word – we are living skeletons of the war.*

With the onslaught of excessive violence, the boundaries defining family, community, and cosmos slip, grow indistinct, reconfigure in new and painful ways. And through the breached boundaries, the substance of each spills out across the landscapes of life in a way that is unstructured, highly charged, and immediate. Family has been shattered, not only by death and displacement but by the impossibility of unresolvables: Is a missing relative alive? Can I protect those still with me? How do we live like a family when that which defines family life no longer exists? In its most fundamental sense, family is a historical continuum, and home the place where it unfolds. When these are disrupted, the grounding of self in time, place, and space is upended. Left to a here and now unmoored in time, people lose the guidance of tradition, the comfort of tomorrow. What then becomes of the person severed from time and place? Not the flesh and bones body but the intangible and subjective effervescence animating personal identity and bringing the self to life – that which, all told, makes humans human. The world, as many Mozambicans sadly said to me, is no longer human.[6]

When violence reaches this level of severity, identity itself suffers, as evinced in the words of a *dislocado* (dislocated: internal refugee) in southern Mozambique. As we talked, he stood, handmade hoe in hand, surveying the dry and barren fields where he and many other dislocados had recently arrived to try and eke out food and a fragile home. I thought at the time I had never seen a face so sculpted by resignation and determination at one and the same time.

*We have arrived here from all over, scattered victims of Renamo violence. Everyone has* *lost everything they had. Their homes were burned, their goods stolen, their crops destroyed, their family members slaughtered. Even those that managed to flee often ran different directions from the rest of their families, and today do not know if the rest are alive or dead. Many have been through this cycle more than once, having fled to a "safe area" only to be attacked again. Me, this is my third relocation. I do not know where most of my family is. Maybe we will be attacked yet again – we hear Renamo passing by here at night. It is difficult to find the will to plant crops and tend children when it may all be taken from us tonight, and maybe we will not survive this time.... The worst of it is the way this attacks our spirits, our very selves. Everyone here thinks: Before this I knew who I was, I farmed the land that my father farmed, and his ancestors before him, and this long line nurtured the living. I had my family that I fathered, and I had my house that I built, and the goods that I had worked for. I knew who I was because I had all this round me. But now I have nothing, I have lost what makes me who I am. I am nothing here.*

If people are defined by the world they inhabit, and the world is culturally constructed by the people who consider themselves a part of it, people ultimately control the production of reality and their place in it. They produce themselves. But they are dependent on these productions (Taussig 1993). Should one wish to destroy, to control, or to subjugate a people, what more powerful "target" could be found than that of personhood and reality? To destroy the world, encapsulated in the nexus of place and person described above, is to destroy the self.

It is my opinion that self, identity, and the experience of the world are mutually dependent for all people, as contemporary existential, phenomenological and postmodernist theory are demonstrating. But this view has long permeated African thought. Without trying to overgeneralize African epistemology, I found many Mozambicans hold a similar view to the scholars E. A. Ruch and K. C. Anyanwa (1984:86–7).[7]

> The African culture makes no sharp distinction between the ego and the world, African culture makes the self the center of the world. ... The world which is centered on the self is personal and alive. Self-experience is not separated from the experiencing self. The self vivifies or animates the world so that the soul, spirit or mind of the self is also that of the world. ... What happens so the world happens to the self. Self disorder is a *metaphysical contagion* [italics in original] affecting the whole world.

It would appear to be equally valid to conclude that world disorder is a metaphysical contagion affecting the whole self. Yet if the world makes the self, the self equally makes the world, and this is why terror warfare is ultimately doomed to fail. As we will see in the section on creativity, people have the creative wherewithal to re-create the worlds war has destroyed.

## Reason

> *It is worth noting that the language peculiar to totalitarian doctrines is always an academic and administrative language.*
>
> Albert Camus

Western epistemologies generally try to find "The Reason" (universal and specific) for war – to fix it in time and understanding. If only we could just bring to light the specific structural, mythological, interpersonal acts of domination and resistances, war would make sense. But these are sweeping analyses, ones that all too often leave out the individuals – living, suffering, dying – who *are* the war. Individuals do not make up a generic group of "combatants," "civilians," and "casualties" but an endlessly complex set of people and personalities, each of whom has a unique relationship to the war and a unique story to tell.

Based on my field experiences at the front lines of wars, I hope to challenge – to draw a line through – the epistemologies of Reason, with a capital R, as it applies to War. When war actually becomes a matter of life and death, Reason is replaced with a cacophony of realities. One cannot peel back the layers of the onion to find the core phenomenon; for, as we all know, the onion, like reality, is composed only of layers.

I am reminded of a conversation I had with a young teenage soldier in the bush of north-central Mozambique. I asked him why he was fighting, and he looked at me and in all seriousness replied, "*I forgot.*" For this person, the tattered clothing he was wearing, the gun he carried, the fear and hunger he constantly felt, the "endless days and nights of living in the remote bush on the run without food, shelter, or comfort" were realities. The "why" of it all was far less intelligible; unimportant even.

Behind the political ideologies, the military strategies, the international arms and ally networks that support the war effort, and the commanders that channel this down to the front lines, "I forgot" can exist, the core of the phenomena.

The problems that surround reason do not pertain exclusively to war. The whole notion of reason as it has been defined in Enlightenment philosophy is in crisis. Epistemology can no longer conveniently be separated from ontology, word from act and concept, subject from object, reality from construction. This crisis extends to the heart of theory. For, ultimately, we as theoreticians live our reason. We cannot step outside of it to assess it in any final sense. We are, as Allen Feldman (1991) points out, inescapably implicated in our reasoning about reason. This is nowhere more evident than when we begin to try to "make sense" of the cacophonous flow of our field observations – to wrench word from experience.

Terror warfare, such as that defining Renamo's in Mozambique, seeks to sever all relationships grounding personhood to enforce complete political acquiescence. But, too, our theories are all too often abstracted – and sever personhood from narrative and text. In Western epistemology, we have a legacy of thinking about violence as a concept, a phenomenon, a "thing." We reify it, we "thingify" it, as Michael Taussig (1987) cautions, rather than recognize it as experiential and rendering it real. This approach stands in sharp contrast to the Mozambican's view of violence – a view that sees violence as fluid, as something that people can both make and unmake.

A concern with the reasons of war comes dangerously close to a concern with making war reasonable – which, of course, is a goal of the Enlightenment process. Maybe this search for reason has allowed us to "explain war away": concretized in theory, set in fact, distanced to a comfortable vantage point. I suggest we consider the fact that this search for the "reason" for war actually silences the reality of war.

In her study of torture, Scarry (1985) has noted that pain unmakes the world of the victim. Expanding on Scarry, I (Nordstrom 1992a, 1992b) have suggested that war's violence unmakes the world at large both for those who experience it and for those who witness it. Violence deconstructs reason. The question then arises, Does writing and reading about violence unmake the world? Is this why so many of our theories on violence are modernist, clear concrete categories distanced from the raw experiences they purport to explain?

Another paradox may lie at the core of this question about "writing" violence in theory. How can we write about the "unmaking" and "creating" of the world in a "made" world of academic prose? No matter how representative we try to be, theory and literature have a structure and an order that they impose in and of themselves, always once removed from experience, intolerant of chaos. As Jean Baudrillard (1987:133) succinctly points out, "Theory is simulation."

Theories about violence will always struggle with these issues of representation. Violence is an unsettling topic. It raises piercing questions of human nature, social in/justice and cultural viability – and about our personal accountability and responsibility in the face of these. It challenges cherished notions of a just world and throws into stark relief the sheer daunting complexities of human and cultural reality. It utters the unutterable.

## And the Anthropologist?

*Lived experience overflows the boundaries of any one concept, and one person, or any one society.*

Michael Jackson (1989)

On entering the field, we enter the domain of lived experience. What is "safe" is a study in smoke and mirrors. Everyone has a story, complete with vested interests, and all the stories collide into contentious assemblages of partial truths, political fictions, personal foibles, military propaganda, and cultural lore. The louder the story, especially when it comes to violence and war, the less representative of the lived experience it is likely to be. In the midst of wars of propaganda and justification, the most silenced stories at war's epicenters are generally the most authentic.

To understand a war is not the same thing as understanding a war in the town of X and among the people who populate it. In the same way that a body cannot be understood by a finger, a war cannot be understood by a single locale. It was the war in Mozambique, and the Mozambicans' experience of it, that formed the core concern of my research. Because this research question demanded different field techniques than those normally associated with anthropological studies set in a specific locale, I followed an approach I call the "ethnography of a warzone" (Nordstrom 1994b). Here, the theme of war, rather than a specific locality, situates the study. Process and people supplant place as an ethnographic "site." My reticence to situate this study in a given locale extends to the urban centers and the institutions of power brokers (the "site" of traditional political science research) – the places where war is formally defined, debated, and directed. These sites add to the study, they do not define it.

I selected Zambezia Province, in north-central Mozambique, as my home base for the majority of my stay in the country as it was the province most seriously affected by the war, and one that offered rich cultural diversity. But in the year and a half I worked in Mozambique, I traveled not only throughout the province but also through six of Mozambique's ten provinces. In each location, I followed the ebb and flow of the war from urban centers to rural outposts, visiting locations on the peripheries of the war, locales that had recently been attacked, and villages and towns that had changed hands from the government to the rebel forces a number of times. Roads were heavily land mined and subject to frequent attacks and seldom, if ever, traveled outside of sporadic military convoys confined to a few main transitways. Like virtually everyone else who did not have the skills to walk across provinces, I depended on air travel. Unlike many, my major mode of travel was cargo planes taking emergency supplies to war-devastated areas lucky enough to have a flat dirt runway relatively free of mines. In what I found to be one of the war's many ironies, my ethnography, like emergency supplies and government officials, was confined to locations where a landing strip and a security clearance could be eked out. I dubbed this "runway anthropology."

The nature of this ethnography thus reflects in many ways the nature of the reality of many Mozambican lives: conflict, starvation, deprivation, and the demands of work, family, and health have produced an extremely fluid population. As I noted before, nearly one-third of the population has experienced some form of dislocation.[8] These Mozambicans can no longer, at present, ground their "selves" – their lives, their livelihoods, their dreams – in a single place. In responding to an external threat, they carry reworked notions of home, family, community, and survival with them. Repositioning has come to define a major sociocultural current.

In each place I visited, I made a concerted effort to collect the stories of average people, many of whom found themselves on the front lines of a war they neither started nor supported. Eschewing the popular notion that battlefields are comprised of male adult soldiers – especially since the vast majority of casualties in Mozambique were noncombatants – I turned my attention to both sexes and all ages, equally. Given the circumstances of the war, I worked in areas where the rebels were in close proximity, but I never elected to work in rebel-occupied areas.

The logistics of conducting an ethnographic study in a warzone are not as complicated as the fact that we begin to care about the world we have entered. We can sympathize with the trauma of a person looking over the charred landscapes that used to be called home; feel the gut-wrenching horror they feel wondering if the rest of their family made it to safety or not. We can understand the overwhelming grief of people who had to leave a family member where she or he fell, unburied, as they fled an attack, knowing they have condemned a loved one to roam the earth as a sorrowful rogue spirit with no resting place.

Everyone grapples with violence in his or her own way. What is traumatic, difficult, hopeful is in all likelihood different for every person in the field. It is impossible to escape the impact of the sheer violence: I will carry with me images of violence for the rest of my life which are variously poignant and unsettling, absurd and tragic. Some resonate with examples in the general literature and media on warfare, and these constitute the acceptable, and in many ways privileged, discourses on violence. The maimed and the dead – victims of political torture, heroes and martyrs of causes, innocent victims of repression – fill this category.

Yet it is not the raw violence per se that most captures the essence of war for me. Curiously, the images that have done so for me seldom appear in formal discussions of warfare. To give one example: one of the things that struck me the first time I saw the massacre of innocent civilians was that, in the physical trauma of death, many of the dead men's pants had fallen down. This example may appear frivolous to people who have not witnessed such scenes. But to those living daily with the specter of large-scale political violence, death scenes of familials – not only butchered but exposed – present a powerful statement on death, (in)dignity, and the nature of human existence.

It is misleading, however, to focus exclusively on the physicality of bodies as the repository of violence. When I am among people who have not been near the brute force of war, I am often asked, "What was it like? Did you see many dead bodies?" The question rankles. Even if I were to answer the question, which I never do, it would not be the ruined bodies themselves I have seen that summarize the agonizing truths of war for me but the stories behind the bodies. In considering the question of what war is like, I might, for example, think of the color pink and the trails it has left on the landscape of war in my mind. Two stories, related only by color, help to explain this.

Early in my years of studying war, I was visiting a village I did not know well, several hours travel from my in-field home. I was sleeping in the house of "a relative of a friend of a friend" I had never met. Quite early in the morning, I was roused unexpectedly from bed and asked to get dressed. No explanation, no food or coffee. There was something people wanted me to see. A group of men were waiting outside the door, most of whom I did not know, and we set off on foot through the fields and finally into the forest. We walked for quite a while it seemed. Finally we came into a small clearing, and in front of us a dead man hung from a tree – suspended on a pink bedsheet. The man in charge turned to me in concern and said, "We need to find out if this is murder or suicide." Had this man chosen to escape insurmountable personal troubles, impossible war demands? Or had the war found him? Had someone killed him?

I am never sure why I am included in or excluded from certain things in the field. I had no idea why I was brought to witness this poor man hanging forlornly in the early morning sun. Did people think because of my interest in traditional medicine I was a medical specialist? Did they want someone to witness the inescapable violence people had to live with, someone who could carry the story back to the urban centers? I never did find out. They asked me to help examine the body to try to determine if the man had been murdered or not, and I did. But mostly I remember watching that body swing on the pink bedsheet in the slight breeze as I wondered about war, tragedy, absurdity, and the insurmountable.

The second story begins in the same time period. I had a friend in the community in which I lived who eased the tragedies of war for me. He was a fun and life-affirming man who loved ceremonies, parties, a good joke, and his fellow human beings. I could always talk to him about the war, and he listened with a sympathetic ear. He hated the conflict tearing at his country.

The next time I visited the country, I looked forward to reuniting with my friend. The war continued, and deprivation and terror had touched everyone's lives. When I reached my friend's house, I was surprised to see an assault rifle leaning in the entryway, a revolver on the living room table. I settled into a chair to catch up on the news. An armed man materialized in the shadows of the porch and had a hurried whispered conversation with my host. I looked quizzically at my friend when he returned, and he sighed and handed me a photo album. The album itself was the kind you could find at any department store: the cover depicted the common scene of a young couple walking hand in hand in some romantic locale at sunset – all colored in bright pinks and images of serenity. Inside, however, were pages of photographs of maimed, mutilated, and murdered youths from the area. My friend shrugged his shoulders and explained the war had reached an intolerable level, something had to be done to save the country. He had decided to join the "security forces" to combat the "terrorists." The pictures were of his work, the "solutions" he and those he worked with employed. The victims, mostly youths, looked to me to have died alone and unarmed: in a search for information; as a warning delivered in a message of terror; in a fearful and retributive rage, anything but as soldiers on a battlefield. I have never gotten over the shock of this. How could I be friends with a man capable of such torture? How could I have *been* friends with such a man? My friendship with this person is over. I have not kept in touch with him. But the impossible quandary of the situation stays with me: it represents the harsh realities of war that many live with on a day-to-day basis. And it is not so much the gruesome pictures of bodies that distress me; it is the hopeless incongruity of their being in that photo album with the serene pink cover.

These are not the only scenes that define the heart of war for me, nor the only colors, sights, smells, tragedies, and fears I have experienced through others' experiences of war. Each one gives a depth and a complexity to violent conflict that goes well beyond the shallow depictions of war that are offered in the traditional texts and media sound bites that "describe" war.

## Creativity

*[The world is] created out of human experience.*

E. A. Ruch and K. C. Anyanwa (1984)

Renamo, with its tactics of severing the noses, lips, and ears of civilians, seems to reclaim the original sense of the absurd: "The absurd, from the Latin, *absurdus*, is literally the deaf, the voiceless, and hence the irrational" (Ruf 1991:65).

But if war, especially terror warfare, strives to destroy meaning and sense, people strive to create it. This, ultimately, is why dirty war is doomed to fail. No matter how brute the force applied to subjugate a people, local-level behaviors arise to subvert the hold violence exerts on a population. This, of course, is a highly contested process. The situation at the local level is complex and contradictory. There are people working within the political, military, and economic spheres who seek to benefit from the fractures caused by war. Others work equally hard to solve the inequalities, injustices, and abuses caused by war and those who exploit violence for their own gain. It is the latter that interests me here.

Traditional Western approaches to violent conflict do not often recognize the creative strategies people on the front lines employ to survive the war. I was little prepared for the way in which people tried to reconfigure the destructive violence that marked their lives and to rebuild worlds so wrenchingly taken away from them by violence. It was only when I was in the middle of Mozambique (both literally and in terms of my research) that I began to appreciate the creativity of the average people caught in the traumatic contingencies of warfare. While this creativity does not extend to all people and all parts of the war, I am always encouraged by how much exists in day-to-day life. To give an idea of the range and richness of these world-building actions, I will give three different examples that can be introduced as the creating of symbols (the three monkeys), of society (the transport of fish), and of culture (the work of healers).

The first example involves three little carved wooden monkeys. When I first went to the country in 1988, the war economy was such that few market goods of any kind were available. I was always interested in the fact that one of the things you could find with regularity was a set of three little carved monkeys: see no evil, hear no evil, speak no evil. For me, this was especially telling considering the regularity with which one heard stories of Renamo severing the ears and lips of civilians to silence resistance and control political will. One day I was sitting on the curb talking with a street vendor acquaintance of mine with whom I frequently sat and discussed the war (it had taken his legs, his family, and his home) and better days. During a lull in the conversation, with a sly twinkle in his eye, he pulled out a set of three monkeys to show me. The first monkey had one hand over his mouth and the other over one eye, but the second eye peered out wide open and both ears were uncovered and listening. The second monkey had one hand over one eye and the other hand over one ear; this time the mouth was uncovered and twisted into a grimace or a cry, but still one eye was watching and one ear was listening. The last monkey sat with a cynical grin on its face: eyes, ears, and mouth open and cognizant. This monkey sat with its hands covering its groin. The symbolism is not lost on Mozambicans: the numbers of women who have been raped in the war are legion, and a significant number of men have been emasculated both physically and figuratively.

I have returned to Mozambique twice since my first trip and have traveled from the plush offices of power brokers to the crumbling embers of villages in the far reaches of the country. And in the places where force became violence, the subversive message of the monkeys – that we will cover our ears when you cut off our lips and still look with one eye; that we will watch, listen, and speak, but we will "cover our tails" in doing so – was reflected time and again, in village after village after town. The first part of the message conveys resistance; the second laces it with wry humor. The two together have given many a hope and a will to survive a very dirty war.

The three monkeys stand as popular symbologues (dialogues based on symbolic representations) that speak both to the war and through the war: statements constructed by the victims themselves to convey the complex way violence is lived, learned, subverted, and survived. Symbologues abound during war. "Violent concentrated action," writes Antonin Artaud (1974:62), "is like lyricism; it calls forth supernatural imagery, a bloodshed of images." To speak directly about the war is to court danger. So songs, myths, parables, jokes, and stories circulate – each a palimpsest of meaning wherein "mythical" villains, heroes, murderers, and traitors implicate contemporary actors in the war drama. Everyone in the know "knows" what is being conveyed about whom: who to trust, fear, avoid. For those not in the know (one hopes, those who have the power to kill), these are "simply stories." The "reason" Mozambicans apply in such situations extends well beyond that ascribed by Enlightenment philosophies focusing on discursive consciousness. It is a form of creative reasoning that combines symbolic, emotional, representational, discursive, and existential realities. Generally speaking, the split between epistemology, ontology, and life is an artificial one for Mozambicans.

> In African culture . . . experience does not address itself to reason alone, imagination alone, feeling and intuition alone, but to the totality of a person's faculty. The truth of this experience is lived and felt, not merely thought of. (Ruch and Anyanwa 1984:86–7)

There are many other ways people work to subvert terror and destruction and to reconstruct a purposeful social universe. In Mozambique, these are not just part of the war response; they are critical to survival. The second example I cite here became apparent to me when I was in an inland town that had recently been attacked a number of times. Crops and animals decimated and goods stolen, the markets had little to offer. I was therefore taken aback to find for sale some fish that had seen better days. This is particularly noteworthy, for it entailed several men walking with baskets of ocean fish on their heads for seven days from the coast through several language and ethnic communities and a number of dangerous war zones. This is a trip no formal trader would brave: the dangers were too great and the profit negligible. So why make such a trip? The men's answers to me – "Because that's how life goes on" – did not make a lot of sense at first. But as I listened to them talk, I realized that through their journey they performed an invaluable function. They carried messages for families and friends separated by the fighting; conveyed details on troop deployments and dangers; and transmitted critical economic, crop, trade, and political news, not to mention gossip and irreverent stories, between communities severed from one another by the war. They linked different ethnic and language groups in a statement that the war was not about local rivalries and could not be, if they were to survive. They forged trade and social networks through the disordered landscapes of violence. And by walking for seven days with baskets of fish on their heads through lethal front lines, they simply defied the war in a way that everyone they passed could enjoy and draw strength from. They were, literally, constructing social order out of chaos.

These traders created outgoing linkages in the country. In a complementary process, people also work to create a valid community and a stable social universe wherever they find themselves. Curandeiros[9] are a locus of creativity in solving the problems of war. Encoded in their traditions are idea(l)s that mitigate the harmful effects of abusive power, violence, and warfare. While African medicine has long assisted in warfare (Lan 1985; Ranger 1982, 1985), in Mozambique it has largely condemned Renamo's ruthlessness. I spoke with well over a hundred curandeiros throughout the country, and most had developed "treatments" aimed at protecting civilians and ameliorating the violence unleashed on society.

In refugee camps, in informal dislocation centers, in burned-out villages trying to rebuild, I found curandeiros performing treatments to take the war out of the community, the violence out of the people, and the instability and terror out of the culture. As one curandeiro explained,

> *People have just seen too much war, too much violence – they have gotten the war in them. We treat this, we have to. If we don't take the war out of the people, it will just continue on and on, past Renamo, past the end of the war, into the communities, into the families, to ruin us.*

Scholars such as Pierre Bourdieu (1977) and Jean Comaroff and John Comaroff (1991) brought to academic attention what the curandeiros have long known, that hegemonic ideals and cultures of violence can be dangerously, and unwittingly, reproduced throughout a society and can even undermine resistance and resolution.

Hundreds of conversations I had with Mozambicans reflected their preoccupation with defusing the culture of violence the war had wrought. It is a violence, they stress, that can last far beyond formal military cease-fires. People constantly reminded themselves and others about the insidious nature of violence, which allows it to reproduce itself and to destroy worlds and lives in the process. It is as if, fearful of the tendency toward *habitus* – toward what Bourdieu (1977:191) calls "unrecognizable, socially recognized violence" – Mozambicans have set into motion a cultural dynamic that continually challenges the entrenchment of a culture of violence. The following quote is from my field notes. I was sitting with several older women in a village that had seen a great deal of the war. The bombed-out and uninhabited husks of buildings stood outlined behind us in the afternoon sun, behind the sea of small thatch and mud huts that had sprung up to house the many people displaced by the war. We were sitting on the ground chewing on the stalk of a weed (I was chewing on the weed because the women had handed it to me; the women did so out of a habit they had developed to appease their appetites when food was scarce). We were talking about the war's impact on people's lives.

> *When people come back to our community after having been kidnapped and spending time with the Bandidos [Renamo], or arrive here after their community has been destroyed by the war, there are a lot of things they need. They require food and clothing, they need a place to live, they need medical attention. But one of the most important things they need is calm – to have the violence taken out of them. We ask that everyone who arrives here be taken to a curandeiro for treatment. The importance of the curandeiro lies not only in his or her ability to treat the diseases and physical ravages of war but in the ability to take the violence out of a person and to reintegrate them back into a healthy lifestyle. You see, people who have been exposed to the war, well, some of this violence can affect them, stick with them, like a rash on the soul. They carry this violence with them back to their communities and their homes and their lives, and they begin to act in ways they have never*

*acted before. They bring the war back home with them – they become more confused, more violent, more dangerous, and so too does the whole community. We need to protect against this. The curandeiro makes consultations and patiently talks to the person, he gives medicinal treatments, he performs ceremonies, he works with the whole family, he includes the community. He cuts the person off from any holds the war has on him or her, he scrapes off the violence from their spirit, he makes them forget what they have seen and felt and experienced in the war, he makes them alive again, alive and part of the community. He does this with Bandido [Renamo] soldiers too. If someone finds a soldier wandering alone, we take him and bring him to the curandeiro. Most people do not really want to fight. These soldiers have done terrible things, but many of them were kidnapped and forced to fight. They dream of their home and family and machambas [farms], of being far away from any war. The curandeiro takes the war out of them, he uneducates their war education. He reminds them how to be a part of their family, to work their machamba, to get along, to be a part of the community. He cures the violence that others have taught.*

In the midst of war, the treatments the curandeiros provide are not set prescriptions faithfully reproduced. They are creative acts in the true sense of the word. Worlds are destroyed in war; they must be re-created. Not just worlds of home, family, community, and economy but worlds of definition, both personal and cultural. As people look out over a ruined landscape that was once home – now shorn of life and livelihood, humanity and hope – they cannot simply "reconstruct society as it was before." For in the violence and upheaval, it cannot be, may never be, the same as "it was before."

In the face of the monkey's creation of symbologues, the fish vendor's forging of social order, and the curandeiro's production of culture, I find the theories on the cultural construction of reality relevant but inadequate.[10] They start from the basis of an operating culture that imparts knowledge through interpersonal interaction. What happens when very little is operating and what has operated is of little immediate use? What shards of cultural relevance do the vendors and healers have to build on? Worlds cannot simply be created; they must be created anew. How do the poetics and practices of the people in these three examples interweave in the creation of cultures of survival and resistance?

The dilemma is clear: between the world as it was, the world as it should be, and the now of a world destroyed lies an abyss, a discontinuity, a need to define the one by the other, and the impossibility of doing so. The solution, Mozambicans taught me, lies, in part, with the imagination. I have come to think that this is a trait people have specifically nurtured to counteract destructive violence. When people look out over a land that should resonate with meaning and life, but that now stares blankly back with incomprehensible images of barren fields, broken communities, tortured bodies, and shattered realities, they are left with the choice of accepting a deadened world or creating a livable one. It is the imagination – creativity – that bridges the abyss, if not to reconstruct the past, to make the present livable.

Scarry (1985:163) has argued that pain unmakes the world and imagining makes it. Together "pain and imagining are the 'framing events' within whose boundaries all other perceptual, somatic, and emotional events occur; thus, between the two extremes can be mapped the whole terrain of the human psyche." She invokes Sartre in exploring the idea that absence provokes an imagining of a special sort.

> Sartre, for example, draws conclusions from the fact that his imagined Pierre is so impoverished by comparison with his real friend Pierre, that his imagined Annie has none of the vibrancy, spontaneity, and limitless depth of presence of the real Annie. But, of course, had he compared his imagined friends not to his real-friends-when-present but to his wholly absent friends, his conclusions would have been supplemented by other, very different conclusions. That is, the imagined Pierre is shadowy, dry, and barely present compared to the real Pierre, but is much more vibrantly present than the absent Pierre. (Ibid.)

In like fashion, it is the destruction of the world that prompts such vivid powers of imagining in victims of war and violence.

But unlike Scarry's view, some Mozambicans are able to imagine their real friend, their real home, their real society and culture as vibrantly as the "real thing." We can afford to leave underdeveloped our ability to imagine our real friend Pierre in a reasonably stable world. But when Pierre is dead, disappeared, or maimed, and when the world that held him is so hopelessly destroyed that left unattended it can only ring a death toll for the society affected, people must create, and to do so, they must first imagine what it is they are going to create. For Pierre will never be the same, and the world is still at war.

For Scarry (1992), imagining is grounded in perceptual mimesis. For the Mozambicans, contemplating their ruined villages and contentious political imbroglios, there is little to mime – and imagining becomes an act of pure creativity.

Not all Mozambicans have such developed powers of creative imagining. Not unusually, the creative members of the culture – healers, visionaries, performers – have developed these skills to a fine art. Their talents lie not only with their abilities to imagine but also with their abilities to convey these images to others so that they, too, may share in the reconstruction of their symbolic and social universes. I have visited a number of communities that had been recently decimated by the war. One of the most powerful experiences I had at these times was sitting with people amid the fragments of what was once their home and community and listening, watching, the imagining – the creation of identity, home, and resistance afresh. I choose the word *watching* as well as *listening* purposefully: as the Mozambicans talk about what has happened and what will happen, and as they discuss this in the context of human nature and the meaning of life, I found I could not only understand but "see" the world they were creating. Apparently so did the others present. New identities of suffering and resistance were forged, home was reinvented, the world was relandscaped with significance, people survived.

# 18

# Reflections on Managing Danger in Fieldwork: Dangerous Anthropology in Belfast

*Jeffrey A. Sluka*

In many areas of the world, anthropological fieldwork is more dangerous today than it was in the past.[1] There are approximately 120 "armed conflicts" (euphemism for wars) in the world today (Nietschmann 1987), and given that about one-third of the world's countries are currently involved in warfare and about two-thirds of countries routinely resort to human rights abuses as normal aspects of their political process to control their populations, it is clear that few anthropologists will be able to avoid conflict situations and instances of sociopolitical violence in the course of their professional lives (Nordstrom and Martin 1992:15). While it has long been recognized that danger is probably inherent in anthropological fieldwork, it is only recently that the methodological and subjective issue of danger has been addressed directly and systematically. In 1986, Nancy Howell first called attention to the need to discuss the issue of danger in fieldwork in an unpublished paper, "Occupational Safety and Health in Anthropology." She noted that the personal dangers involved in doing fieldwork had largely been ignored, denied, or taken for granted and argued that this issue should be a major concern of anthropological fieldworkers. She also suggested that one of the professional associations should conduct a comprehensive survey of occupational safety and health in the discipline, and this idea was adopted by the American Anthropological Association (AAA). In 1990, the first publications directly dealing with danger in fieldwork emerged – an AAA special report titled *Surviving Fieldwork* (Howell 1990) and an article titled "Participant Observation in Violent Social Contexts" (Sluka 1990). This chapter presents an updated and revised version of my earlier article on managing danger, incorporating reflections on new fieldwork. I begin with a brief description of the research setting during my two periods of fieldwork in the Catholic-nationalist ghettos of Belfast, Northern Ireland, in 1981–2 and 1991 and then make some general comments and recommendations concerning the conduct of ethnographic research in dangerous or

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violent social contexts, deriving from these experiences and similar ones by other anthropologists.

While special ethnographic, methodological, theoretical, and ethical sensitivities are required when working on, and in, dangerous areas, to a substantial degree the dangers faced by anthropologists in their fieldwork can be mediated through foresight, planning, and skillful maneuver. While this chapter deals specifically with participant observation in countries characterized by political instability, conflict, and insurgency, much that is said is broadly applicable to generalized situations in which fieldworkers may be in physical danger from human sources (i.e., research participants, authorities, and others).[2]

## Setting

In 1981–2, I conducted research in Divis Flats, a Catholic-nationalist ghetto on the lower Falls Road in Belfast. This research was based on participant observation and interviews with seventy-six families, and the monograph emerging from this was a study of the social dynamics of popular support for the Irish Republican Army (IRA) and Irish National Liberation Army (INLA) (Sluka 1989).[3] After nearly a decade, in July 1991, I returned to the Catholic-nationalist ghettos of Belfast for six months of fieldwork on "aspects of political culture in Northern Ireland." These "urban village" ghetto communities represent the major battlegrounds or "killing fields" of the war in Northern Ireland. For over twenty-three years, the residents of the Catholic-nationalist ghettos have been caught between the urban guerrilla warfare of the IRA and INLA and the counterinsurgency operations of the Security Forces. Since the beginning of the war in 1969, the British authorities have sought to contain repression and resistance within the Catholic ghettos of Belfast, Derry, Newry, and other towns and cities and in the rural border areas where Catholics are the majority population (e.g., the so-called bandit country of south Armagh around Crossmaglen) (Rolston 1991). Counterinsurgency operations and the "dirty war" apparatus (Dillon 1990; Faligot 1983), coupled with the activities of pro-government death squads and sectarian attacks by Loyalist extremists, have created an unpredictable deployment of terror concentrated in these communities, with the result that every family or household can tell you about a relative, neighbor, or friend in jail or killed by the Security Forces or Loyalists. The Catholic ghettos are "killing fields" in the sense that they represent the major sites of violence, the battlegrounds where domination and resistance in general and the war in particular are concentrated, contained, and isolated. They are spaces of violence, death, and transformation that continually generate both recruits to the Republican paramilitaries and enough popular support and sympathy among the rest of the people to maintain the current struggle.

When I returned to Belfast in 1991, two things had occurred in the interim that directly affected this research. First, in 1986, a bloody internal feud split the INLA and led to the formation of a new, breakaway paramilitary organization calling itself the Irish Peoples Liberation Organization (IPLO). Second, in 1989, my book on popular support for the IRA and INLA in Divis Flats was published. I sent a copy of the book to friends in Belfast, who subsequently lent it to a number of people to read, including senior Republican activists. Because of the close association of the INLA, and now IPLO, with Divis Flats, I had contacts who advised me that if I ever returned to Belfast and wanted to meet the "High Command" of the IPLO, this could be arranged. Because this research was based on sabbatical leave, I was able to pursue an attempt at participatory research. I wanted to return to Belfast before I decided exactly what research I would engage in, and I wanted to find a subject that offered to be of mutual advantage to me and to the local community. I hoped local people in Belfast might suggest such a research topic.

When I arrived in Belfast I was introduced by friends, in a local pub, to, first, Martin "Rook" O'Prey, the local Belfast commander of the IPLO, and a few days later, the overall commander, Jimmy Brown. (I name these people here because they are now deceased and are publicly recognized as having held these positions in the IPLO at the time of their deaths.)[4] Brown told me that he had read my book and thought it was very good. He then shocked me by asking if I would be interested in writing a book about the IPLO. I said that might be possible, if we could agree on the precise conditions and expectations involved, and we arranged to meet a week later to talk about it. A few days before this meeting could take place, Rook O'Prey, who I had only just met, was assassinated by a Loyalist death squad in his home, one of the new houses built as part of redevelopment, at the bottom of Divis Tower and in plain sight of the army observation post on top. The meeting was postponed a week, and then I met with Brown and the new commander for Belfast.

These IPLO leaders told me that if I was interested in writing a book "like the first one" about their organization, they would "open all the doors" I needed to gather the information. The first thing I discussed with them was, did they know what an anthropologist was and understand what I would be doing? I said that as a social scientist I was committed to objectivity – that is, letting the evidence lead to the conclusions – and the politics of truth. I was willing to write an ethnography of the IPLO if I was allowed academic independence and freedom to write the truth as I saw it, as a result of my own research. The IPLO leaders agreed to this. They knew what an anthropologist was and wanted me to act as one because they believed that an independent academic study would carry more authority and could not be easily dismissed as propaganda. They said they would like to see a book that presented an inside or participant's view of the IPLO. They admitted that they hoped the book would do two things for them. First, because the IPLO lacked effective means of publicizing their perspective – for example, nothing like the weekly paper, *An Phoblacht/Republican News*, which presents the perspective of the Provisional Republican movement (Sinn Fein and the IRA) – the book would be a chance to present their perspective to the world and describe who they were, what they were doing, and why they were doing it. Second, the book would humanize the people in the IPLO, which might serve as a partial antidote to the concerted propaganda campaign by the British authorities aimed at vilifying and dehumanizing them as an aspect of their counterinsurgency or "psychological warfare" operations.

The conditions we agreed on were that I would be allowed to talk to or interview any member I wished, and I could ask questions freely. The interviews would be completely open, and I was not required to submit a list of the questions or subjected to any other apparent monitoring or control practice. During the course of the research, I was never refused an answer to any question. (As with my previous research, I voluntarily chose not to ask about some things such as weapons, finance, and planned military operations, which I felt were unnecessary and potentially dangerous both to me and to other research participants.) I was free to do my anthropological "thing," with only two conditions. I promised that I would allow the IPLO to review the manuscript of the book before it was published. They would not have editorial control, but I agreed to two things. First, I would alter anything in the manuscript necessary to ensure the immediate security of any living member of the IPLO, for example, to protect anonymity of the research participants. Second, if there was anything else in the manuscript that we disagreed on, I would give them a right of response. That is, while I would not modify or delete my own independent conclusions and interpretations, I would include IPLO statements expressing their disagreement with my views wherever they felt it necessary. I thought this was fair, since it would leave readers of the book to judge for themselves whose view they gave more credence to. This struck me as an equitable and reciprocal research "bargain," in which both the researcher and participants stood to benefit, and believed that it did not compromise my professional ethics. It also represented, to the best of my knowledge, the most direct, open, and unimpeded access any researcher has yet been granted to a paramilitary organization in Northern Ireland. I decided to accept the IPLO's offer and approach the research as an experiment in liberation anthropology.

Over the next six months I researched and did fieldwork with the IPLO. This was based on interviews, library and archival research, and participant observation – as far as I thought this was practicable in the circumstances. I conducted formal interviews with fifteen members of the IPLO, selected to provide a representative cross-sample; I conducted interviews in Catholic-nationalist ghettos in Belfast, Derry, Newry, and Dublin, with men and women of all ranks and including both founding and new members. I spent considerable time "hanging out," traveling with, and talking informally with about two dozen other IPLO members and attended a number of IPLO-related social and political functions, such as the funeral and other events surrounding the death of Rook O'Prey.

With regard to the dangers inherent in such fieldwork, I handled or "managed" these much as I had during my first period of research in Belfast (see Sluka 1990). But this new research involved much more direct and intense interaction with guerrilla fighters than I had had in 1981–2, and it presented new problems and dangers. Because members of the IPLO are actively involved in a war, their lives are dangerous, and it is dangerous simply being with them. As with the IRA and INLA in my previous research, I never felt that I was in any danger from the IPLO. As before, my major concern was the authorities – particularly the army and police – and Loyalist paramilitaries, both of whom I believed represented more of a threat now than before because now I was directly researching a guerrilla organization. In particular, the fieldwork period was marked by a major increase in Loyalist violence, from which academics were not immune. In September 1991, Adrian Guelke, a lecturer in politics at the Queen's University of Belfast, was shot by a Loyalist death squad from the Ulster Freedom Fighters (UFF, generally acknowledged as a nom de guerre or front for the Ulster Defence Association). In the early hours of the morning, two or three masked gunmen entered his house and he was shot in the back at close range with a pistol as he lay sleeping with his wife. His life was saved when the automatic pistol used in the attack jammed. Guelke is a South African-born opponent of apartheid who has lived in Northern Ireland since 1974. A distinguished academic, he had no connections with any paramilitary or political groups and was working on a book comparing political violence in South Africa, Israel, and Northern Ireland. The motives for the attack are not clear, and the Security Forces claim it was a case of mistaken identity, but Guelke believes that South African elements, who have links with the Loyalist paramilitaries, may have set him up.[5]

I tried to ameliorate these dangers in two ways. First, I tried to camouflage my research with the IPLO as best I could so that only they and a couple of close and trusted friends knew that I was doing it. I did research on two other projects at the same time (one on Republican martyrs and the other on the cultures of terror and resistance in Northern Ireland), and when asked about my research, I talked about these instead. Second, I tried to control and limit my contact with IPLO members. They were not the only people I spent my time with. In fact, most of my time was not spent with IPLO members, particularly in the first few months of the fieldwork, when I worked on other projects and did library and archival research on the history of the IPLO. Restricting interaction with research participants is not ideal in participant observation, but I believed it necessary for security reasons. As the research progressed, I spent increasingly more time with IPLO members, and the most intense period of interviewing and participant observation occurred during the last two months.

In the end, during the course of this fieldwork, I was not directly threatened in any way. While I was stopped by Security Forces patrols for identity checks twice while in the company of IPLO leaders, I was not approached by the army or police, and they never indicated to me that they were aware that I was conducting research with the IPLO, When asked by soldiers and police about my research, I told them I was studying political culture and did not mention the IPLO. On one occasion I crossed the border illegally with Jimmy Brown on a trip to Dublin. When I told him I was driving to Dublin with Jimmy, a trusted friend warned me that under no circumstances should I use the back roads to avoid the border checkpoints. He warned that if I ran into a Security Forces patrol or SAS unit (the elite commando forces of the British army) on a deserted back road while alone in a car with the commander of the IPLO, I was likely to be shot dead. When

Jimmy and I reached the border, I followed his directions and we used back roads to cross and avoid the checkpoint. I did so because I believed that he was in the best position to decide on the route we should take. I only did this once, and I probably would not do it again.

On one occasion I was participant observer during an IPLO operation, a propaganda exercise. This was a photo-shoot for publicity purposes, and an IPLO photographer was present. I was invited along because they figured I could take some photos of my own for the book. Six armed IPLO members in military uniform and wearing masks, dark glasses, and IPLO armbands emerged in Poleglass (a Catholic-nationalist ghetto on the outskirts of West Belfast) to set up a vehicle checkpoint. They stopped about half a dozen cars and then disappeared. The operation lasted only a few minutes but was probably the most dangerous thing I have ever done as an anthropologist. Armed guerrillas are usually shot on sight in Northern Ireland. For the IPLO to emerge in public in this way is to enter a combat situation. Because of the constant surveillance and patrolling of these districts by the Security Forces, such operations have to be planned very carefully, and there is always a distinct danger of encountering an army patrol or undercover unit or being observed by the surveillance helicopters constantly hovering overhead.

Another new problem I faced resulted from the fact that I taped the fifteen formal interviews. In my previous research I had not taped interviews, so the necessity of protecting tapes was a new experience. I tried to protect these in two ways. First, I tried to ensure there was nothing on them that would directly identify any individual, particularly the interviewee. Second, I hid them away from the house where I lived, so there was never more than one there at a time. Of course, these were not foolproof protections. I felt justified in making the tapes because I had formal agreements with the IPLO as an organization and the individuals interviewed that I would try to use IPLO members' own words to present their views in the book. Because they were willing to accept the risk, and I believed I could protect the tapes and minimize that risk, I taped the interviews.

## Reflections and Recommendations

What then are my recommendations to anthropologists considering fieldwork in dangerous or violent social contexts? Before you go to the field, try to evaluate as realistically as possible the degree of danger, and try to identify potential sources of danger. Decide if you are prepared to accept the risks involved, and if you are, consider both what sorts of actions you might take to ameliorate or manage them and what sorts of actions might exacerbate them. Give some thought to what an "acceptable level" of danger might be. I assume that most researchers are not prepared to give their lives for their research and would retreat to safer ground if a direct threat to life or limb arose. Recognize as well that you may have to terminate your research on your own initiative, or that the authorities or other "powers that be" may compel you to do so. Always have a plan of escape, a means of extricating yourself from the situation as quickly as possible, should the need arise.

Discuss the potential dangers with advisers and colleagues, and seek out people with direct experience in the area where you intend to do your research. If at all possible, try to go to the proposed field location for an exploratory visit before you commit yourself to doing research there. I was able to visit Belfast for two weeks during the summer prior to my arrival there for fieldwork.

Investigate your sources of funding. For example, Myron Glazer (1972:137), a sociologist who studied student politics in Chile, learned only after his return from the field that his funding came from a US Army-sponsored research group. Today, governments, militaries, and intelligence agencies are funding research both directly and indirectly through front organizations (e.g., right-wing think tanks). Ethical considerations aside, it can be dangerous to accept funding from agencies that one's research participants consider objectionable. Certainly, the danger of being defined as a spy is greatly exaggerated if one is funded by the military or the CIA. Know the origins of your funding, consider how people in your research area might view those origins, and be open with them about it.

Given that the people among whom anthropologists do their research have usually never had an anthropologist working in their midst, it should be kept in mind that they are naturally going to try to figure out what you are doing there. Usually, at least at first, they will define the anthropologist with reference to preexisting categories derived from experience with other strangers who have appeared in the community. Spy, journalist, policeman, tax collector, and missionary are common categories often mistakenly applied to anthropologists in the field. It is essential that researchers in the field make a substantial effort to counter these public definitions of themselves, a process entailing a conscious effort at impression management (Berreman 1962; Goffman 1959). It can be done by recognizing how people are likely to define you, avoiding acting in ways that might reinforce these suspicions, and being as honest and straightforward as possible about who you really are and what you are really doing.

Because the most common suspicion that research participants have about anthropologists is that they are spies, and it is difficult to find an anthropologist who has done fieldwork who has not encountered this suspicion, this danger deserves special mention. Being defined as a spy is inherently dangerous, and the link between anthropology and war-related research has exacerbated this danger (see Sluka 1990). Anthropologists have been involved in war-related, particularly counterinsurgency, research, others have had their research used or "applied" by governments, militaries, and intelligence agencies to help plan military operations, and spies or intelligence agents of various sorts have used the cover that they were anthropologists. As a result, people in many parts of the world have come increasingly to believe that anthropologists, even those engaged in "innocent" (or in Boas's [1973] terms "honest") research, are actually or potentially dangerous to them. Many nations and peoples are therefore justifiably suspicious of anthropologists and will not allow them to do research, and fieldwork has become more dangerous today than it was in the past.

If you do not want to be defined as a spy, then do not be one or act like one. (See Glazer 1970 for a good account of dealing with research participants' suspicions that the researcher is a spy.) At first, I avoided asking questions about sensitive political topics. In a similar manner, anthropologists seeking to counter suspicions that they are missionaries would at first avoid asking questions about religion. The sociologist Ned Polsky (1967:126–7) suggests that a good rule of fieldwork in sensitive contexts is to "initially, keep your eyes and ears open but keep your mouth shut. At first try to ask no questions whatsoever. Before you can ask questions . . . you should get the 'feel' of their world by extensive and attentive listening."

When you consider how your research participants are likely to define you, consider ways of not only countering these definitions but of also promoting one that will enhance your safety and your research. It is not enough to not be a threat to your research participants; act in such a way as to *be seen* not to be a threat. In my case, my association with the priest and the former IRA man was fortunate in this respect because once they accepted my explanations of what I was doing in Belfast, others found it easier to do so as well. Polsky (ibid., 129) refers to this cumulative effect as "snowballing"; "get an introduction to one [informant] who will vouch for you with others, who in turn will vouch for you with still others." He suggests that it is best to start at the top, with the most prestigious person in the group you are studying. He also suggests that answering research participants' questions frankly will help in this regard (ibid., 131). I suggest that it is important to give people as honest and complete a description of what you are doing as you can, particularly when they specifically ask for such an explanation.

However, people will develop their own explanations of what researchers are doing, and these are often much-simplified versions of the explanations given by the researchers themselves. It is very common for research participants to reduce the sometimes quite involved explanations given by researchers simply to the explanation that they are "writing a book" about the community or some aspect of it. For example, this was both

William Foote Whyte's (1943:300) and my own experience. It is important to bear in mind that people may reduce your best and most complete explanations to much simpler, less accurate, and perhaps inaccurate ones. It can also be dangerous to give simplified explanations of what you are doing. For example, if you simply tell people that you are writing a book about them, when they learn specific details of what you are writing about, they may believe that you have misled them. They will naturally wonder why you would want to do so and wonder if you have some ulterior motive. Be honest and give as complete and accurate a description of what you are doing as you can, but recognize that people are going to interpret and possibly misinterpret this. Continuously monitor their definitions of you, as these may change over time, and view your efforts at impression management as an ongoing process.

That you should approach this as a conscious effort at impression management is not to suggest that this should be some sort of cold-blooded Machiavellian manipulative strategy. Like Polsky, I argue that it is important to be honest with people. This is imperative both ethically and with reference to managing danger. Being dishonest is more dangerous than being honest, because it creates the possibility of being caught out in a lie. By extension, acting ethically is also safer than acting unethically.[6] Be as honest and ethical as possible, bearing in mind that it is your research participants' definition of these things that you should seek to conform to rather than your own. Of course, this may raise other dangers, for example, when the definitions of what is ethical differ between the group studied and other groups in society, particularly between the group studied and the authorities.

Being honest is relatively simple as long as you have nothing to hide. This was not a problem during my first period of fieldwork, but it became one during the second period because I needed to camouflage my research with the IPLO. I told the authorities vaguely that I was studying aspects of political culture; the IPLO and a few trusted friends knew I was working with the IPLO; and I told everyone else I was doing research on Republican martyrs and the cultures of terror and resistance. In most fieldwork situations today, marked as they are by conflict, it will probably prove to be impossible to be completely truthful with everyone. Nonetheless, it is a good danger management strategy to be as truthful as possible.

Along with honesty, flexibility can be important in danger management. Consider how far you are prepared to modify your interests, methods, and goals to adapt to dangerous contingencies that may arise. Doing research in a dangerous environment may produce situations in which researchers must consider modifying or perhaps even compromising their work. These are difficult decisions to make, and they may be fateful both for the research and for the researcher. Polsky discusses flexibility, summing it up in the comment that "a final rule is to have few unbreakable rules." He points out that you should revise your plans "according to the requirements of any particular situation" and recognize that you will probably encounter "unanticipated and ambiguous situations for which one has no clear behavioral plan at all" (1967:133).

While in the field, take precautions to secure your field notes and recordings. To do so is, of course, required to protect your research participants, but it may also be necessary to protect yourself. This issue is discussed by Jenkins (1984), who suggests that one should be selective in information gathering. He points out that some information should not be used at all and recommends that information of this sort should not be recorded. Some information is best kept only in one's head. When sensitive information is recorded, it is imperative to protect research participants' anonymity. Jenkins (ibid., 156) suggests that one carry around only the current day's notes, and it is probably advisable in some cases that you never have more than a few weeks' notes in your possession at any time while in the field. Your notes can be kept under lock and key, and arrangements can be made to periodically remove them from the field (perhaps by mailing them off or by depositing them in a safe deposit box).

Consider the possibility that some dangers may not end once you return from the field. There may be those at home who object to your research, and they may threaten you as well. (For example, I have been threatened by Loyalists since I left Belfast.) Also, consider the possibility that ethical and other considerations may mean that you will not be able to publish your findings.

If you intend to do research on political topics, particularly if you intend to do "partisan anthropology" or participate in political activities, it goes without saying that the dangers are correspondingly greater.[7] In reference to "partisan anthropology," the Association of Social Anthropologists' book on ethnographic research notes that "siding with a guerrilla movement . . . can be dangerous to oneself as well as to one's objectivity" (Ellen 1984:80). It is interesting to note that the usual concern is not that such involvement may be dangerous but rather that it may not be "objective." It should be kept in mind that one does not actually have to be a member or supporter of a political organization to be at risk from their enemies. Association, even if purely "objective," can be dangerous in itself. In some cases the status of an outsider or "objective scientific observer" provides a degree of protection, but do not count on it. And if you are actually a participant, your status as a social scientist will probably offer you no protection at all.

One might think that neutrality is a good danger management strategy, but this is not always the case. For example, June Nash, in what is perhaps the best account by an anthropologist of managing dangers encountered while conducting fieldwork in a politically sensitive environment, notes,

> In Bolivia it was not possible to choose the role of an impartial observer and still work in the tin mining community of Oruro, where I had gone to study ideology and social change. . . . The polarisation of the class struggle made it necessary to take sides or to be cast by them on one side or the other. In a revolutionary situation, no neutrals are allowed. (1976:150)

By contrast, Frances Henry discusses research in a situation of conflict between the government and trade unions in Trinidad. She notes,

> Commitment to the unions . . . could conceivably have led to loss of freedom, detention, or, at the very least, deportation. . . . On the other hand, commitment to the government could have resulted in loss of rapport with union officials. Identification with either faction can lead to serious personal difficulties and it obviously limits one's research freedom. (1966:553)

Henry was able to establish rapport with both sides and discusses how she got around attempts to get her to abandon her neutrality. Basically, she did so by expressing "sympathy or agreement with persons on both sides" (ibid.). In face-to-face interactions with her research participants, she expressed sympathy with them, even though they had conflicting points of view. While Henry maintains that in fact she was "neutral," this was not the image she presented to her research participants. Rather, she misled her informants by presenting an image of being on their side when she knew that she was not. Besides the ethical questions this raises, Henry admits that this was dangerous, and I would not recommend it.

In some cases, professing neutrality may be a good danger management strategy; in others, it may not be. In some cases, you may want to tell some people that you are neutral and others that you are not. It may sound like a case of "situational ethics," but I had no qualms about telling British soldiers on the streets of Belfast who inquired as to my personal politics that I was a "neutral social scientist" while at the same time letting my research participants in the ghettos know that I sympathized with their situation.

When conducting research based on participant observation in communities involved in political conflicts, it is generally the case that, as Nash, myself, and many others have found, "no neutrals are allowed." As Glazer (1970:314) notes, "In times of heightened group antagonism there is little room for neutrality." This does not necessarily mean that you have to become a partisan. In my case, it was sufficient to communicate in various ways to people where my "sympathies" lay; that is, with them. Whether or not you take sides, those actively involved in the situation are going to define whose side they think you are on. They will act toward you on the basis of this definition, regardless of your professions of neutrality.

Gerrit Huizer (1973), a social psychologist who has done fieldwork in several Latin American countries, including El Salvador and Chile, provides illumination here. When he worked in a village in El Salvador, government officials often warned him of the "dangers" of living among the peasants. Despite these warnings, he chose not to carry a pistol like government officials did. Instead, he "relied mostly on the common human sympathy" he felt for the villagers. Basically, Huizer's approach to handling danger is to gain people's confidence by convincing them that you are "on their side." This is done by sincerely identifying with their interests, understanding and sympathizing with their problems and grievances, and showing them that you are willing to act accordingly (ibid., 21, 28). I think that this is quite the most common approach taken by anthropologists today, and it can be very effective as a danger-ameliorating approach to fieldwork.

When working in a community in which guerrilla organizations are present, you must learn to walk softly. Be sensitive to what sorts of questions may be asked and what sorts are taboo. For example, I found that it was all right to ask people what they thought of the IRA and INLA, if they did or did not support them and why, about the role the guerrillas played in the community, and about criticisms they had concerning them. But I did not ask questions about things like arms and explosives, or about who might be a guerrilla or actively involved with them. If you want to make direct contact with guerrillas, it is best to make it known that you are interested in this and then wait until they come (or do not come) to you. If you do make contact (which is illegal in most cases), you must be flexible and honest with them.

In situations in which insurgencies are going on, fieldworkers may have to deal with both the insurgents and the authorities combating them at the same time and this can be a very difficult task. Often, if you become associated with one, this alienates you from the other. In many field situations the authorities represent a significant source of danger. This warning is particularly true if you are studying or involved with political organizations. For example, Arnold Ap, an anthropologist in West Papua, was tortured and killed by the Indonesian army in 1984, as a result of his association with the Free Papua Movement (OPM). The army claimed that he was "a known OPM helper" (Osborne 1985:xiv). And in 1980, Miriam Daly, a lecturer at the Queen's University of Belfast, was assassinated, probably by intelligence agents, as a result of her involvement with the Irish Republican Socialist party (Faligot 1983:98).

Just as I found in my research, Polsky found that most of the risk in his fieldwork came from the authorities rather than from his research participants. He notes that "most of the danger for the fieldworker comes not from the cannibals and head-hunters but from the colonial officials" (1967:145). In his particular case, he found that most of the risk came from the police rather than from the "career criminals" that were his research participants.

> The criminologist studying uncaught criminals in the open finds sooner or later that law enforcers try to put him on the spot – because, unless he is a complete fool, he uncovers information that law enforcers would like to know, and, even if he is very skillful, he cannot always keep law enforcers from suspecting that he has such information. (Ibid.)

The dangers emanating from the authorities include the risks of intimidation, physical assaults, arrest, interrogation, torture, prosecution, imprisonment, and even execution or assassination. Other dangers include being defined as a guerrilla "sympathizer" or being accused of "giving aid and comfort to the enemy," as a result of which the authorities may revoke their permission for the research. These dangers should be recognized, and efforts should be made to reduce them. (See Carey 1972 for a good discussion of the legal risks faced by researchers in situations in which illegal activities occur.)

An associated phenomenon that can also generate danger is the fact that "people tend to associate the research that a researcher is conducting with the researcher himself" (Henslin 1972:55). As Henslin points out, if you do research on drug users or homosexuality, you may fall under suspicion of being a drug user or homosexual yourself. If you do research on a political movement, some, particularly those opposed to that movement, may believe that you are a partisan. The more political or controversial a subject one researches, the more likely one is to be suspected of bias or partisanship.

While you are in the field, do not grow complacent about the dangers you face, and do not treat the situation as a game or adventure. Do not ignore potential threats when they arise: they rarely just "go away" if you ignore them. For example, dangerous rumors may emerge at almost any time while in the field. Whether these rumors are true or false, they should be dealt with. If they are false, they should be publicly denied rather than ignored. If there is some truth to the rumors, work to convince people that you are not a threat, and if you are a direct threat, get out. James T. Carey (1972:86–7) discusses "handling damaging rumors" in fieldwork and makes some useful suggestions. Try to anticipate the circumstances under which dangerous or damaging rumors are likely to arise, and then limit your actual observations of activities and situations (e.g., illegal ones) that might lead to these circumstances. If and when such rumors do arise, try to get people who have vouched for you in the past to do so again.

Make a continuing effort to define and redefine risks and dangers in light of actual experiences, and work to reduce such dangers by improving old methods and developing new ones as your network of contacts and degree of experience expand over time. Managing the dangers inherent in fieldwork in a context like that of Belfast is not something that can be gotten out of the way in the first few weeks in the field and then dismissed as taken care of. On the last day of my first period of research in Belfast, I was returning home to pack and found the street cordoned off by the army. They would not allow me to go down the street to my house because of the presence of a "suspect device." I argued with a sergeant about it, and he finally said in disgust that I could go to the house if I was prepared to take responsibility for the risk. It turned out that local children had taped some wire to a can of paint and rolled it under an army Land Rover as a prank.

With time, you may be able to successfully allay suspicions and reduce some of the dangers, but new ones will continue to arise. One need not be paranoid about the dangers involved in doing research in violent social contexts, but a good dose of realistic appreciation goes a long way. And, all in all, it is no doubt better to be a bit paranoid about such things than it is to be a bit complacent about them.

Finally, remember that while most dangers can be mediated at least to some degree by skillful maneuver, some dangers may be beyond management. For example, despite your best efforts at danger management, simple bad luck can sometimes result in the termination of the research, or worse yet the termination of the researcher. Researchers working in dangerous environments should, like professional gamblers, recognize that their enterprise is inherently a combination of both skill and luck (Ellen 1984:97). Good luck can sometimes help overcome a lack of skill, and well-developed skills can go far to help overcome the effects of bad luck. But sometimes no amount of skill will save one from a gross portion of bad luck. What distinguishes the professional from the amateur, in both gambling and anthropology, is the concerted effort always to maximize skillful handling of the situation, while recognizing that skill alone is no guarantee of success. Danger is not a purely "technical" problem and is never totally manageable.

It might seem that most of these recommendations amount to little more than common sense. They are by no means exhaustive, but I hope that they are thought provoking or "consciousness raising" and indicative of some of the problems involved in managing danger. They are intended to be a starting point from which those considering research in dangerous contexts can map out their own strategies for conducting fieldwork safely. It should go without saying that counting on people to rely on common sense is a wholly inadequate approach to almost anything. Certainly, it is not adequate for advisers to tell their students that they should use "common sense" while they are in the field, and leave it at that. The example of the anthropologist shot in Belfast is a case in point. Some might say that his mistake was simply that he did not use common sense. My point has been that such an analysis of these cases is an inadequate response.

## Conclusion

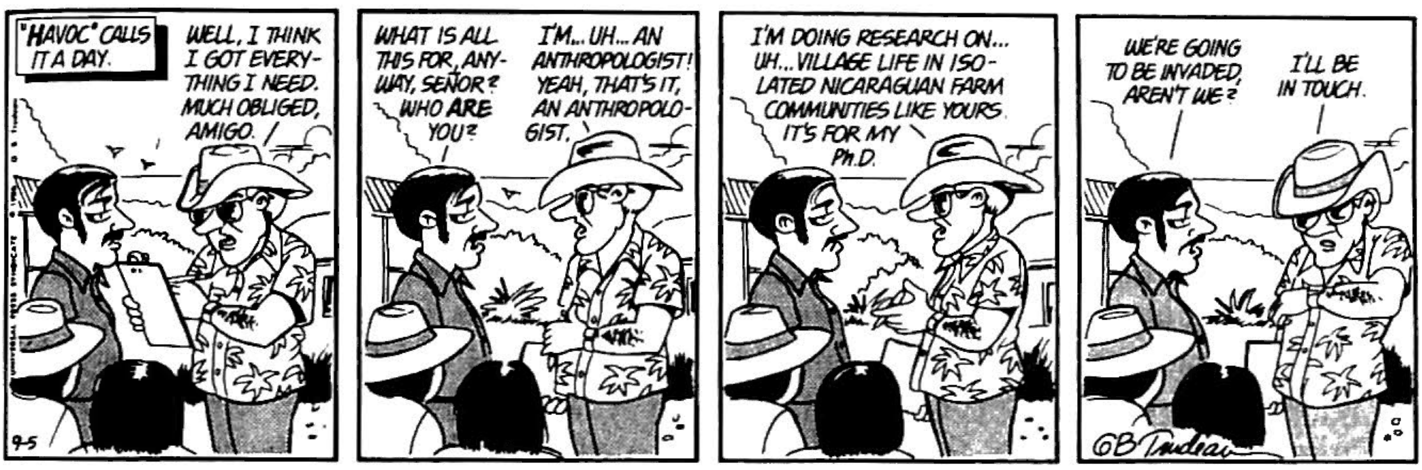
These observations were made in reference to my experience in Northern Ireland, where a guerrilla war has been going on now for more than twenty-three years. As I said at the beginning, there are about 120 armed conflicts in the world today. There is an urgent need for research in all of the places where these conflicts are occurring, and many other violent or dangerous locations as well.

Fieldwork is possible even in the most dangerous contexts. Anthropologists should not select themselves out of research in such contexts on the basis of stereotypes, media images, or inadequate information concerning the dangers involved. And they should not select themselves out of such research because training in managing such dangers is not provided in anthropology. Many more anthropologists could and should do fieldwork in these areas. The dangers are often exaggerated, and in most cases they are not insurmountable.

The world is not becoming a safer place for the pursuit of anthropological fieldwork, but, perhaps for that very reason, there is more need now for such research than there has ever been before. We can meet this challenge, but we should do so rationally by considering the dangers as methodological issues in their own right. The intention of this chapter has been to further our consideration of danger as a methodological issue and contribute to developing ways of minimizing risks and protecting anthropologists while they are in the field. It is not an exaggeration to say that this is, in fact, a matter of life and death.

# Part VI
# Fieldwork Ethics

## Jeffrey A. Sluka




The relational nature of fieldwork addressed in Part III, the conflicts described in Part IV, and the changing relation between anthropologists and their research participants referred to in Part V have made contemporary fieldwork morally and ethically more complex. A grasp of the ethical implications of fieldwork is indispensable before students can develop their first research design. Part VI of this reader deals with ethics in fieldwork. In making the selections for this part, many difficult choices had to be made. We could have simply chosen exemplary case studies from the growing literature on ethical dilemmas faced by anthropologists in the field (e.g., Rynkiewich and Spradley 1976, Appell 1978, Ellen 1984, Cassell and Jacobs 1987, Fluehr-Lobban 2003b, Caplan 2003, Meskell and Pels 2005), or included something on the Tierney/Chagnon "Darkness in El Dorado" debate, which has been described as the greatest ethical debacle in the history of the discipline (see Tierney 2000, Borofsky 2005). In the end, we have chosen to focus more generally on how the concern with ethics emerged and has evolved in the discipline – that is, on the context of ethical thinking rather than particular case studies. Thus, we have chosen articles by Irving Horowitz, Philippe Bourgois, and Gerald Berreman, which provide the general history, context, and critique of the evolution of ethical codes in anthropology, Pollock's case study of providing amateur medical care, which has been one of the classic or "traditional" ethical dilemmas in anthropological fieldwork, and

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finally we have included a copy of the Code of Ethics of the American Anthropological Association (AAA) as it has evolved today (American Anthropological Association, 1998; for the British code of ethics see Association of Social Anthropologists 1999).

In the late 1960s and early 1970s, anthropologists first began seriously discussing the ethical dilemmas faced by fieldworkers studying and living in a world rife with political turmoil, and the first code of ethics in anthropology emerged as a direct result of the protest against counterinsurgency research for the US government in Latin America and Southeast Asia. Leading anthropologists denounced the collaboration of anthropology with the counterinsurgency agencies of the US government – specifically Project Camelot in Latin America, Project Agile in Thailand, and the Himalayan Border Countries Project in India. In particular, the issues raised by Project Camelot and the publication of the Thailand counterinsurgency research (Wolf and Jorgensen 1970) launched the debate on ethics in anthropology, revealed the pressing need to set ethical standards within the profession, and led to the development and adoption of the American Anthropological Association's "Principles of Professional Responsibility" (PPR) in 1971 (Fluehr-Lobban 2003a).

In the 1990s, there was renewed vigor in discussion and debate about ethics in anthropology, which led to the adoption of a new – critics such as Gerald Berreman would say eviscerated – code of ethics by the American Anthropological Association in 1998. The impetus for this change was a general trend in the discipline toward "professionalism" and the changing employment market for anthropologists, with an increasing number employed outside of academia in the private sector. Peter Pels (1999) has identified professionalization as a dominant trend in contemporary anthropology. He argues that the purpose of professional ethics codes is to guarantee the technical and moral quality of service rendered to clients and help discipline members of the profession who fail to maintain what in corporate-speak would be termed "quality control." Professionalism seems to mean redefining anthropology as a form of employment like any other – simply a marketable research skill or commodity for sale in the marketplace. Thus, professionalization and corporatization are the same thing, and the new AAA code of ethics reflects these developments. This is a significant change, because the primary purpose of the original ethics code was first and foremost the protection of research participants from harm. The shift toward professionalism subverts this, with the result that harm or negative consequences to research participants are ameliorated by "competing" ethical obligations and responsibilities to employers and authorities, now referred to in corporate-speak as "clients" or "shareholders," whose interests are treated as equal to those of the people researched.

Pels (1999) observes that, in defining their professional interests and duties, anthropologists are now wavering between responsibility to the people researched, on the one hand, and service to those who fund research and the authorities under whose jurisdiction those researched live, on the other. We now appear to be confused about who our "clients" are – those studied, ourselves, funders and sponsors, the public? And, if all four, do we have equal ethical obligations to them all? In the PPR, there was a hierarchical principle that helped to sort out this conflict – the interests of the research participants were treated as primary, since their cooperation can be obtained only on the basis of trust.

As noted, the first major debate about ethics in anthropology, which eventually led to the adoption of the first code of ethics in the discipline, was sparked by the controversy generated about government-sponsored social science research for military and political purposes, which occurred as a result of Project Camelot. Camelot was a major research project in 1964 into the causes of insurgency and potential revolution in Latin America, sponsored by the US Departments of Defense and State, which was terminated in its early stages as a result of the controversy it generated. In "The Life and Death of Project Camelot" (1973), Horowitz, who also edited a definitive volume on the controversy (1974), describes Project Camelot and recounts the history of the academic reactions to it which eventually led to its cancellation. Horowitz argues that the central issue is the dangers inherent in government-sponsored "mission-oriented" research motivated by political and military interests, especially when they are carried out in other nations, and that at the heart of the problem of Project Camelot is the question "What are and are not the legitimate functions of a scientist?" (1973:147).

The theme of Philippe Bourgois's article "Confronting the Ethics of Anthropology" is "the politics of ethics," and he presents a powerful critique of anthropological research ethics as embodied in the AAA code, based on his fieldwork experience in Central America. He begins by observing that "The ethics of anthropological research are too complicated and important to be reduced to unambiguous absolutes or even perhaps to be clearly defined... The eminently political orientation of a supposed apolitical commitment to empirical research must be appreciated for its internal inconsistencies and ultimate ethical poverty" (1991:110). In 1981, while in El Salvador exploring potential research sites, Bourgois was caught up with local villagers fleeing from a government counterinsurgency "search and destroy" operation that became a peasant massacre. He fled with them, and "for the next fourteen days, we stayed together running at night and hiding during the day" (1991:118). These military operations were supported by the Reagan administration's foreign policy, and when he returned to the United States he sought out the media and human rights lobbyists on Capitol Hill to present his testimony to the public. But most significantly, he published a report critical of US policy, titled "Running for My Life in El Salvador," in the *Washington Post* (February 14, 1982).

The academic reaction was severe criticism of his actions as unethical, and this nearly ended his anthropological career. In a telling footnote, he recounts that "A church-based director of a human rights organization rebuked me when I explained to him the anthropological ethics which prevented me from showing members of US Congress photographs of peasant victims during my testimony on the military invasion: 'For God's sake; what are you talking about! Testify as a human being then – not as an anthropologist'" (1991:123).

Bourgois argues that the problem with contemporary anthropological ethics is that the boundaries of what is defined as ethical are too narrowly drawn and that ethics can be subject to rigid, righteous interpretations which place them at loggerheads with overarching human rights concerns. He asks: What are the limits of informed consent in research involving highly unequal power relations? Is the consent of the powerful required to do research with the powerless, or on the effects of powerlessness? Do we need the consent of repressive authorities in order to do research with those who are oppressed by them? How does one fulfill the obligation to obtain informed consent from the powerful, and respect their privacy and interests? He argues that it is very difficult, if not impossible, to satisfy the discipline-bound code of ethics if one does research on political topics such as marginalization and oppression, and that these unresolved questions reveal "that there is nothing apolitical about the North American commitment to relativism and to its methodologically defined body of ethics. Most dramatically, the ethic of informed consent as it is interpreted by human subjects review boards at... universities implicitly reinforces the political status quo" (1991:120).

Bourgois concludes that "It would be dangerous and arrogant to think that there are definite answers to any of these ethical/moral questions. We need to discuss them and think about them in both practical and theoretical terms" (1991:122–3). It is also notable that this article was published in Faye Harrison's groundbreaking edited volume, *Decolonizing Anthropology*, published in 1991. At that time he could write that "few self-respecting anthropologists would condone the exercise of anthropology at the service of a world superpower or as a complement to espionage" (1991:111). However, as noted in the introduction to Part V, we see that this is being challenged, and there are now anthropologists who *are* prepared to condone this in support of US President Bush's and UK Prime Minister Blair's "war on terror." This has not only exacerbated the dangers and hazards of anthropological fieldwork, but reinforces the criticism that in the 1990s there was a shift to the right in the discipline, marked by the weakening of the AAA code of ethics, which represents a trend toward the "recolonization" of the discipline.

Gerald Berreman has been one of the leading actors and spokespersons involved in the dialogue over ethics during the past four decades. In "Ethics versus 'Realism' in Anthropology" (1991), he presents a strong defense of the preservation of the core features of the original code, the Principles of Professional Responsibility. Berreman was a member of the AAA Committee on Ethics during some of the most turbulent times in the history of the discipline, and was one of the drafters of the original PPR in 1971. Berreman fears a neoliberal return to an immoral and instrumental interventionism. He describes what he sees as the evisceration of the original AAA code of ethics, leading up to the adoption of the new code in 1998, and offers an excellent discussion of the issue of clandestine research, a subject which is of renewed relevance today during a "new era" in which much anthropological research is increasingly subject to contract–client restriction.

Berreman's article is the most important and damning critique of the changes in the AAA code of ethics. These changes were driven by the interests of the growing number of anthropologists employed outside of universities in the private sector, and the new code is partly intended to prevent the division of the profession into "academic" and "practicing" moieties. Berreman decries the shift away from idealism (and all notions of universal human rights, ethics, or morals are, by definition, idealistic) toward self-centered practicality, and he identifies four major removals that represent this shift – that paramount responsibility is to the people studied, the censuring of covert research, the principle of accountability for ethical violations, and the commitment to public duty rather than private interest. Berreman scathingly concludes (1991:52) that omitting these concerns results in

> not a code at all, but a mild statement of interest, and one conspicuously devoid of ethical content. It is in fact, a license for unfettered free-enterprise research, advising and engineering disguised as anthropology, with the intent of employing the ethical reputation of the discipline to enable and facilitate a wide range of mission-oriented activities, including those of dubious ethical and even egregiously unethical nature. (1991:52)

Many anthropologists, but particularly those in the Third World, share Berreman's view that such changes represent an "abject surrender of principle to a misguided practicality, a sacrifice of public interest to misperceived self-interest: replacing ethics with greed" (1991:57). Berreman coins the term "Reaganethics" to describe what he sees as the retreat away from more global, humanistic concerns, embodied in the original code of ethics, toward a greater concern with the protection and preservation of academic careers and reputations.

Finally, in "Healing Dilemmas" (1996), Donald Pollock addresses the ethical dilemmas of "doctoring" in the field, with regard to his fieldwork with the Kulina of western Brazil in the early 1980s. While the practical issues of amateur doctoring are fairly common for anthropologists working in remote areas of the world, and was one of the first ethical issues addressed in the discipline (e.g., McCurdy 1977), there are still few discussions of the ethical issues entailed. Pollock discusses the "dilemma of power" (1996:149) or politics in providing amateur medical care during fieldwork, which "arises for anthropologists when our hosts request or even demand the benefits of western biomedicine, and when we find ourselves coerced, manipulated, or ordered to provide medical assistance" (1996:150).

In considering the ethical issues of "the small-scale, amateur medical work of well-meaning anthropologists" (1996:156), Pollock observes that "in the case of fieldworkers, anthropologists who have few or no technical skills and little knowledge of disease, and who have only the weakest ethical obligation, at best, to treat sick people (and traditionally strong ethical obligations not to impose western culture on indigenous communities), nonetheless plunge right in and play doctor" (1996:150). While anthropologists have generally been reluctant to engage in such practices, they have often felt pressured to do so, not least by their own belief that providing medical assistance is a minimal form of reciprocity – of giving something back or trying to help the community in some immediate and practical way for their consenting to allow the researcher to work with them. Pollock also discusses the ethical dilemma of *not* doctoring in the field, which he argues is partly the consequence of the positivist assumption that anthropologists should maintain the rational and detached researcher's view, "marginal yet informed," observing all around them without becoming involved in or a part of it (1996:154).

Regardless of the ongoing debates and issues surrounding research ethics, today anthropologists increasingly believe that solutions to ethical problems that arise during fieldwork should be found through negotiation. The trend is toward ethical "contracts" or agreements worked out in the field with research participants. While codes of ethics provide useful guidelines, in the field ethical research relationships must be actively, if not creatively, negotiated and adapted to the specifics of the situation or context.

# 19

# The Life and Death of Project Camelot

*Irving Louis Horowitz*

In June of this year [1965] – in the midst of the crisis over the Dominican Republic – the United States Ambassador to Chile sent an urgent and angry cable to the State Department. Ambassador Ralph Dungan was confronted with a growing outburst of anti-Americanism from Chilean newspapers and intellectuals. Further, left-wing members of the Chilean Senate had accused the United States of espionage.

The anti-American attacks that agitated Dungan had no direct connection with sending US troops to Santo Domingo. Their target was a mysterious and cloudy American research program called Project Camelot.

Dungan wanted to know from the State Department what Project Camelot was all about. Further, whatever Camelot was, he wanted it stopped because it was fast becoming a *cause célèbre* in Chile (as it soon would throughout capitals of Latin America and in Washington) and Dungan had not been told anything about it – even though it was sponsored by the US Army and involved the tinderbox subjects of counterrevolution and counterinsurgency in Latin America.

Within a few weeks Project Camelot created repercussions from Capitol Hill to the White House. Senator J. William Fulbright, chairman of the Foreign Relations Committee, registered his personal concern about such projects as Camelot because of their "reactionary, backward-looking policy opposed to change. Implicit in Camelot, as in the concept of 'counterinsurgency,' is an assumption that revolutionary movements are dangerous to the interests of the United States and that the United States must be prepared to assist, if not actually to participate in, measures to repress them."

By mid-June the State Department and Defense Department – which had created and funded Camelot – were in open contention over the project and the jurisdiction each department should have over certain foreign policy operations.

On July 8, Project Camelot was killed by Defense Secretary Robert McNamara's office which has a veto power over the military budget. The decision had been made under the President's direction.

On the same day, the director of Camelot's parent body, the Special Operations Research Organization, told a Congressional committee that the research project on revolution and counterinsurgency had taken its name from King Arthur's mythical domain because "It connotes the right sort of things – development of a stable society with peace and justice for all." Whatever Camelot's outcome, there

Irving Louis Horowitz, "The Life and Death of Project Camelot," pp. 138–48 from Thomas Weaver (ed.), *To See Ourselves: Anthropology and Modern Social Issues* (Glencoe, IL: Scott, Foresman & Co., 1973). Copyright © 1973 Scott, Foresman and Company.

should be no mistaking the deep sincerity behind this appeal for an applied social science pertinent to current policy.

However, Camelot left a horizon of disarray in its wake: an open dispute between State and Defense; fuel for the anti-American fires in Latin America; a cut in US Army research appropriations. In addition, serious and perhaps ominous implications for social science research, bordering on censorship, have been raised by the heated reaction of the executive branch of government.

## Global Counterinsurgency

What was Project Camelot? Basically, it was a project for measuring and forecasting the causes of revolutions and insurgency in underdeveloped areas of the world. It also aimed to find ways of eliminating the causes, or coping with the revolutions and insurgencies. Camelot was sponsored by the US Army on a four to six million dollar contract, spaced out over three to four years, with the Special Operations Research Organization (SORO). This agency is nominally under the aegis of American University in Washington, DC, and does a variety of research for the Army. This includes making analytical surveys of foreign areas; keeping up-to-date information on the military, political, and social complexes of those areas; and maintaining a "rapid response" file for getting immediate information, upon Army request, on any situation deemed militarily important.

Latin America was the first area chosen for concentrated study, but countries on Camelot's four-year list included some in Asia, Africa, and Europe. In a working paper issued on December 5, 1964, at the request of the Office of the Chief of Research and Development, Department of the Army, it was recommended that "comparative historical studies" be made in these countries:

*(Latin America) Argentina, Bolivia, Brazil, Colombia, Cuba, Dominican Republic, El Salvador, Guatemala, Mexico, Paraguay, Peru, Venezuela.*

*(Middle East) Egypt, Iran, Turkey.*

*(Far East) Korea, Indonesia, Malaysia, Thailand.*

*(Others) France, Greece, Nigeria.*

"Survey research and other field studies" were recommended for Bolivia, Colombia, Ecuador, Paraguay, Peru, Venezuela, Iran, and Thailand. Preliminary consideration was also being given to a study of the separatist movement in French Canada. It, too, had a code name: Project Revolt.

In a recruiting letter sent to selected scholars all over the world at the end of 1964, Project Camelot's aims were defined as a study to "make it possible to predict and influence politically significant aspects of social change in the developing nations of the world." This would include devising procedures for "assessing the potential for internal war within national societies" and "identify(ing) with increased degrees of confidence, those actions which a government might take to relieve conditions which are assessed as giving rise to a potential for internal war." The letter further stated: "The US Army has an important mission in the positive and constructive aspects of nation-building in less developed countries as well as a responsibility to assist friendly governments in dealing with active insurgency problems." Such activities by the US Army were described as "insurgency prophylaxis" rather than the "sometimes misleading label of counterinsurgency."

Project Camelot was conceived in late 1963 by a group of high-ranking Army officers connected with the Army Research Office of the Department of Defense. They were concerned about new types of warfare springing up around the world. Revolutions in Cuba and Yemen and insurgency movements in Vietnam and the Congo were a far cry from the battles of World War II and also different from the envisioned – and planned for – apocalypse of nuclear war. For the first time in modern warfare, military establishments were not in a position to use the immense arsenals at their disposal – but were, instead, compelled by force of a geopolitical stalemate to increasingly engage in primitive forms of armed combat. The questions of moment for the Army were: Why can't the "hardware" be used? And what alternatives can social science "software" provide?

A well-known Latin American area specialist, Rex Hopper, was chosen as director of Project Camelot. Hopper was a professor of sociology and chairman of the department at

Brooklyn College. He had been to Latin America many times over a thirty-year span on research projects and lecture tours, including some under government sponsorship. He was highly recommended for the position by his professional associates in Washington and elsewhere. Hopper had a long-standing interest in problems of revolution and saw in this multimillion dollar contract the possible realization of a life-long scientific ambition.

## The Chilean Debacle

How did this social science research project create a foreign policy furore? And, at another level, how did such high intentions result in so disastrous an outcome?

The answers involve a network spreading from a professor of anthropology at the University of Pittsburgh, to a professor of sociology at the University of Oslo, and yet a third professor of sociology at the University of Chile in Santiago, Chile. The "showdown" took place in Chile, first within the confines of the university, next on the floor of the Chilean Senate, then in the popular press of Santiago, and finally, behind US embassy walls.

It was ironic that Chile was the scene of wild newspaper tales of spying and academic outrage at scholars being recruited for "spying missions." For the working papers of Project Camelot stipulated as a criterion for study that a country "should show promise of high payoffs in terms of the kinds of data required." Chile did not meet these requirements – it is not on the preliminary list of nations specified as prospects.

How then did Chile become involved in Project Camelot's affairs? The answer requires consideration of the position of Hugo G. Nutini, assistant professor of anthropology at Pittsburgh, citizen of the United States and former citizen of Chile. His presence in Santiago as a self-identified Camelot representative triggered the climactic chain of events.

Nutini, who inquired about an appointment in Camelot's beginning stages, never was given a regular Camelot appointment. Because he was planning a trip to Chile in April of this year – on other academic business – he was asked to prepare a report concerning possibilities of cooperation from Chilean scholars. In general, it was the kind of survey which has mild results and a modest honorarium attached to it (Nutini was offered $750). But Nutini had an obviously different notion of his role. Despite the limitations and precautions which Rex Hopper placed on his trip, especially Hopper's insistence on its informal nature, Nutini managed to convey the impression of being an official of Project Camelot with the authority to make proposals to prospective Chilean participants. Here was an opportunity to link the country of his birth with the country of his choice.

At about the same time, Johan Galtung, a Norwegian sociologist famous for his research on conflict and conflict resolution in underdeveloped areas, especially in Latin America, entered the picture. Galtung, who was in Chile at the time and associated with the Latin American Faculty of Social Science (FLACSO), received an invitation to participate in a Camelot planning conference scheduled for Washington, DC, in August 1965. The fee to social scientists attending the conference would be $2,000 for four weeks. Galtung turned down the invitation. He gave several reasons. He could not accept the role of the US Army as a sponsoring agent in a study of counterinsurgency. He could not accept the notion of the Army as an agency of national development; he saw the Army as managing conflict and even promoting conflict. Finally, he could not accept the asymmetry of the project – he found it difficult to understand why there would be studies of counterinsurgency in Latin America, but no studies of "counterintervention" (conditions under which Latin American nations might intervene in the affairs of the United States). Galtung was also deeply concerned about the possibility of European scholars being frozen out of Latin American studies by an inundation of sociologists from the United States. Furthermore, he expressed fears that the scale of Camelot honoraria would completely destroy the social science labor market in Latin America.

Galtung had spoken to others in Oslo, Santiago, and throughout Latin America about the project, and he had shown the memorandum of December 1964 to many of his colleagues.

Soon after Nutini arrived in Santiago, he had a conference with Vice-Chancellor Alvaro

Bunster of the University of Chile to discuss the character of Project Camelot. Their second meeting, arranged by the vice-chancellor, was also attended by Professor Eduardo Fuenzalida, a sociologist. After a half-hour of exposition by Nutini, Fuenzalida asked him pointblank to specify the ultimate aims of the project, its sponsors, and its military implications. Before Nutini could reply, Professor Fuenzalida, apparently with some drama, pulled a copy of the December 4 circular letter from his briefcase and read a prepared Spanish translation. Simultaneously, the authorities at FLACSO turned over the matter to their associates in the Chilean Senate and in the left-wing Chilean press.

In Washington, under the political pressures of State Department officials and Congressional reaction, Project Camelot was halted in midstream, or more precisely, before it ever really got under way. When the ambassador's communication reached Washington, there was already considerable official ferment about Project Camelot. Senators Fulbright, Morse, and McCarthy soon asked for hearings by the Senate Foreign Relations Committee. Only an agreement between Secretary of Defense McNamara and Secretary of State Rusk to settle their differences on future overseas research projects forestalled Senate action. But in the House of Representatives, a hearing was conducted by the Foreign Affairs Committee on July 8. The SORO director, Theodore Vallance, was questioned by committee members on the worth of Camelot and the matter of military intrusion into foreign policy areas.

That morning, even before Vallance was sworn in as a witness – and without his knowledge – the Defense Department issued a terse announcement terminating Project Camelot. President Johnson had decided the issue in favor of the State Department. In a memo to Secretary Rusk on August 5 the President stipulated that "no government sponsorship of foreign area research should be undertaken which in the judgment of the Secretary of State would adversely affect United States foreign relations."

The State Department has recently established machinery to screen and judge all federally-financed research projects overseas. The policy and research consequences of the Presidential directive will be discussed later.

What effect will the cancellation of Camelot have on the continuing rivalry between Defense and State departments for primacy in foreign policy? How will government sponsorship of future social science research be affected? And was Project Camelot a scholarly protective cover for US Army planning – or a legitimate research operation on a valid research subject independent of sponsorship?

Let us begin with a collective self-portrait of Camelot as the social scientists who directed the project perceived it. There seems to be general consensus on seven points.

First, the men who went to work for Camelot felt the need for a large-scale, "big picture" project in social science. They wanted to create a sociology of contemporary relevance which would not suffer from the parochial narrowness of vision to which their own professional backgrounds had generally conditioned them. Most of the men viewed Camelot as a bona fide opportunity to do fundamental research with relatively unlimited funds at their disposal. (No social science project ever before had up to $6,000,000 available.) Under such optimal conditions, these scholars tended not to look a gift horse in the mouth. As one of them put it, there was no desire to inquire too deeply as to the source of the funds or the ultimate purpose of the project.

Second, most social scientists affiliated with Camelot felt that there was actually more freedom to do fundamental research under military sponsorship than at a university or college. One man noted that during the 1950s there was far more freedom to do fundamental research in the RAND corporation (an Air Force research organization) than on any campus in America. Indeed, once the protective covering of RAND was adopted, it was almost viewed as a society of Platonist elites or "knowers" permitted to search for truth on behalf of the powerful. In a neoplatonic definition of their situation, the Camelot men hoped that their ideas would be taken seriously by the wielders of power (although, conversely, they were convinced that the armed forces would not accept their preliminary recommendations).

Third, many of the Camelot associates felt distinctly uncomfortable with military sponsorship, especially given the present United States military posture. But their reaction to this discomfort was that "the Army has to be educated." This view was sometimes cast in Freudian terms: the Army's bent toward violence ought to be sublimated. Underlying this theme was the notion of the armed forces as an agency for potential social good – the discipline and the order embodied by an army could be channeled into the process of economic and social development in the United States as well as in Latin America.

Fourth, there was a profound conviction in the perfectibility of mankind; particularly in the possibility of the military establishment performing a major role in the general process of growth. They sought to correct the intellectual paternalism and parochialism under which Pentagon generals, State Department diplomats, and Defense Department planners seemed to operate.

Fifth, a major long-range purpose of Camelot, at least for some of its policy-makers, was to prevent another revolutionary holocaust on a grand scale, such as occurred in Cuba. At the very least, there was a shared belief that *Pax Americana* was severely threatened and its future could be bolstered.

Sixth, none of them viewed their role on the project as spying for the United States government, or for anyone else.

Seventh, the men on Project Camelot felt that they made heavy sacrifices for social science. Their personal and professional risks were much higher than those taken by university academics. Government work, while well-compensated, remains professionally marginal. It can be terminated abruptly (as indeed was the case) and its project directors are subject to a public scrutiny not customary behind the walls of ivy.

In the main, there was perhaps a keener desire on the part of the directing members of Camelot not to "sell out" than there is among social scientists with regular academic appointments. This concern with the ethics of social science research seemed to be due largely to daily confrontation of the problems of betrayal, treason, secrecy, and abuse of data, in a critical situation. In contrast, even though a university position may be created by federally-sponsored research, the connection with policy matters is often too remote to cause any *crise de conscience.*

## The Insiders' Report

Were the men on Camelot critical of any aspects of the project?

Some had doubts from the outset about the character of the work they would be doing, and about the conditions under which it would be done. It was pointed out, for example, that the US Army tends to exercise a far more stringent intellectual control of research findings than does the US Air Force. As evidence for this, it was stated that SORO generally had fewer "free-wheeling" aspects to its research designs than did RAND (the Air Force-supported research organization). One critic inside SORO went so far as to say that he knew of no SORO research which had a "playful" or unregimented quality, such as one finds at RAND (where for example, computers are used to plan invasions but also to play chess). One staff member said thar "the self-conscious seriousness gets to you after a while." "It was all grim stuff," said another.

Another line of criticism was that pressures on the "reformers" (as the men engaged in Camelot research spoke of themselves) to come up with ideas were much stronger than the pressures on the military to actually bring off any policy changes recommended. The social scientists were expected to be social reformers, while the military adjutants were expected to be conservative. It was further felt that the relationship between sponsors and researchers was not one of equals, but rather one of superordinate military needs and subordinate academic role. On the other hand, some officials were impressed by the disinterestedness of the military, and thought that far from exercising undue influence, the Army personnel were loath to offer opinions.

Another objection was that if one had to work on policy matters – if research is to have international ramifications – it might better be conducted under conventional State Department sponsorship. "After all," one man said, "they are at least nominally committed to civilian political norms." In other words, there was a considerable reluctance to believe that the Defense Department, despite its superior organization, greater financial affluence, and executive influence, would actually improve upon State Department styles of work, or accept recommendations at variance with Pentagon policies.

There seemed to be few, if any, expressions of disrespect for the intrinsic merit of the work contemplated by Camelot, or of disdain for policy-oriented work in general. The scholars engaged in the Camelot effort used two distinct vocabularies. The various Camelot documents reveal a military vocabulary provided with an array of military justification; often followed (within the same document) by a social science vocabulary offering social science justifications and rationalizations. The dilemma in the Camelot literature from the preliminary report issued in August 1964 until the more advanced document issued in April 1965, is the same: an incomplete amalgamation of the military and sociological vocabularies. (At an early date the project had the code name SPEARPOINT.)

## Policy Conflicts Over Camelot

The directors of SORO are concerned that the cancellation of Camelot might mean the end of SORO as well in a wholesale slash of research funds. For while over $1,000,000 was allotted to Camelot each year, the annual budget of SORO, its parent organization, is a good deal less. Although no such action has taken place, SORO's future is being examined. For example, the Senate and House Appropriations Committee blocked a move by the Army to transfer unused Camelot funds to SORO.

However, the end of Project Camelot does not necessarily imply the end of the Special Operations Research Office, nor does it imply an end to research designs which are similar in character to Project Camelot. In fact, the termination of the contract does not even imply an intellectual change of heart on the part of the originating sponsors or key figures of the project.

One of the characteristics of Project Camelot was the number of antagonistic forces it set in motion on grounds of strategy and timing rather than from what may be called considerations of scientific principles.

The State Department grounded its opposition to Camelot on the basis of the ultimate authority it has in the area of foreign affairs. There is no published report showing serious criticism of the projected research itself.

Congressional opposition seemed to be generated by a concern not to rock any foreign alliances, especially in Latin America. Again, there was no statement about the project's scientific or intellectual grounds.

A third group of skeptics, academic social scientists, generally thought that Project Camelot, and studies of the processes of revolution and war in general, were better left in the control of major university centers, and in this way, kept free of direct military supervision.

The Army, creator of the project, did nothing to contradict McNamara's order cancelling Project Camelot. Army influentials did not only feel that they had to execute the Defense Department's orders, but they are traditionally dubious of the value of "software" research to support "hardware" systems.

Let us take a closer look at each of these groups which voiced opposition to Project Camelot. A number of issues did not so much hinge upon, as swim about, Project Camelot. In particular, the "jurisdictional" dispute between Defense and State loomed largest.

### *State vs. defense*

In substance, the debate between the Defense Department and the State Department is not unlike that between electricians and bricklayers in the construction of a new apartment house. What "union" is responsible for which processes? Less generously, the issue is: who controls what? At the policy level, Camelot was a tool tossed about in a larger power struggle which has been going on in government circles since the end of World War II, when the Defense Department emerged as a competitor for honors as the most powerful bureau of the administrative branch of government.

In some sense, the divisions between Defense and State are outcomes of the rise of ambiguous conflicts such as Korea and Vietnam, in contrast to the more precise and diplomatically controlled "classical" world wars. What are the lines dividing political policy from military posture? Who is the most important representative of the United States abroad: the ambassador or the military attaché in charge of the military mission? When soldiers from foreign lands are sent to the United States for political orientation, should such orientation be within the province of the State Department or of the Defense Department? When undercover activities are conducted, should the direction of such activities belong to military or political authorities? Each of these is a strategic question with little pragmatic or historic precedent. Each of these was entwined in the Project Camelot explosion.

It should be plain therefore that the State Department was not simply responding to the recommendations of Chilean left-wingers in urging the cancellation of Camelot. It merely employed the Chilean hostility to "interventionist" projects as an opportunity to redefine the balance of forces and power with the Defense Department. What is clear from this resistance to such projects is not so much a defense of the sovereignty of the nations where ambassadors are stationed, as it is a contention that conventional political channels are sufficient to yield the information desired or deemed necessary.

## Congress

In the main, congressional reaction seems to be that Project Camelot was bad because it rocked the diplomatic boat in a sensitive area. Underlying most congressional criticisms is the plain fact that most congressmen are more sympathetic to State Department control of foreign affairs than they are to Defense Department control. In other words, despite military sponsored world junkets, National Guard and State Guard pressures from the home State, and military training in the backgrounds of many congressmen, the sentiment for political rather than military control is greater. In addition, there is a mounting suspicion in Congress of varying kinds of behavioral science research stemming from hearings into such matters as wiretapping, uses of lie detectors, and truth-in-packaging.

## Social scientists

One reason for the violent response to Project Camelot, especially among Latin American scholars, is its sponsorship by the Department of Defense. The fact is that Latin Americans have become quite accustomed to State Department involvements in the internal affairs of various nations. The Defense Department is a newcomer, a dangerous one, inside the Latin American orbit. The train of thought connected to its activities is in terms of international warfare, spying missions, military manipulations, etc. The State Department, for its part, is often a consultative party to shifts in government, and has played an enormous part in either fending off or bringing about *coups d'état*. This State Department role has by now been accepted and even taken for granted. Not so the Defense Department's role. But it is interesting to conjecture on how matter-of-factly Camelot might have been accepted if it had State Department sponsorship.

Social scientists in the United States have, for the most part, been publicly silent on the matter of Camelot. The reasons for this are not hard to find. First, many "giants of the field" are involved in government contract work in one capacity or another. And few souls are in a position to tamper with the gods. Second, most information on Project Camelot has thus far been of a newspaper variety; and professional men are not in a habit of criticizing colleagues on the basis of such information. Third, many social scientists doubtless see nothing wrong or immoral in the Project Camelot designs. And they are therefore more likely to be either confused or angered at the Latin American response than at the directors of Project Camelot. (At the time of the blowup, Camelot people spoke about the "Chilean mess" rather than the "Camelot mess.")

The directors of Project Camelot did not "classify" research materials, so that there would be no stigma of secrecy. And they also tried to hire, and even hired away from academic positions, people well known and respected for their independence of mind. The difficulty is that even though the stigma of secrecy was formally erased, it remained in the attitudes of many of the employees and would-be employees of Project Camelot. They unfortunately thought in terms of secrecy, clearance, missions, and the rest of the professional nonsense that so powerfully afflicts the Washington scientific as well as political ambience.

Further, it is apparent that Project Camelot had much greater difficulty hiring a full-time staff of high professional competence, than in getting part-time, summertime, weekend, and sundry assistance. Few established figures in academic life were willing to surrender the advantages of their positions for the risks of the project.

One of the cloudiest aspects to Project Camelot is the role of American University. Its actual supervision of the contract appears to have begun and ended with the 25 percent overhead on those parts of the contract that a university receives on most federal grants. Thus, while there can be no question as to the "concern and disappointment" of President Hurst R. Anderson of the American University over the demise of Project Camelot, the reasons for this regret do not seem to extend beyond the formal and the financial. No official at American University appears to have been willing to make any statement of responsibility, support, chagrin, opposition, or anything else related to the project. The issues are indeed momentous, and must be faced by all universities at which government sponsored research is conducted: the amount of control a university has over contract work; the role of university officials in the distribution of funds from grants; the relationships that ought to be established once a grant is issued. There is also a major question concerning project directors: are they members of the faculty, and if so, do they have necessary teaching responsibilities and opportunities for tenure as do other faculty members.

The difficulty with American University is that it seems to be remarkably unlike other universities in its permissiveness. The Special Operations Research Office received neither guidance nor support from university officials. From the outset, there seems to have been a "gentleman's agreement" not to inquire or interfere in Project Camelot, but simply to serve as some sort of camouflage. If American University were genuinely autonomous it might have been able to lend highly supportive aid to Project Camelot during the crisis months. As it is, American University maintained an official silence which preserved it from more congressional or executive criticism. This points up some serious flaws in its administrative and financial policies.

The relationship of Camelot to SORO represented a similarly muddled organizational picture. The director of Project Camelot was nominally autonomous and in charge of an organization surpassing in size and importance the overall SORO operation. Yet at the critical point the organizational blueprint served to protect SORO and sacrifice what nominally was its limb. That Camelot happened to be a vital organ may have hurt, especially when Congress blocked the transfer of unused Camelot funds to SORO.

## *Military*

Military reaction to the cancellation of Camelot varied. It should be borne in mind that expenditures on Camelot were minimal in the Army's overall budget and most military leaders are skeptical, to begin with, about the worth of social science research. So there was no open protest about the demise of Camelot. Those officers who have a positive attitude toward social science materials, or are themselves trained in the social sciences, were dismayed. Some had hoped to find "software" alternatives to the "hardware systems" approach applied by the Secretary of Defense to every military-political contingency. These officers saw the attack on Camelot as a double attack – on their role as officers and on their professional standards. But the Army was so clearly treading in new waters that it could scarcely jeopardize the entire structure of military research to preserve one project. This very inability or impotence to preserve Camelot – a situation threatening to other governmental contracts with social scientists – no doubt impressed many armed forces officers.

The claim is made by the Camelot staff (and various military aides) that the critics of the project played into the hands of those sections of the military predisposed to veto any social science recommendations. Then why did the military offer such a huge support to a social science project to begin with? Because $6,000,000 is actually a trifling sum for the Army in an age of multi-billion dollar military establishment. The amount is significantly more important for the social sciences, where such contract awards remain relatively scarce. Thus, there were differing perspectives of the importance of Camelot: an Army view which considered the contract as one of several forms of "software" investment; a social science perception of Project Camelot as the equivalent of the Manhattan Project.

## Was Project Camelot Workable?

While most public opposition to Project Camelot focused on its strategy and timing, a considerable amount of private opposition centered on more basic, though theoretical, questions: was Camelot scientifically feasible and ethically correct? No public document or statement contested the possibility that, given the successful completion of the data gathering, Camelot could have, indeed, established basic criteria for measuring the level and potential for internal war in a given nation. Thus, by never challenging the feasibility of the work, the political critics of Project Camelot were providing back-handed compliments to the efficacy of the project.

But much more than political considerations are involved. It is clear that some of the most critical problems presented by Project Camelot are scientific. Although for an extensive analysis of Camelot, the reader would, in fairness, have to be familiar with all of its documents, salient general criticisms can be made without a full reading.

The research design of Camelot was from the outset plagued by ambiguities. It was never quite settled whether the purpose was to study counterinsurgency possibilities, or the revolutionary process. Similarly, it was difficult to determine whether it was to be a study of comparative social structures, a set of case studies of single nations "in depth," or a study of social structure with particular emphasis on the military. In addition, there was a lack of treatment of what indicators were to be used, and whether a given social system in Nation A could be as stable in Nation B.

In one Camelot document there is a general critique of social science for failing to deal with social conflict and social control. While this in itself is admirable, the tenor and context of Camelot's documents make it plain that a "stable society" is considered the norm no less than the desired outcome. The "breakdown of social order" is spoken of accusatively. Stabilizing agencies in developing areas are presumed to be absent. There is no critique of US Army policy in developing areas because the Army is presumed to be a stabilizing agency. The research formulations always assume the legitimacy of Army tasks – "if the US Army is to perform effectively its parts in the US mission of counterinsurgency it must recognize that insurgency represents a breakdown of social order. . . ." But such a proposition has never been doubted – by Army officials or anyone else. The issue is whether such breakdowns are in the nature of the existing system or a product of conspiratorial movements.

The use of hygienic language disguises the antirevolutionary assumptions under a cloud of powder puff declarations. For example, studies of Paraguay are recommended "because trends in this situation (the Stroessner regime) may also render it unique when analyzed in terms of the transition from 'dictatorship' to political stability." But to speak about changes from dictatorship to stability is an obvious ruse. In this case, it is a tactic to disguise the fact that Paraguay is one of the most vicious, undemocratic (and like most dictatorships, stable) societies in the Western Hemisphere.

These typify the sort of hygienic sociological premises that do not have scientific purposes. They illustrate the confusion of commitments within Project Camelot. Indeed the very absence of emotive words such as revolutionary masses, communism, socialism, and capitalism only serves to intensify the discomfort one must feel on examination of the documents – since the abstract vocabulary disguises, rather than resolves, the problems of international revolution. To have used clearly political rather than military language would not "justify" governmental support. Furthermore, shabby assumptions of academic conventionalism replaced innovative orientations. By adopting a systems approach, the problematic, open-ended aspects of the study of revolutions were largely omitted; and the design of the study became an oppressive curb on the study of the problems inspected.

This points up a critical implication for Camelot (as well as other projects). The importance of the subject being researched does not *per se* determine the importance of the project. A sociology of large-scale relevance and reference is all to the good. It is important that scholars be willing to risk something of their shaky reputations in helping resolve major world social problems. But it is no less urgent that in the process of addressing major problems, the autonomous character of the social science disciplines – their own criteria of worthwhile scholarship – should not be abandoned. Project Camelot lost sight of this "autonomous" social science character.

It never seemed to occur to its personnel to inquire into the desirability for successful revolution. This is just as solid a line of inquiry as the one stressed – the conditions under which revolutionary movements will be able to overthrow a government. Furthermore, they seem not to have thought about inquiring into the role of the United States in these countries. This points up the lack of symmetry. The problem should have been phrased to include the study of "us" as well as "them." It is not possible to make a decent analysis of a situation unless one takes into account the role of all the different people and groups involved in it; and there was no room in the design for such contingency analysis.

In discussing the policy impact on a social science research project, we should not overlook the difference between "contract" work and "grants." Project Camelot commenced with the US Army; that is to say, it was initiated for a practical purpose determined by the client. This differs markedly from the typical academic grant in that its sponsorship had "built-in" ends. The scholar usually *seeks* a grant; in this case the donor, the Army, promoted its own aims. In some measure, the hostility for Project Camelot may be an unconscious reflection of this distinction – a dim feeling that there was something "non-academic," and certainly not disinterested, about Project Camelot, irrespective of the quality of the scholars associated with it.

## The Ethics of Policy Research

The issue of "scientific rights" versus "social myths" is perennial. Some maintain that the scientist ought not penetrate beyond legally or morally sanctioned limits and others argue that such limits cannot exist for science. In treading on the sensitive issue of national sovereignty, Project Camelot reflects the generalized dilemma. In deference to intelligent researchers, in recognition of them as scholars, they should have been invited by Camelot to air their misgivings and qualms about government (and especially Army-sponsored) research – to declare their moral conscience. Instead, they were mistakenly approached as skillful, useful potential employees of a higher body, subject to an authority higher than their scientific calling.

What is central is not the political motives of the sponsor. For social scientists were not being enlisted in an intelligence system for "spying" purposes. But given their professional standing, their great sense of intellectual honor and pride, they could not be "employed" without proper deference for their stature. Professional authority should have prevailed from beginning to end with complete command of the right to thrash out the moral and political dilemmas as researchers saw them. The Army, however respectful and protective of free expression, was "hiring help" and not openly and honestly submitting a problem to the higher professional and scientific authority of social science.

The propriety of the Army to define and delimit all questions, which Camelot should have had a right to examine, was never placed in doubt. This is a tragic precedent; it reflects the arrogance of a consumer of intellectual merchandise. And this relationship of inequality corrupted the lines of authority, and profoundly limited the autonomy of the social scientists involved. It became clear that the social scientist savant was not so much functioning as an applied social scientist as he was supplying information to a powerful client.

The question of who sponsors research is not nearly so decisive as the question of ultimate use of such information. The sponsorship of a project, whether by the United States Army or by the Boy Scouts of America, is by itself neither good nor bad. Sponsorship is good or bad only insofar as the intended outcomes can be predetermined and the parameters of those intended outcomes tailored to the sponsor's expectations. Those social scientists critical of the project never really denied its freedom and independence, but questioned instead the purpose and character of its intended results.

It would be a gross oversimplification, if not an outright error, to assume that the theoretical problems of Project Camelot derive from any reactionary character of the project designers. The director went far and wide to select a group of men for the advisory board, the core planning group, the summer study group, and the various conference groupings, who in fact were more liberal in their orientations than any random sampling of the sociological profession would likely turn up.

However, in nearly every page of the various working papers, there are assertions which clearly derive from American military policy objectives rather than scientific method. The steady assumption that internal warfare is damaging disregards the possibility that a government may not be in a position to take actions either to relieve or improve mass conditions, or that such actions as are contemplated may be more concerned with reducing conflict than with improving conditions. The added statements about the United States Army and its "important mission in the positive and constructive aspects of nation building..." assume the reality of such a function in an utterly unquestioning and unconvincing form. The first rule of the scientific game is not to make assumptions about friends and enemies in such a way as to promote the use of different criteria for the former and the latter.

The story of Project Camelot was not a confrontation of good versus evil. Obviously, not all men behaved with equal fidelity or with equal civility. Some men were weaker than others, some more callous, and some more stupid. But all of this is extrinsic to the heart of the problem of Camelot: What are and are not the legitimate functions of a scientist?

In conclusion, two important points must be clearly kept in mind and clearly apart. First, Project Camelot was intellectually, and from my own perspective, ideologically unsound. However, and more significantly, Camelot was not cancelled because of its faulty intellectual approaches. Instead, its cancellation came as an act of government censorship, and an expression of the contempt for social science so prevalent among those who need it most. Thus it was political expedience, rather than its lack of scientific merit, that led to the demise of Camelot because it threatened to rock State Department relations with Latin America.

Second, giving the State Department the right to screen and approve government-funded social science research projects on other countries, as the President has ordered, is a supreme act of censorship. Among the agencies that grant funds for such research are the National Institutes of Mental Health, the National Science Foundation, the National Aeronautics and Space Agency, and the Office of Education. Why should the State Department have veto power over the scientific pursuits of men and projects funded by these and other agencies in order to satisfy the policy needs – or policy failures – of the moment? President Johnson's directive is a gross violation of the autonomous nature of science.

We must be careful not to allow social science projects with which we may vociferously disagree on political and ideological grounds to be decimated or dismantled by government fiat. Across the ideological divide is a common social science understanding that the contemporary expression of reason in politics today is applied social science, and that the cancellation of Camelot, however pleasing it may be on political grounds to advocates of a civilian solution to Latin American affairs, represents a decisive setback for social science research.

# 20

# Confronting the Ethics of Ethnography: Lessons From Fieldwork in Central America

*Philippe Bourgois*

## North American Cultural Anthropology

The ethics of anthropological research are too complicated and important to be reduced to unambiguous absolutes or even perhaps to be clearly defined. The human tragedy and political dilemmas I encountered in my ethnographic fieldwork in Central America obliged me to confront the inadequacy and internal contradictions of current definitions of research ethics in North American anthropology. These ethical quandaries arise within the epistemological tension imposed by the US intellectual tradition of allegedly apolitical liberal relativism in opposition to engaged universalism. In Europe, Latin America, and elsewhere this intellectual dichotomy between science and politics is not as clearly drawn. Indeed, throughout much of the rest of the world, engaged scholarly analysis is not only legitimate but part of the researcher's "social responsibility."

I am not proposing a systematic political economy of North American anthropological knowledge, but I do feel that the framing of ethical issues in cultural anthropology needs to be understood in the context of the history of the development of the discipline in the larger society. The eminently political orientation of a supposed apolitical commitment to empirical research must be appreciated for its internal inconsistencies and ultimate ethical poverty. Finally, the emergence in the late 1980s of the "post-modernist deconstructivist" focus on "culture-as-text" within symbolic anthropology as the most attractive theoretical tendency among US anthropologists needs to be placed in the problematic context of anthropological ethics in a politically polarized world.

In the late 1960s and early 1970s North American social scientists began discussing the ethical dilemmas faced by fieldworkers studying and living in a world rife with political turmoil. Several important edited volumes in anthropology were produced on the subject (cf. Weaver 1973; Huizer and Mannheim 1979; Hymes 1972) and major journals devoted considerable space to earnest – and at times polemical – debates by important figures in the discipline (cf. *Current Anthropology* 1968, 9(5):391–435; 1971, 12(3): 321–56).

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In an important early volume a dozen anthropologists from around the world questioned the historical relationship between the development of the discipline in a functionalist theoretical framework in Great Britain and the political and economic realities of British colonial domination and indirect rule in Africa and elsewhere (Asad 1973). This critical reappraisal of the roots of the discipline was even prominently incorporated in a major cultural anthropology textbook in 1981 (Keesing 1981: 481–99). Respected anthropologists in North America have denounced the conscious and unconscious collaboration of anthropology with the counterinsurgency agencies of the US government – specifically Project Camelot in Latin America (Horowitz 1967), Project Agile in Thailand (Gough 1973; Jones 1971; Wolf and Jorgensen 1970), and the Himalayan Border Countries Project in India (Berreman 1973).[1]

The at times polemical debates of the late 1960s and early 1970s have injected an important self-consciousness among US anthropologists researching far from home. We have come a long way from our European forebears (especially the British) who flew into colonial war zones under the auspices of colonial offices to interview "natives" and write "how-to-administer-more-humanely" reports for government bureaucracies intent on increasing "administrative" efficiency and lowering costs. Today few self-respecting anthropologists would condone the exercise of anthropology at the service of a world superpower or as a complement to espionage. Significantly, however, during the 1980s and early 1990s, articles and volumes devoted primarily to the *politics of ethics* were relatively scarce with notable exceptions (Magubane and Faris 1985; Rebel 1989a, 1989b; Sanadjian 1990). Instead, most ethnographers now include a discussion of the methodological and personal ethical dilemmas they faced during their fieldwork. Most recently, when the politics of ethics are referred to it is often in the style of the "reflexive poetics" of one of the post-modernist approaches, far removed from effective practice. In their deconstruction of domination the post-modernists risk trivializing the experience of oppression. At its worst this "poetics of politics" degenerates into what the late Robert F. Murphy (1990:331) satirized as "a kind of egghead rap-talk" or "thick writing."

## The Discipline's Narrow Definition of Ethics

Traditionally our discipline cites a limited dimension of ethical dilemmas: We worry about whether or not our research subjects have truly consented in an "informed" manner to our study; we ponder over the honesty of our presentation of self; we condemn the distortion in the local economy caused by the resources we inject into it in the form of "informants'" gifts or wages; we are wary of the social disapproval foisted on our primary informants when they become the objects of envy or ridicule from the rest of the community because of the resources, prestige, or shame we bring them; we no longer steal ceremonial secrets unapologetically; we examine our emotions introspectively to control our ethnocentrism; we uphold cultural relativism and avoid unconsciously conveying disrespect for traditional institutions and values through our lifestyle; we preserve the anonymity of our research subjects and host communities; we feel guilty for violating the privacy of our informants and their culture; we worry about "scientific colonialism" and our "responsibility to the host community" (so we send extra copies of our publications to our research site); we do not take photographs indiscriminately and we do not tape record without obtaining prior permission; we discuss the pros and cons of consulting forbidden archives or quoting from personal diaries and letters; we question the ethics of accepting financial support from governments and politically biased institutions; we worry about the potential misuse of our research material once it has been published in the public domain; and finally we take care not to jeopardize the access of future colleagues to our fieldwork site by our actions and publications.

These are indeed, all vitally important ethical issues that we must all confront during fieldwork and write-up. But why does the anthropological concern with ethics stop here? What about the larger moral and human dimensions of the political and economic structures ravaging most of the peoples that anthropologists have studied historically? With notable exceptions most North American anthropologists do not include the political or even the human rights dimension confronting the people they research in their discussion of "anthropological ethics." In fact the trend has been to avoid these issues by a theoretical focus on the meaning of signs and symbols outside of social context – what Roger Keesing (1987) has also criticized methodologically as the tendency to impose articulate but intensely subjective and exotic interpretations of religion, myth, and cosmology on the people we study; and what Hermann Rebel argues are the "tendencies in recent work by anthropologists . . . to downplay the degradation and terror experienced by victims of exploitation and persecution" (1989a:117).

The problem with contemporary anthropological ethics is not merely that the boundaries of what is defined as ethical are too narrowly drawn, but more importantly, that ethics can be subject to rigid, righteous interpretations which place them at loggerheads with overarching human rights concerns. How does one investigate power relations and fulfill the researcher's obligations to obtain informed consent from the powerful? What about the right to privacy of absentee landlords as a social group? It is much more difficult – if not impossible – to satisfy the discipline-bound anthropological/methodological code of ethics if we attempt to research marginalization and oppression, than if we focus on the philosophical aesthetics of cosmology. Can we analyze the urgent problems faced by our research subjects and still obey our discipline's interpretation of methodological ethics?

## A Moral Imperative to Anthropology

The simple solution so often adopted by anthropologists is to avoid examining unequal power relationships – and to orient their theoretical interests towards safer, more traditionally exotic focuses. In the late 1960s Eric Wolf (1972[1969]:261) admonished anthropology to avoid a "descent into triviality and irrelevance" by focusing on large-scale "problems of power."

A logistical imperative could also be advanced for why cultural anthropologists might want to assign priority to an analysis of power inequalities in their research. Unlike philosophers, literary critics, or art historians, we usually study living human beings. Furthermore we differentiate ourselves methodologically from other social science and humanities disciplines which also study humans through our technique of participant/observation fieldwork. We are not allowed to remain at our desks to pore over census tracts; we have to venture into everyday life not just to interview people but to actually participate in their daily life and to partake of their social and cultural reality. In the Third World, therefore, fieldwork offers a privileged arena for intensive contact with politically imposed human tragedy. Perhaps this methodological obligation to be participant/observers could inject a humanistic praxis into our research? Does social responsibility have to contradict our discipline's commitment to cultural relativism?

A moral argument for theoretical compassion does not stop at methodological praxis. We also have a historical responsibility to the particular types of research subjects selected by our forebears. Historically our discipline differentiated itself from sociology and other social sciences by focusing on the "distinctive other" (Hymes 1972:31). Since our inception we have had what Keesing (1987:161) calls a "predilection for the exotic" and what Sidney Mintz (1970:14) criticized as a "preoccupation with purity." We are most famous for having trekked deepest into the remotest corners of colonial territories to try to find people outside the reach of "civilization." We have unabashedly worshipped "the traditional" – so long as it is in a pristine vacuum. Over the past two decades we have begun to remedy our ahistorical, disarticulated focus on the particular. Ethnographies are increasingly situating our research in regional contexts. In fact, as Carol Smith (1984) notes in an article on the Maya in the Western Highlands of Guatemala, it has almost become fashionable for anthropologists to bemoan the myopic-community-study-in-a-vacuum focus of traditional anthropology.

Even when we succeed in finding a particularly remote cultural cranny where a "traditional" people has had only minimal contact with the outside world we can safely predict that these noble folk will sooner than later be sucked into the world economy in a traumatized manner. There is a good chance that their land and subsistence base will be stolen; their efforts at resistance will be met with violence, sometimes genocide; their entrance into the labor market will be in the most vulnerable niche; if they are hired by a multinational agro-export company – as they so often are – they will be systematically assigned to the labor gangs that spray venomous pesticides; if they work for a transnational subsidiary exploiting mineral resources – as they so often do – they will be sent to the bottom of the shafts to contract lung cancer – or worse. If they manage to maintain their ancestral lands, when they finally start to bring their produce to markets they will be obliged to sell at below subsistence prices; when they come into contact with the dominant ethnic groups and classes of their nation they will be ridiculed. In other words, with few exceptions, the traditional, noble, and "exotic" subjects of anthropology have today emerged as the most malnourished, politically repressed, economically exploited humans on earth. As a rule of thumb, the deeper, more traditional, and more "isolated" the people our forebears studied, the more traumatized their lifeways have become today.

Given that there is virtually no such thing as a traditional people disconnected from the outside world, then our "traditional" fieldwork sites should grant us privileged access to the massive sector of humanity pinned into the world economy's most vulnerable nexus. We have chosen to study the wretched of the earth. These are the individuals too often condemned to periodic famines, to below subsistence-level incorporation in flooded labor markets, to relocation, dislocation, or more simply extermination. Many of our discipline's former research subjects are fighting back in organized political movements; but as the Central American experience demonstrates, their struggles are prolonged, bloody, and often unsuccessful. Although as uninvited outsiders it might be naive and arrogant for us to think we have anything definitive to offer, we can still recognize the ethical challenge. Why do we avoid it?

In the early 1980s dissertations were written on the hermeneutics of shame among the Maya. But how can we understand the meaning of that important cultural construct if we ignore the tens of thousands of Maya massacred by the military at the same time, or the hundreds of thousands who migrate each year to harvest cotton, sugar cane, and coffee.

Even if there were no urgent human rights imperatives as in the case of the Maya; even if there was no extreme economic exploitation and subsistence dislocation; there is at least a scientific imperative to situate their "webs of significance" in the context of what they are really doing every day.

## Compassion for the Fourth World – Only

The journals and books that regularly denounce ethnocide and genocide published by indigenous rights organizations – such as the IWGIA in Copenhagen, Survival International in England and France, or Cultural Survival in Cambridge, Massachusetts – are a welcome exception to the tendency for anthropologists to escape a human rights mandate. Significantly, however, often these organizations tend to legitimize their militance by purposefully narrowing their focus in the classically anthropological manner – in pursuit of the "noble savage." They prefer to denounce genocide when it also entails ethnocide.

In Central America this theoretical orientation is referred to as *indigenista* or "fourth worldist." Amerindian culture is seen in a manichean manner as the human ideal while Hispanic culture is treated as irrelevant at best. This *indigenista* tendency is most prevalent among North American anthropologists, and one can recognize its intellectual roots in the discipline's traditional focus on exotic, isolated community studies.

Although guided by a moral vision to denounce human rights abuse, fourth worldists tend to ignore international geo-political contexts because of their geographically and culturally reductionist theoretical focus. This leads to arbitrary compassion; for example, I published a brief account of the poisoning of Guaymi Indian banana workers who spray pesticides for the United Fruit Company in a special issue of a French fourth worldist journal documenting the human rights violations of indigenous peoples in Central America (Bourgois 1986). The editors would not have been interested in the article had the poisoned sprayers been Hispanic mestizoes rather than Amerindians. In fact they decided not to publish anything on massacres in the Salvadoran countryside because the peasants being killed were not Amerindians. Ironically only two generations ago most of the grandparents of these "Hispanic" Salvadoran rural dwellers currently being massacred were Pipils. They were forced to abandon their traditional language, dress, and indigenous culture when the government began systematically killing all indigenous peoples – between 18,000 and 30,000 individuals were massacred – following an Amerindian rebellion in 1932.

Fourth worldists provide vitally needed documentation of tragic human rights violations but they often fail to make common cause with human beings. They discriminate according to ethnicity, reproducing the traditional anthropological focus on the "exotic other" in a vacuum. This obscures their theoretical understanding of the structural roots of repression and exploitation by framing it exclusively in manichean culturalist terms.

## Fieldwork in Central America

### *At war in Nicaragua's Moskitia*

Let me document this critique in a classically anthropological manner – by drawing on my own fieldwork experience. My first stop in pursuit of a dissertation topic was Nicaragua in 1979 just after the overthrow of Somoza by the Sandinistas (Bourgois 1981). Like a good anthropologist, I went as far away from the capital city as possible into the Moskitia, the most remote corner of the only province where an indigenous population – the Miskitu – were said to have maintained organically their non-Hispanic culture. Their language, religious system, cultural identity, structure of land tenure, etc., was indeed distinct from the national Hispanic mainstream. Of course, the historical record reveals that there is nothing "traditional" or isolated about Amerindian culture in the Moskitia. The Miskitu emerged as a people distinct from their Sumu Amerindian neighbors in the 1600s through the colonial confrontation of the two great superpowers of the time – Spain and England. They allied themselves with British pirates – and later with Her Majesty herself – to fight off the Spanish conquerors. In the process they became the first indigenous people to obtain firearms. This enabled them to conquer all their aboriginal neighbors. They became warriors and economic middlemen selling Amerindian slaves and smuggled trade goods from the Central American mainland to British settlers in the Caribbean. Some of this historical legacy has been frozen linguistically – one-third of the words in their "traditional" language, for example, bear a relationship to English (Holm 1978).

Soon after I arrived in the heartland of Miskitu territory, the indigenous population began mobilizing to defend their historic rights to land and autonomy in a tragic alliance with the Central Intelligence Agency. "My" fieldwork village, accessible only by a full-day's journey upstream in a dug-out canoe, became the central arena of a bloody conflict against the central government. Although the underlying causes for this indigenous war were the historical structures of racism and marginalization of the region dating back to the colonial period, and repression by the central government during the contemporary period, the actual bloody logistics of the fighting was sponsored economically and was escalated militarily by the US government.

My theoretical training in anthropological approaches to political economy and history prepared me to deal with understanding who the Miskitu were – why there was nothing "traditional" about them – and why they might rise up in arms against their central government. I was completely unprepared, however, for what to do on the more important practical human level. Should I publish my material or would CIA analysts perusing academic journals seize upon my information to refine counterinsurgency operations the way monographs by unsuspecting – and not so unsuspecting – anthropologists working in Indochina were abused in Southeast Asia during the Vietnam War? (cf. discussion by Jones 1971 and the preface to Condominas

1977.) When I discussed these issues at professional societies in the US context I was "being political" or I was "outside the realm of anthropology." If I went to the media I was by definition no longer an academic researcher – or worse yet – I was a political activist posing as an anthropologist. If we are to be logically consistent to our discipline's position on honesty of self-presentation should we punish the closet human rights activists as firmly as we condemn the counterrevolutionary spy?

## *Peasant massacre in El Salvador*

My next aborted fieldwork experience proved to be even more painful and even more "political." Having been a participant/observer among a people who went through an extraordinarily rapid political mobilization, I entered the robust, interdisciplinary debate on peasant political mobilization. Anthropology's tradition of participant/observation fieldwork encouraged me to try to live among radicalized peasants rather than limit myself to examining their vital statistics from the vantage point of historical archives or census tract statistics. Indeed, I felt this lack of fieldwork data to be a crucial limitation of much of the literature published in political science, sociology, and history on peasant revolts.

With this in mind I went to explore the possibility of fieldwork in a Salvadoran refugee camp in Honduras (Bourgois 1982). My central ethical concern was that counterinsurgency experts would have access to my eventual publications and that I might unwittingly contribute to more efficient repression in the long run. I was also concerned lest I attract attention to refugee political leaders merely by being seen talking to them regularly. Because of these problems, I initially canceled my fieldwork plans, but on second thought I decided that the potential of the research warranted at least a preliminary feasibility investigation.

In my exploratory visits to the refugee camps, I was surprised to learn that the refugees desperately wanted foreigners to reside in the camps with them. They sought out my company because a foreign witness deters local military officials from engaging in random abuses. They assured me that far from placing them in danger, my physical presence granted them a measure of security. The church and United Nations organizations operating the camps were also interested in having an anthropologist present on a long-term basis. They pointed out that, as a full-time researcher, I would also be in an ideal position to document human rights abuses and to help receive civilians continuing to flee government search and destroy operations just across the border. In fact, all the human rights workers I spoke with urged me to stay and undertake my study in the camps.

This did not remedy the problem of the potential misuse of my published research. Several refugees suggested I cross the border into El Salvador and discuss this complicated issue with the fighters and sympathizers who remained in their home communities. (I think the refugees also hoped that a brief visit on my part would end my repeated uninformed questions on such obvious facts as the distance between their houses or the fertility of their fields.) Although CIA analysts probably collect most theoretical studies on peasant politicization in Central America, I thought one manner of reducing the practical counterinsurgency value of such research would be to delay publication – aside from periodic human rights reports – until the political and military situation had changed sufficiently to limit the applicability of my data. The theoretical questions I would be exploring were already a part of the rigorous scholarly debate on revolutionary peasants in the social sciences. Was that entire debate to be dropped from social science research for fear of raising the analytic capabilities of the CIA?

To abbreviate a long story, a few days after my arrival, while I was still debating this issue a group of peasants planning to cross into El Salvador a few hours later offered to let me accompany them. I impetuously – in retrospect unwisely – jumped at the opportunity. My intent was to stay in El Salvador for only 48 hours. I thought conversations with peasants and fighters in the war zone would help me come to a final decision as to whether or not extended fieldwork in the refugee camps in Honduras was feasible and – more importantly – ethically defensible.

My 48 hour visit to El Salvador was prolonged into a fourteen day nightmare when the

Salvadoran military launched a search and destroy operation against the region. The government forces surrounded a 40 square kilometer region (approximately a dozen hamlets) and began systematically bombarding, mortaring, and strafing the entire zone with airplanes, Huey helicopters, and artillery. There were approximately a thousand peasants living in the area and only one or two hundred of these had guns and probably less than a dozen were formal members of the FMLN. The population was composed of a typical cross-section of peasants – the kind of people you would find anywhere in rural Latin America if you circled off 40 square kilometers: grandmothers, grandfathers, young and middle-aged men and women, pregnant mothers, suckling infants, children etc. . . . We were all the target of the Salvadoran air force and army. I gave the following oral account to a journalist shortly after my return to the US:

> When the bombardments and strafings began we would take over anywhere we could. I was told to crouch beside a tree trunk and, whatever I did, not to move. They'd shoot at anything that moved. I remember inching around a tree trunk to keep something solid between me and the machine-gun fire of the helicopters.
>
> Sometimes the mortar shots came 10 times in a row, and there's a tremendous sense of panic when you hear them getting closer and closer. I was told that when I heard a mortar fired I should grit my teeth and keep my mouth open to prevent my ear drums from rupturing. . . . On the first four days, . . . about 15 men, women and children . . . were wounded. Shrapnel was removed, and amputations were performed with absolutely no pain medicine. (*Washington Post* February 14, 1982 pp. C1)

On the fourth night of the invasion we tried to break through the government troops encircling us. The plan was for the FMLN fighters (i.e., younger peasants with guns and minimal military training) to draw fire from a machine gun nest set up by the government soldiers on a knoll while the rest of us civilians tried to run by unseen in the darkness of the night. Once again, there were about a thousand of us all ages, several pregnant, others sick, one blind, and many under three years of age:

> We were on a rocky path with a Salvadoran gunpost off to our left. FMLN guerrillas, also on our left and to the rear, drew fire while we made a break for it. The babies the women were carrying were shrieking at the noise and, as soon as we got within earshot, the Salvadoran forces turned their fire on us.
>
> At this point, it was pandemonium. Grenades were landing around us; machine guns were firing; we were running. A little boy about 20 yards ahead of me was blown in half when a grenade landed on him. His body lay in the middle of the path, so I had to run over it to escape. (Ibid.)

I remember at one point being crouched near a women under cover of some bushes when her baby began to cry. She waved at me with her hand and whispered to me to run away as fast as possible before the government soldiers heard the noise. I obeyed, and sprinting forward I heard machine gun bullets and shrieks all around me. Mothers and infants made up the bulk of the casualties that night. Only a mother can carry her baby under fire because only a mother has a chance of preventing her suckling infant from crying. The Salvadoran military was shooting in the darkness into the sound of crying babies.

Six to seven hundred of us managed to sprint past the machine gun nest. For the next fourteen days, we stayed together running at night and hiding during the day:

> One of the major hazards we always faced was the noise of crying babies and the moans of the wounded, making the whole group vulnerable to detection. Rags were stuffed in the mouths of the wounded, so their cries would not be heard. The babies cried a lot because they were hungry; their mothers' milk had dried up.
>
> A young woman gave birth on the second night of our flight. She was up and running for her life the next day, along with the rest of us. Those of us who were young and healthy were lucky. It was the law of survival at its cruelest: the slow runners and the elderly were killed. (Ibid.)

At one point we crossed back through the villages we had fled out of:

> . . . we were hit with the overpowering stench of decaying bodies. There were donkeys, pigs,

> horses, chickens – all dead. The soldiers had burned down as many of the houses as they could, ripped apart the granaries, it even looked as if they had tried to trample the fields. (Ibid.)
>
> ... [We] came upon the naked body of a middle-aged woman. Her clothes had been ripped off and apparently acid had been poured on her skin because it was bubbling off. The body had been left in a prominent position along the path, presumably to terrorize any survivors. (Bourgois 1982:21)

## The academic reaction

That was the end of my fieldwork on ideology and material reality among revolutionary peasants. It was also almost the end of my anthropological career, after I sought out the media and human rights lobbyists on Capitol Hill to present my testimony to the public. I had violated several of the anthropological/methodological ethics discussed earlier along with the specific duties of a graduate student to keep his/her academic advisors informed of a change in research plans. A strong argument was made to terminate me as a graduate student – and with abundant justification according to the anthropological ethics I had broken: I had crossed a border illegally thereby violating the laws of my host country government; I had not notified my dissertation committee of my decision to explore a new dangerous research site; I had notified the media and contacted human rights organizations thereby violating the right to privacy of my research subjects; I had potentially jeopardized the future opportunities of colleagues to research in Honduras and El Salvador by breaking immigration laws and calling attention to government repression in public forums.

Significantly, had I not gone to the media with my testimony of human rights violations anthropological ethics would not have been violated in as serious a manner. It would have remained a personal story between my committee and myself as an unsuccessful and reckless preliminary fieldwork exploration that had been decided against as too dangerous. By remaining silent I would not have violated anyone's rights to privacy nor have threatened my colleagues' access to the field, nor offended my host country government.

Of course my personal sense of moral responsibility obliged me to provide public testimony and I entered the media/political arena. I discovered that a North American anthropologist is not supposed to document human rights violations if it involves violating a host country government's laws or contravenes the informed consent and right to privacy of the parties involved. In other words, anthropology's ethics can be interpreted at loggerheads with humanity's common sense. I could have crossed into FMLN territory as a journalist or as a human rights activist but not as an anthropologist because access to the information I was seeking was only available by crossing a border illegally; publicizing that information also violated a people's right to privacy and informed consent. Subsequently, lobbying to change US foreign policy exacerbates these transgressions since political denunciation is not conduct befitting an anthropologist.

To reiterate, the problem is rooted in a specifically North American epistemology of relativism and "value-free science" which forbids engaged research and – when taken to its logical conclusion – denies absolute assertions including those of universal human rights. This alleged "apolitical" orientation expresses itself within US academia in a phobic relationship to the media and in a righteous condemnation of "political activism." In contrast to Europe – especially France – where political militance and an occasional *Op Ed* in *Le Monde* is a sign of academic prestige (Bourdieu 1984), in the US, newspaper editorials and magazine articles are often interpreted as an indication of lack of serious commitment to science. While we do have to be cautious of sacrificing analytical rigor by becoming too immersed in media presentations and political polemics are we supposed to keep our human rights denunciations out of the public domain in the name of anthropological ethics and scientific rigor?[2]

It is important that the discipline enforce the tenets of informed consent and respect for host country governments. Taken out of context, however, these academic requisites obscure the political and economic realities of the regions where we have traditionally been most active. A research project which investigates structures of inequality will have a hard time passing a human subjects review board if the canons of anthropological ethics are rigidly applied. Are we supposed to abandon controversial research? Most political economy studies can be defined as potentially unethical. A fieldworker cannot obtain important information on unequal power relations by strictly obeying the power structure's rules and laws (cf. Nader 1972:303ff). How does one obtain meaningful information on peasant/landlord relations if the landlord is required to provide truly informed consent? What are the limits to "informed consent" in settings of highly unequal power relations? Do we have to notify absentee landlords prior to interviewing sharecroppers on their estates? Are we allowed to obtain jobs in factories in order to document union repression? Did I have an obligation to obtain informed consent from the Salvadoran government troops firing at us before photographing the children they wounded? Where does one draw the line? Does one abandon urgent research simply because a dictatorial host nation government does not want its repressive political system to be documented? How does one decide whether a host country government is sufficiently repressive to warrant breaking its laws? These unresolved questions reveal that there is nothing apolitical about the North American commitment to relativism and to its methodologically defined body of ethics. Most dramatically, the ethic of informed consent as it is interpreted by human subjects review boards at North American universities implicitly reinforces the political status quo. Understood in a real world context, the entire logic of anthropology's ethics is premised on a highly political assertion that unequal power relations are not particularly relevant to our research.

## *Informed consent: United Fruit Company versus banana workers*

For my final dissertation fieldwork project, I purposefully selected a host country which was free of civil-political strife; nevertheless, the same ethical contradictions arose. I studied ethnic relations on a United Fruit Company banana plantation spanning the Costa Rica/Panama border (Bourgois 1988, 1989). My first obvious problem was that the transnational corporation had redefined the border, and the plantation's operations illegally straddled Panama and Costa Rica. My real host country "government," therefore, turned out to be the United Fruit Company – not Costa Rica or Panama. High level United Fruit Company officials considered my topic – "a history of the ethnicity of the population in the plantation region" – innocuously "anthropological" and ordered local plantation officials to graciously open their confidential files to me. I was even allowed to reside in workers' barracks for some 16 months. Had management's consent been truly informed and had the Company understood what a historical analysis of ethnicity in a plantation context would reveal, I would not have been allowed to document systematically the transnational's quasi-apartheid labor hierarchy; its ethnic discrimination on occupational safety issues; or its destruction of the union movement by ethnic recruitment etc. The head managers would not have toured me through their golf course, drunk whiskey with me, and made racist comments about their workers to me if they had really understood anthropological participant/observation research technique. Although I was never overtly dishonest; and although I always precisely explained my research topic to everyone; they obviously did not understand my research implications or they would have run me out of the area and/or beaten me up.

In fact, participant/observation fieldwork by its very definition dangerously stretches the anthropological ethic of informed consent. We obviously have an obligation to let the people we are researching know that they are being studied and that a book and/or articles will eventually be written about them. Furthermore, we have to explain as precisely as possible the focus of our study. At the same time, we are taught in our courses preparatory to fieldwork that the gifted researcher must break the boundaries between outsider and insider. We are supposed to "build rapport" and develop such a level of trust and acceptance in our host societies that we do not distort social interaction. Anything less leads to the collection of skewed or superficial data. How can we reconcile effective participant/observation with truly "informed consent"? Is rapport building a covert way of saying "encourage people to forget that you are constantly observing them and registering everything they are saying and doing"? Technically, to maintain truly informed consent we should interrupt controversial conversations and activities – Miranda act style – to remind everyone that everything they say or do may be recorded in fieldwork notes.

Experienced fieldworkers usually advise novice ethnographers not to take notes in public while undertaking fieldwork. Is that a false presentation of self? Is good participant-observation fieldwork inconsistent with anthropological ethics? Where do we draw the line? Are we allowed to research illegal operations? Do we systematically have to avoid frequenting the rich and powerful who regularly bend and break laws? Once again, these important ethical dilemmas become even more pronounced when we are focusing on conflict and unequal power relations.

## Theoretical Context

I hope I have not raised these issues in too moralistic and righteous a tone. Anthropologists do not have to convert themselves into human rights activists and political cadre for "worthy" causes in order to remain ethical persons. Although perhaps another – arguably more consistent – way of reformulating anthropological ethics would be to require that our studies among the "poor and powerless" contribute to their empowerment. That would certainly be different from the current practice of requiring "ethical researchers" to obtain the informed consent of landlords and military bureaucracies. Nevertheless, this discussion of our human responsibility to our research subjects does not imply that we automatically have something concrete to offer in their struggles for survival or for political rights. We are outsiders; and we have a formidable capacity unwittingly by our mere presence to cause trouble or to complicate matters seriously.

Symbolic studies of all kinds are important for the vitality of anthropology just as is literary and artistic criticism, folklore, and philosophy for understanding the most important dimensions of humanity. From a more specifically political perspective, interpretative, post-modernist studies have potentially emancipating insights to offer us. They have identified axes of domination which have been inadequately worked out by Marxists. There have been many important exploratory articles dealing from a symbolic perspective with the meaning of violence and political repression (cf. Falla 1983; Taussig 1984). More squarely within the post-modernist movements, feminist theoreticians such as Donna Haraway (1988) with her call for "situated knowledges" have advocated politically committed research. There are also voices from inside anthropology calling colleagues to task for ignoring histories of explosively repressive status quo's in pursuit of vigorously essentialist visions of isolated Indians (Starn 1991). Similarly, interpretive anthropologists and sociologists from historically dominated ethnic groups have challenged intellectuals to confront the content of racism in creative ways (Gilroy 1987, Limon 1989, Rosaldo 1990, Santamaria 1986).

Nevertheless, in our explosive real world context, practitioners of the post-modernist deconstructivist movements within anthropology can be more confident than can most political economy-oriented ethnographers that their publications will not cause absentee landlords to unleash the secret police on their respondents; nor are their future colleagues as likely to be barred by host governments because of how they interpreted the poetics of power. All of us, regardless of our theoretical orientation, need to reexamine the place of human concern in our pursuit of science among the starving and the persecuted. Let us not be political when we claim to be apolitical in the name of ethics; and vice versa let us not allow our "politics" to stagnate behind sensitive rhetoric.

It would be dangerous and arrogant to think that there are definite answers to any of these ethical/moral questions. We need to discuss them and think about them in both practical and theoretical terms. Meanwhile, however, as all of us (without exception) wallow in a phenomenological swamp of signs and symbols we should not forget that our "informants" continue to be crucified.

# 21

# Ethics versus "Realism" in Anthropology

*Gerald D. Berreman*

## Introduction

At the annual meeting of the American Anthropological Association in December 1985, I participated as a representative of the Association's Committee on Ethics (COE) during the era in which its Principles of Professional Responsibility (PPR) – the official euphemism for our code of ethics – was drafted and adopted: 1969 to 1971. The situation at the time of that meeting was that a new, draft Code of Ethics (dCoE) had been placed before the membership in the October 1984 edition of the *Anthropology Newsletter*, fundamentally redefining and reformulating the concepts of ethics and responsibility in the profession to accord with what its authors said were the changed circumstances of the time. Discussion of the draft code was invited in the columns of the *Newsletter* and in an open forum, which had been held for the purpose at the 1984 annual meeting, with the goal of revising it and bringing it to a vote in the fall of 1985. If adopted it was to supersede the PPR or, if a vote were to appear premature at that time, further discussion was to be scheduled before holding the vote. The latter course was followed (including the 1985 session, "Ethics, Professionalism and the Future of Anthropology," and a follow-up session at the 1986 meetings). My role in each was to put the draft code into historical context and give my response to it. This chapter is based on those two presentations. The vote, incidentally, was never held.

I will introduce the context for what I have to say simply by asserting that I believe humane ethics in research and scholarship to be practical necessities for anthropologists today, just as human rights and self-interest – social justice and survival – have become inseparable for people everywhere (Berreman 1980:12; cf. 1971c:398–9). Therefore, when I speak of ethics versus realism in anthropology I am not referring to the "consummate realism" that Ernest Becker called "instrumental utopianism" (1971:xi), and that both he and C. Wright Mills before him advocated: the forthright application of reason to the solution of human problems. Rather, the "realism" I contrast ironically with ethics is that referred to by Mills (1963:402) as the boast of "crackpot realists," whom Becker called "hard-headed realists," that is, "the militarists and other bureaucrats ... [with] their age-old practical nightmares" (Becker 1971:xi). That

Gerald D. Berreman, "Ethics versus 'Realism' in Anthropology," pp. 38–71 from Carolyn Fluehr-Lobban (ed.), *Ethics and the Profession of Anthropology: Dialogue for a New Era* (Philadelphia: University of Pennsylvania Press, 1996). Copyright © 1991 by the University of Pennsylvania Press.

said, let me proceed to some historical background for the issues of ethics and "realism" in anthropology today.

## History: Anthropology and Ethics, 1919 to 1986

The first ethical issue to discernibly attract the attention of the American Anthropological Association was the brief and tragicomical imbroglio of 1919 to 1920, wherein Franz Boas, founder of our discipline in America, became the only member of the association ever to be censured and expelled (Stocking 1968:273). His offense was that he reported in *The Nation* "incontrovertible proof," which had "accidentally" come his way, that "at least four" anthropologists had served as spies under cover of scholarly research during World War I (Boas 1919:729; see also AAA 1920:93–4).

The first serious systematic concern with ethics as such in our profession came about thirty years later, after World War II, when in 1948 the association adopted the "Resolution of Freedom of Publication," urging "all sponsoring institutions to guarantee their research scientists complete freedom to interpret and publish their findings without censorship or interference, provided that the interests of [those] studied are protected" (AAA 1949: 370).[1] (That resolution should be borne in mind, I think, as we consider the provisions of the draft code.) But it took the infamous and ill-fated Project Camelot, an American counterinsurgency research plan for Chile in 1965 – more than fifteen years after the research freedom resolution – to focus anthropological attention squarely on the issues of ethics and secrecy in research (Horowitz 1967). It was in response to this that the Beals Committee was appointed by the Executive Board of the AAA in 1967, which then produced the groundbreaking report entitled "Background Information on Problems of Anthropological Research and Ethics" (Beals et al. 1967:2–13). As a result of this report, the membership of the AAA voted adoption, in March 1967, of the "Statement of Problems of Anthropological Research and Ethics," based on that report. It is still in effect and is part of the packet of materials entitled *Professional Ethics* (1983), that is available from the Executive Office of the American Anthropological Association. It comprises essentially a draft code of ethics for the association and is a forerunner, both in time and content, of the Principles of Professional Responsibility.

At this point in the history of these matters, we move into what for many members of the association were the glory days (or the gory days, depending upon one's social and political viewpoint) of the late 1960s and early 1970s. The virtuous and the villainous were unambiguously defined no matter which side one was on, with few who were neutral or undecided. To say that is not to belittle the struggle or its importance. It was the good fight and the stakes were high. University and college departments were politically and ethically split (my own, for one), as was the profession. Friendships were severed, even as others were forged that would be strong and everlasting; enemies were made, respect was won and lost, principles were upheld and betrayed. The association was riven. The turmoil was perhaps most vividly displayed during and following the presidential election of 1970, wherein I, a member of the Committee on Ethics (which had confronted the Executive Board on issues surrounding the PPR and the ethics of anthropologists' involvement in mission-oriented activities in Southeast Asia, notably Thailand), was nominated as a presidential candidate. It was the first time that a nomination for association office had come from the electorate, as provided in the constitution, in addition to those candidates provided by the Committee on Nominations (three in the case of the presidency; *Newsletter*, 1970d:1). Shortly before the election, two of the three nominees of the committee withdrew their candidacies in favor of the third, considered the strongest of the three, in order to make it a two-person contest "because of the serious issues confronting the association and the introduction of a new nominee" (*Newsletter*, 1970e:1). The tactic evidently worked, as the committee's remaining candidate, Anthony Wallace, won by a margin of about two to one, although there is, of course, no way to know what might have been the outcome had all four candidates fulfilled the agreement upon which their acceptance of nomination was constitutionally predicated: that they would run and would serve if elected (*Newsletter*, 1971:1).[2]

In any case, in an effort to heal the wounds of these divisive events, at the annual meeting I was asked by the president, George Foster, to give a brief address to the council in my role as the defeated candidate and presumed spokesperson for the dissident minority. In view of the then-recent abolition of the presidential address, mine was the only address delivered before the council that year, an unusual parliamentary event, to say the least, and an opportunity I could hardly decline (Berreman 1971a; 1971b). I doubt that my talk did much to realize the hopes which motivated the request that I speak, but though it viewed the future of the association through what proved to have been a rather clouded crystal ball, it did gratify those who had supported my nomination for the values it represented. The tensions, disagreements, and divisions that surfaced in those days have diminished only slowly and uncertainly at best. The continuing controversy over the PPR, and the possibility of its replacement by the dCoE, are manifestations of that schism, and the heat of the continuing arguments reflects that of the Vietnam era, with some of the same cast of characters generating it. It is still a good fight and the stakes are still high, on both sides, but the substance of disagreement has clearly diminished within the profession, the arguments have become less dramatic, the membership less polarized, and the social and political context less conducive to clear-cut definition and resolution of the issues. Nevertheless, they retain their vital, ultimate importance to anthropology and will continue to trouble it as a discipline and a profession through periods of both apathy and concern.

I have gone through those now rare and yellowed documents, letters, telegrams, pronouncements, minutes of endless meetings, and recaptured some of the vitality I remember from that bygone era – the issues debated, the evidence cited, the strategies planned, the counterstrategies detected – and was reminded of the names and actions of coconspirators, adversaries, and commentators whom I had not seen or thought of for years, as well as those who are oft- and well-remembered. They are all there in the dusty files of the faithful and, no doubt, in those of the faithless as well. They await some energetic chronicler of our profession to bring them systematically to light as an exercise in the sociology (or anthropology) of knowledge, social change, and history.

At the time of these events, American military involvement in Vietnam, both directly and via Thailand, was heavy and rapidly escalating. Chad Gordon put it clearly in a memo to the Cambridge Project advisory board which objected to that involvement: "As the Defense Department's posture in the world becomes increasingly bizarre and dangerous, any participant in such projects will undoubtedly feel called upon to account for his action to colleagues, students and the wider public" (quoted by Coburn 1969:1253). Coburn adds, with renewed relevance today to the proposed dCoE, "It is this issue of accountability that troubles many" (ibid.).

Such concerns were widespread even as those, including anthropologists, whose actions inspired them were running amok with their involvement as advisers and contributors to military adventurism (Student Mobilization Committee 1970; Flanagan 1971).

## Committee on Ethics and Principles of Professional Responsibility

At the end of 1968, the Executive Board of the AAA appointed an ad hoc Committee on Ethics, whose mission was "to consider questions of the role of the Association with regard to ethical conduct on the part of anthropologists," and to report the results of their deliberations to the Executive Board (*Newsletter*, 1969:3).

The ad hoc committee was composed of David Schneider, cochairman (University of Chicago), who played a key role in selecting his co-members, David Aberle, cochairman (University of British Columbia), Richard N. Adams (University of Texas), Joseph Jorgensen (University of Michigan), William Shack (University of Illinois, Chicago Circle), and Eric Wolf (University of Michigan). They met on

25–6 January 1969 and wrote a report which included a recommendation for an elected "Standing Committee on Ethics" of the association. Defining in detail the responsibilities of the proposed standing committee, the ad hoc committee outlined "the framework for the issues that the Committee on Ethics must consider." This constituted, in effect, a draft code of ethics for the AAA, and was published in the April 1969 issue of the *Newsletter* (compare: ad hoc Committee on Ethics, *Newsletter*, 1969:3–6, with Principles of Professional Responsibility, *Newsletter*, 1970f:14–16).

That draft code was perhaps too radical for the Executive Board in those turbulent times, for they tabled it and attended only to a second recommendation of the ad hoc committee, namely,

> IMMEDIATE election of an Ethics Committee DIRECTLY responsible to the electorate of Fellows (and of students enfranchised).... The Committee urged that the new, elected Committee ... be independent of the Board. ... This recommendation was NOT accepted by the Board.... It agreed to hold an election for a pro tem Ethics Committee concurrent with the autumn [1969] election of the President-Elect and new members of the Executive Board. (*Newsletter*, 1969:3)

Thus, the two bodies agreed on the principle of an elected ethics committee, but disagreed on whether it should be responsible directly to the electorate or to the Executive Board. This difference of opinion remained a bone of contention for years because, once the elected Committee on Ethics was in place, it defined its responsibility as being directly to the membership of the association, which the Executive Board disputed.

The mission of the new committee was to include consideration of the advisability and nature of an ethics code. The elected committee would consist of nine elected members except that initially, for purposes of continuity, three would carry over for one year from the appointed ad hoc committee, while six would be elected (three allotted terms of three years and the other three, terms of two years). In addition there would be a member of the Executive Board to serve ex officio as a nonvoting liaison member of the committee. Those elected in the fall of 1969 were Norman Chance (University of Connecticut), Robert Ehrich (Brooklyn College, CUNY), Wayne Suttles (Portland State University), Terence Turner (University of Chicago), Oswald Werner (Northwestern University), and I (University of California, Berkeley). Those carried over from the ad hoc committee were Eric Wolf (chair), Joseph Jorgensen, and William Shack. David Aberle, who had been a member of the ad hoc committee, had in the meantime been elected to the Executive Board, which appointed him as liaison member to the pro tem committee.

The newly constituted committee met several times, working primarily toward the preparation of a code of ethics, which it submitted to the Executive Board in May 1970. The board promptly retitled it "Principles of Professional Responsibility" (to soften the blow for members who did not want anyone to subject them to the constraints a "code" seems to imply), and it was published in the November 1970 *Newsletter*, as cited above. After heated debate in the columns of the *Newsletter*, in committees, at annual meetings, in departments, and in private, and after many intervening crises in the Executive Board, the Committee on Ethics, the council, and within the membership, the principles were adopted by vote of the association in May 1971.

The PPR's initial year, spanning the period from its formulation to its adoption, was a stormy one, to say the least. On 30 March 1970, the Student Mobilization Committee to Stop the War in Vietnam (SMC) sent to a number of scholars, including some members of the Committee on Ethics of the AAA, as well as to people in other academic disciplines (primarily people involved in Asian studies of various sorts), materials indicating participation by scholars in what they regarded as clandestine, counterinsurgency research and other activities in Southeast Asia under sponsorship of various United States governmental agencies, including such mission-oriented ones as the Departments of Defense and State and special agencies within them. The SMC planned to release a report on these documents at a press conference they had called for 3 April at the Annual Meeting of the Association for

Asian Studies, to be held in San Francisco during the first week of April 1970, and at another press conference scheduled simultaneously, or nearly so, in Washington, DC. The materials were minutes, letters, reports, financial accountings, publications, and similar contents of the files of an anthropology professor at a major West Coast university, copied by a student employee of the professor from files to which she had legitimate access in the course of her work. She had regarded their contents as alarming, and had taken the liberty of making copies for herself, which she then turned over to the Student Mobilization Committee. The SMC proceeded to publish them in the 2 April 1970 edition of *The Student Mobilizer*, under the edition title *Counterinsurgency Research on Campus Exposed*. At the press conferences arranged to announce these findings and distribute *The Student Mobilizer*, four of the scholars who had been sent advance copies of some of the documents issued statements condemning the work that had been exposed: Eric Wolf, Joseph Jorgensen, Marshall Sahlins (in Washington, DC), and I (in San Francisco). Later, others joined in condemning that which had been exposed, while still others condemned the exposure and those who participated in it and those who had decried the activities exposed. Some wrote of "liberated" documents; others of "purloined" ones. Controversy raged, charges and countercharges, insults and counterinsults were traded, lawsuits were threatened (though since no law had been broken, none was ever filed). For vivid and partisan accounts – and all were partisan in this matter, including me – see "Anthropology on the Warpath in Thailand" (Wolf and Jorgensen 1970a:26–36) and "Anthropology on the Warpath: An Exchange" (Foster et al. 1971:43–6).

The Executive Board wrote a stinging (and, I would have to say, ill-informed) letter on 19 May 1970, reprimanding the Committee on Ethics and, specifically, Wolf and Jorgensen for their statements and actions (*Newsletter*, 1970b:1, 10). The letter included these remarks:

> The Board instructs the Ethics Committee to limit itself to its specific charge, narrowly interpreted, namely to present to the Board recommendations on its future role and functions, and to fulfill this charge without further collection of case materials or by any quasi-investigative activities.

It concluded that "the Board explicitly instructs the members of the Ethics Committee . . . to make clear in individual statements that they do not speak for the Committee or the Association."[3] It was the alleged violation of this last after-the-fact instruction which evidently brought the wrath of the Board down on Wolf and Jorgensen for their joint statement on the SMC revelations, and exempted – at least from explicit condemnation – Sahlins and me. Sahlins was exempted because he was not on the committee, and I was because my statement was made as an Asianist scholar in the context of the Association for Asian Studies meetings, rather than as a member of the AAA ethics committee. Wolf and Jorgensen had identified themselves as members of the ethics committee and indicated their intent to bring "these serious matters" to the attention of the American Anthropological Association, but in no way did they imply that they were speaking *for* the Committee on Ethics. They responded quickly, strongly, and in detail in a letter dated 25 May 1970 to the president and president-elect of the association, the Executive Board (which includes those two officers), and the Committee on Ethics. They concluded the letter with their resignation from the committee, pointing out that

> In its Statement the Board wishes the Ethics Committee to limit itself to its specific charge, narrowly interpreted; but it is not equally specific about its own intention to cope with the issues raised by an applied anthropology which has for its focal concern the internal security of the present Thailand government. In drawing attention to the action of particular members of the Ethics Committee, the Board evidently hoped to avert a threat to the internal harmony of our Association. In not applying themselves with equal diligence to an analysis of the issues which prompted these individual actions, the Board averts its eyes from the real sources of a danger which threatens not only the integrity of the Association, but the fate and welfare of the peoples among whom we work. In view of the failure of the Board to interpret its mandate to the

Ethics Committee to include a concern of vital relevance to the profession, we ourselves fail to perceive how the Committee can "present to the Board recommendations on its future role and functions." We therefore tender our resignations as Chairman and Member of the Committee on Ethics. (Wolf and Jorgensen 1970b, which includes not only this letter, but the letter and other documents which the Executive Board found untenable).[4]

Within a week, David Aberle resigned as the board's liaison member on the Committee on Ethics (Aberle 1970:19), while Wolf and Jorgensen encouraged other members to remain on the committee to continue its work.

A month later, in response to demands from both sides of the controversy, David Schneider, then a member of the Executive Board, proposed that a committee be appointed to look into the entire issue, both the activities of the Committee on Ethics and those of anthropologists working in Thailand. Accordingly, the Executive Board quickly appointed the "Ad Hoc Committee to Evaluate the Controversy Concerning Anthropological Activities in Relation to Thailand," consisting of three members: Margaret Mead, chair (Columbia University and American Museum of Natural History); William Davenport (University of California, Santa Cruz); David Olmsted (University of California, Davis); and, as executive secretary, Ruth Freed (New York University and American Museum of Natural History). This ad hoc committee (often called the "Mead Committee") collected documents from a variety of sources, many of them from the Committee on Ethics, and others from principals in the controversy and from other people knowledgeable about Thailand and the issues of the controversy. By this committee's account, "all members of the ad hoc committee and its Executive Secretary . . . examined all of these materials in detail" and in the process "approximately 6000 pages were read, and many reread in order for the ad hoc committee to write this report" (Davenport et al. 1971:2). This achievement was the more remarkable in view of the fact that the committee's chairperson was for much of the year in the South Pacific. The six-page report that resulted was submitted to the Executive Board on 27 September 1971. It totally exonerated all members of the American Anthropological Association of any ethical wrongdoing in the context of this controversy: "1. No civilian member of the American Anthropological Association had contravened the principles laid down in the 1967 Statement on *Problems of Anthropological Research and Ethics* (Beals Report) in his or her work in Thailand" (Davenport et al. 1971:4). It went on to explain seeming offenses as merely misleading claims in research applications and reports made necessary by a climate where defense relevance was required in order to obtain government funding for research. As the committee put it, "The mislabeling or redirecting of scientific projects in order to obtain funds may have seemed necessary: it may also have prepared anthropologists . . . to close their eyes to misuse of their data . . . and . . . talents" (ibid., 4). It was suggesting that "counterinsurgency" may have been merely a buzzword incorporated in applications and reports to release funds for the author's scholarly research, just as earlier buzzwords such as "community development" and "mental health" served this function.

At the same time, the Mead Committee reprimanded the members of the Committee on Ethics who, it said, "acted hastily, unfairly, and unwisely in making public statements . . . without first having consulted the anthropologists named in the purloined documents which formed the basis of their charges, and without having obtained authorization from the Board" (ibid.). The Mead Report was presented to the Council of the AAA in New York at its annual meeting on 19 November 1971, where by vote of the assembled body, and over the vehement objections of its authors, it was divided into three substantive sections and presented for discussion preparatory to a vote on whether to accept or reject each of the sections: part 1 was primarily about the activities of anthropologists in Thailand; part 2 was primarily on the actions of the Committee on Ethics; and part 3 constituted proposed guidelines, recommendations, and resolutions.

The debate, before some four hundred members, focused on the exoneration of those who had been criticized for their work in Thailand. The atmosphere was tense in view of the demands of the chairperson of the ad hoc committee, on behalf of herself and the other authors, that the report not be put to a vote but simply be put in the record as submitted (for the handwriting was already on the wall), and the membership's determination, ratified in short order by its vote, to put each section to a vote. No doubt the most telling moments in the discussion of the report were when two members of the Committee on Ethics, responding to the assertion that no civilian member of the AAA had contravened the ethical principles of the association in their work in Thailand, read from two egregiously unethical projects in which civilian fellows of the association had been directly involved, as demonstrated in documents which the ad hoc committee had had in its hands, provided by the Committee on Ethics. The first was as follows, as reported in its author's own abstract of the project:

> AD-468 413 Military Research and Development Center, Bangkok (Thailand) LOW ALTITUDE VISUAL SEARCH FOR INDIVIDUAL HUMAN TARGETS: FURTHER FIELD TESTING IN SOUTHEAST ASIA. By [an anthropologist] 15 June 65, 83 pp. Unclassified. Project description text:
>
> This report is a detailed study of quantitative information on the ability of airborne observers to sight and identify single humans on the ground. The target background for most of the testing was rice paddy with scattered bushes and trees at the end of the dry season in Southeast Asia . . . [etc., etc.].

The second item read to the membership consisted of excerpts from a forty-four-page proposal in which a fellow of the AAA was involved, titled "Counter-Insurgency in Thailand: The Impact of Economic, Social and Political Action Programs." This was a half-million-dollar social science research and development proposal submitted to the Advanced Research Projects Agency of the Department of Defense by the American Institutes for Research of Pittsburgh in 1967. After introducing the problem, to design preventive counterinsurgency measures for Thailand, and to "pave the way for the generalization of the methodology to other programs in other countries" (p. ii, see below), the proposal went on to state:

> The struggle between an established government and insurgent forces involves three different types of operations: the first is to make inputs into the social system that will gain the active support of an ever-increasing proportion of the population. *Threats*, promises, ideological appeals, and tangible benefits are the kinds of inputs that are most frequently used. The second is to reduce or interdict the flow of the competing inputs being made by the opposing side by installing anti-infiltration devices, cutting communications lines, *assassinating key spokesmen, strengthening retaliatory mechanisms* and similar preventative measures. The third is to counteract or neutralize the political successes already achieved by groups committed to the "wrong" side. This typically involves *direct military* confrontation.
>
> *The social scientist can make significant contributions to the design of all three types of operations.* (American Institutes for Research, 1967:1; all emphasis added)

The proposal continues in this vein for forty-four chilling pages. In the final paragraph of what is termed the "Operational Plan," we read the following: "The potential applicability of the findings in the United States will also receive special attention. In many of our key domestic programs, especially those directed at *disadvantaged sub-cultures*, the methodological problems are highly similar to those described in this proposal, and the application of the Thai findings at home constitutes a potentially most significant project contribution" (ibid., 34; emphasis added).

The proposal was accepted and funded, but its semiannual reports are classified, hence unavailable – for good reason, one suspects. (See Berreman 1981a:72–126, wherein many more examples of ethically questionable and untenable anthropological projects are itemized.)

In that great auditorium where over four hundred anthropologists were seated, there followed a deathly silence. No rebuttal was offered by the Mead Committee or from the membership. The votes were held on the three sections and the report was overwhelmingly rejected, section by section. The *Newsletter* of January 1972 reported it on page 1 under the headline "COUNCIL REJECTS THAI CONTROVERSY COMMITTEE'S REPORT." After introducing the account in general, the article proceeded: "The first part in this division [of the report for purposes of the vote] ... was rejected by a vote of 308 to 74. The second part was also rejected, 243 to 57. And a final motion to consider the issue of anthropologists' actions in Thailand unresolved and to reject the remainder of the report ... was carried by a vote of 214 to 14 as the clock move into Saturday and the Council dwindled away" (*Newsletter*, 1972:1).

In the same issue of the *Newsletter*, two letters were printed as "Replies to the Report of the Ad Hoc Committee to Evaluate the Thailand Controversy," both of which had been distributed to the council at its meeting. One, by Wolf and Jorgensen, gave point-by-point rebuttals to many of the ad hoc committee's assertions and concluded that "we are as much dismayed by the callousness of the report as by its factual and theoretical faults." The other letter was by May N. Diaz and Lucile F. Newman, who challenged the report and concluded that "anthropologists cannot serve both science and war" (ibid., 3–4).

That, then, is the context within which the Principles of Professional Responsibility originated and evolved. It is against this background that we must understand the emergence of the draft Code of Ethics, changes in the role and function of the Committee on Ethics, and even the reorganization of the American Anthropological Association. I think these three changes are symptomatic of common forces at work within the profession, common pressures from without, and common processes at work on the national and international levels. They comprise a shift away from idealism and toward self-interested practicality. It is to the demand for a new code of ethics as a symptom of these broader discontents, and to the implications of that demand and those discontents, that I now turn – or return.

## The Draft Code of Ethics: Text and Context

The Principles of Professional Responsibility, having been adopted in 1971 and having served the profession adequately, if not remarkably, for some years as a cautionary and exemplary model more than as an enforcement mechanism or deterrent (although with the potential to serve those functions as well), gradually lost the attention of the membership it was designed to serve, as did the committee which was to sustain and implement it. There had been only three amendments to it since its adoption: one, in 1974, relating to plagiarism (*Newsletter*, 1974:9); and two in 1975, one requiring that informants be apprised of the fact that anonymity cannot be guaranteed against the possibility of accidental disclosure, and the other advising that exclusionary policies against colleagues on the basis of non-academic attributes and the transmittal of such irrelevant factors in personnel actions is unethical (*Newsletter*, 1975:1). The only overall revision as such was the removal of generic use of the masculine pronoun from the document in about 1976.

According to the October 1984 *Newsletter*, "by 1975, active concerns had surfaced about aspects of the PPR and the grievance procedures. The growing number of non-academically based anthropologists held that the PPR was based only on academic considerations ... Important inconsistencies and ambiguities were found in the various documents relating to ethics" (*Newsletter*, 1984:2). In short, and in currently popular jargon, the hegemony of academic anthropology, and especially of academic social and cultural anthropology, was challenged. The Executive Board asked the Committee on Ethics to consider these and related problems and to propose remedies. It did so, and its suggestions were circulated to committees and individuals in the association. In 1980, an ad hoc committee was appointed to prepare a new draft code. It was made up of Karl Heider, chair (University of South Carolina); Barry Bainton and Alice Brues (University of Colorado, Boulder); Jerald Milanich (University of Florida and Florida State Museum); and John Roberts (University of Pittsburgh). After the committee members had prepared the draft and it had been circulated, commented upon, and revised, it was finally considered by the Executive Board to be "ready to go to the membership" in 1982 (ibid.). However, in view of the fact that the association was in the throes of reorganization at that time – which process, like the proposed new code, was designed to facilitate and respond to changes in the profession and especially to its members' employment structure –

it was thought the membership might be distracted from attending adequately to the ad hoc committee's proposals on ethics. Accordingly, it was decided that the proposed code would be withheld from the membership until the fall of 1984. Therefore, the draft Code of Ethics (dCoE) was first presented to the membership when it was published in the October 1984 *Newsletter* (ibid.).

An "open forum" on the proposed dCoE was held at the November 1984 annual meetings in Denver. These were also the first meetings of the AAA organized to reflect the reorganization of the association into its five major divisions, now merged with its previously "affiliated" societies. In spite of the chaotic novelty of these changes, there was a lively debate on the ethics proposal – a debate which was reviewed, with discussion invited, by President-elect Helm in the April *Newsletter* (Helm 1985:1). Thereafter, the *Newsletter* was sprinkled with letters and commentaries on the subject, displaying a variety of opinions and commitments, in the months leading to the 1985 annual meetings in Washington, DC, at which the session on "Ethics, Professionalism and the Future of Anthropology" (where the contents of this chapter were originally presented) was held.

These events were in accord with the original declaration in the PPR that it would be "from time to time" scrutinized and revised as the membership of the AAA "sees fit or as circumstances dictate" (*Professional Ethics*, 1983:5).

The 1985 session on ethics was aimed at bringing out key facts, issues, and points of view surrounding the proposed changes in the ethical stance of the AAA in the context of scholarly presentations and intellectual discussion, not to exclude debate and commitment, but with the goal of providing more light than heat.

## Ethics versus Practicality: An Interpretation

The remainder of this chapter will comprise my response to the status or condition of ethics in the AAA as it has emerged since 1985. It is an interpretation, therefore, rather than, as above, a historical review. One must be familiar with both the PPR and the dCoE to consider the matter fairly, which is why I have delved into history at such length and why, in addition, these two critical documents are reproduced in this volume.

There are four major changes proposed in the draft Code of Ethics – four deletions from the Principles of Professional Responsibility – which I regard as drastic. These four must be clearly stated and directly addressed in the implications they hold for anthropology and anthropologists to convey why I, and many others, regard the changes as pernicious.

First is the downgrading – the virtual elimination – of the primary and fundamental tenet of the Principles: "1. Relations with those studied: In research, anthropologists' paramount responsibility is to those they study."

Second is the elimination of secret and clandestine activity in anthropological endeavors as constituting violations of anthropological ethics.

Third is removal of the principle of accountability of the anthropologist for violations of ethical principles – removal of any mention of sanctions, or their legitimacy, to say nothing of eliminating all traces of mechanisms for enjoining adherence to ethical principles.

Fourth is deletion of anthropologists' positive responsibilities to society at large, their own and/or those they study: the responsibility to convey their findings and the implications thereof forthrightly to all concerned fully and publicly, to the best of their professional abilities.

I maintain that these omissions have resulted not in a code at all, but a mild statement of intent, and one conspicuously devoid of ethical content. It is in fact, I think, a license for unfettered free-enterprise research, advising and engineering disguised as anthropology, with the intent of employing the ethical reputation of the discipline to enable and facilitate a wide range of mission-oriented activities, including those of dubious ethical and even egregiously unethical nature.[5] To title the draft document "Code of Ethics" is to misrepresent it seriously. It might be better to adopt a title parallel to that of the PPR – this one: Principles of Professional Irresponsibility (PPI). To do so would serve the interests of candor and thereby make at least one contribution to ethics in the profession.

I will briefly discuss these four principles-by-omission, in reverse order.

## *Positive responsibilities in anthropology*

To my mind the most insidious, because inconspicuous, deletion from the PPR in the dCoE is the issue of the positive responsibility of anthropologists to let it be known publicly what they have learned and what they believe its implications to be for all concerned. It is stated this way in the PPR:

> 2. Responsibility to the Public:
>    d. .... Anthropologists bear a positive responsibility to speak out publicly, both individually and collectively, on what they know and what they believe as a result of their professional expertise gained in the study of human beings. That is, they bear a professional responsibility to contribute to an "adequate definition of reality" [Mills 1963:611] upon which public opinion and public policy may be based.

That is, we acknowledge a responsibility to practice what C. Wright Mills (ibid., and 1959:178–9) called "the politics of truth" for, as he insisted, in the defining instance, truth *is* our politics and our responsibility (Mills 1963:611). If we do not fulfill this responsibility, we are nothing more than human engineers – hirelings in the service of any agency with any agenda that can buy our expertise, as some indeed became during the Vietnam War. This theme has pervaded virtually all discussions of ethics in our profession during the past twenty-five years (cf. Berreman 1968, 1981b).

## *Accountability*

Now it is proposed that we adopt a code of ethics without accountability. In the dCoE there is no mechanism whatsoever by which individuals can be held to account for their actions, no matter how blatantly and destructively they flaunt their profession's ethical standards and regardless of how bland such standards may be.

Why? Do those who propose and endorse this deletion intend to leave open the opportunity to violate at will the very principles of ethical conduct upon which their profession has agreed? I hope not. Is the statement in the PPR too harsh, too constraining, too dangerous? I think not. Accountability is mentioned in the PPR only in its epilogue, where it is stated:

> In the final analysis, anthropological research is a human undertaking, dependent upon choices for which the individual bears ethical as well as scientific responsibility. That responsibility is a human, not superhuman, responsibility. To err is human, to forgive humane. This statement of principles of professional responsibility is not designed to punish, but to provide guidelines which can minimize the occasions upon which there is a need to forgive. *When anthropologists, by their actions, jeopardize peoples studied, professional colleagues, students, or others, or if they otherwise betray their professional commitments, their colleagues may legitimately inquire into the propriety of those actions, and take such measures as lie within the legitimate power of their Association as the membership of the Association deems appropriate.* (*Professional Ethics*, 1983:2; emphasis added)

The italicized sentence is a statement of accountability and is the nearest thing in the PPR to a mechanism for its enforcement. Is it too threatening to the new anthropology, to practicing anthropology, to be incorporated into the proposed code? Does it go against the spirit and intent of the dCoE? I think so. It must be remembered that in the Vietnam anthropologist-warriors' case it was accountability that was the issue; it was accountability that was denied by one side and insisted upon by the other. Maybe *that* is the crux of the problem now; maybe *that* is what the draft code proposes to protect future warrior-anthropologists from. Perhaps now all's to be fair in love, war, and anthropology; maybe it is proposed that henceforth there is to be no more honor among anthropologists than has long been claimed to be among thieves. Are we witnessing an attempt to exempt us as scholars, as scientists, as anthropologists from the principle invoked by the United Nations at Nuremburg after

World War II, which held accountable soldiers and bureaucrats who sought to evade being held responsible for their atrocities on the ground that they were only doing their jobs, for their country? Do the anthropologists protesting against accountability not protest too much? If so, again we must ask, why? If, as I prefer to believe is the case (because I am ever charitable in such matters), the aim is simply to forestall the possibility of an ethics witch-hunt, it is misguided – a clear case of the proposed cure being worse than the affliction. There is a striking lack of evidence to even suggest that the PPR is conducive to such an eventuality. In its twenty-year history, the Committee on Ethics has pursued only one case that I know of to the Executive Board, and *no* case went any further than that for, in the balance of power enacted into the "Rules and Procedures" (*Professional Ethics*, 1983:7–9), the Executive Board must agree with the Committee on Ethics that there is a prima facie case of ethical violation for the matter to be pursued further. That has not, to my knowledge, ever occurred. But I believe that the possibility of censure or other measures of accountability within the association, however mild and symbolic, gives credibility to our claims to ethical standards and therefore has a salutary effect in their realization, which a purely and piously advisory "code" would not.

## Secrecy and clandestine activity

We are urged to consider adoption of an ethics for anthropological endeavor which tolerates secret and clandestine activities or, as the president-elect suggested in 1985, which permits secret activity but prohibits that which is clandestine: perhaps she can explain it; I cannot:

> The PPR coupling of "clandestine and secret research" may constitute a *prima facie* condemnation of some anthropologists employed in non-academic settings. In this . . . respect, it appears that the first step might be to set aside the pejorative concept "clandestine" from the consideration of ethical issues arising in employment and research – notably for government, business, industry and special interest groups – that may involve some form or degree of secrecy. (Helm 1985:13)

Yet again, why? Is it, as seems to be the argument, that although "secret" and "clandestine" mean the same they are after all necessary activities in the minds of some practicing anthropologists and "clandestine" *sounds* worse?

It should be noted that the repudiation of secret or clandestine activity in the name of anthropology has been the most long-standing and the most consistently, unequivocally enunciated of ethical principles embraced by American anthropologists. From our earliest expressions of ethical concern as an association and a profession, no secret or clandestine research, no secret reports, have been tolerated among us. In 1948, the association adopted a resolution on freedom of publication and protection of the interests of those studied. It thereby anticipated the PPR by addressing two of its four key principles, now repudiated in the dCoE. That resolution forty years ago stated in part: "(1) that the AAA strongly urge[s] all sponsoring institutions to guarantee their research scientists complete freedom to interpret and publish their findings without censorship or interference; provided that (2) the interests of the persons and communities or other social groups studied are protected" (American Anthropological Association 1949:370). That resolution was reaffirmed by the association in its 1967 "Statement on Problems and Anthropological Research and Ethics," the introduction to which asserted that, "constraint, deception and secrecy have no place in science. Actions which compromise the intellectual integrity and autonomy of research scholars . . . not only weaken those international understandings essential to our discipline, but in so doing they also threaten any contribution anthropology might make to our own society and to the general interests of human welfare" (*Professional Ethics*, 1983:3). The statement went on to quote and endorse the 1948 resolution quoted above and, in order "to extend and strengthen this resolution," added that,

> Except in the event of a declaration of war by Congress [*note*: there was no such declaration in the Vietnam War], academic institutions should not undertake activities or accept contracts in anthropology that are not related to their normal function of

teaching, research and public service. They should not lend themselves to clandestine activities. We deplore unnecessary restrictive classifications of research reports ... and excessive security regulations imposed on participating academic personnel.

3. The best interests of scientific research are not served by the imposition of external restrictions. The review procedures instituted for foreign area research contracts by ... the Department of State ... offer a dangerous potential for censorship of research. Additional demands [for clearance and the like] ... are incompatible with effective anthropological research. (Ibid.)

There followed an unambiguous resolution passed at the annual meetings of the AAA in 1969, ratified by mail ballot in the spring of 1970: "Resolution 13 (Karen Sacks), Resolved that members of the AAA shall not engage in any secret or classified research" (*Newsletter*, 1970a; 1970c).

A year later, the issue of secrecy and clandestine activity was addressed directly by the membership of the AAA when it adopted the Principles of Professional Responsibility, which dealt with it in at least six places in the body of the principles and once in an appendix – an indication of the great importance such activity has continued to hold in our profession. It is worthwhile to extract and quote each of these treatments of the issue so that we are vividly reminded of the enormity of the deletion of this subject from the dCoE:

From the PPR:

1. Relations with those studied: ...
   g. In accordance with the Association's general position on clandestine and secret research, no reports should be provided to sponsors that are not also available to the general public and, where practicable, to the population studied.
2. Responsibility to the public:
   a. Anthropologists should not communicate findings secretly to some and withhold them from others.
3. Responsibility to the discipline:
   a. Anthropologists should undertake no secret research or any research whose results cannot be freely derived and publicly reported.
   b. Anthropologists should avoid even the appearance of engaging in clandestine research, by fully and freely disclosing the aims and sponsorship of all research.
5. Responsibility to sponsors:
   .... Anthropologists should be especially careful not to promise or imply acceptance of conditions contrary to their professional ethics or competing commitments. This requires that they require of sponsors full disclosure of the sources of funds, personnel, aims of the institution and the research project, and disposition of research results. Anthropologists must retain the right to make all ethical decisions in their research. They should enter into no secret agreements with sponsors regarding research, results or reports.
6. Responsibility to one's own government and to host governments:
   .... [Anthropologists] should demand assurance that they will not be required to compromise their professional responsibilities and ethics as a condition of their permission to pursue research. Specifically, no secret research, no secret reports or debriefings of any kind should be agreed to or given. If these matters are clearly understood in advance, serious complications and misunderstandings can generally be avoided. (*Professional Ethics*, 1983:1–2)

Appendix C: A Note on "Clandestine"

The 1967 *Random House Dictionary of the English Language*, defines the term clandestine as follow:

> characterized by, done for, or executed with secrecy or concealment, especially for purposes of subversion or deception; private or surreptitious.
>
> The [Ethics] Committee construes these definitions to mean that the concealment of research goals from a subject population (including nonarticulation of such goals when they include potentially injurious consequences to the social, economic, cultural and/or physical well being of the population) or other forms of deception of the population with respect to the uses to which the researcher is aware the data he gathers will be put, regardless of whether or not the research program itself is kept secret or whether publications issuing from it are classified, properly

> falls within the meaning of the term "clandestine" and thus is in violation of the 1967 resolution ["Statement on Problems of Anthropological Research and Ethics"]. (Annual Report of the Committee on Ethics, September 1970, *Newsletter*, 1970f:16)

In thinking back to Helm's suggestion that *clandestine* be distinguished from *secret*, and the former be prohibited and the latter permitted, it seems that *secret* is to be the word for approved clandestinity; *clandestine* will be the word for disapproved secrecy or, as she wrote, the "pejorative" word for secrecy. Whatever the terminology, it is clear that the draft code, in rescuing secret and clandestine activities from the list of unethical practices, is proposing that they be regarded as acceptable, necessary, even desirable (in the case of practicing anthropologists) items in the anthropological bag of methodological tricks. This I regard as abject surrender of principle to a misguided practicality; a sacrifice of public interest to misperceived self-interest: replacing ethics with greed.[6]

I had thought these matters through well before joining the Committee on Ethics or confronting the task of helping prepare a code of ethics. At the risk of offering a surfeit of my own opinions on the subject, I will offer some words of my own, presented in a paper for the presidential panel titled "The Funding of Asian Studies" at the Association for Asian Studies annual meeting in 1970 (the same meeting, incidentally, at which the Student Mobilization Committee held its press conference discussed above):

> There is no scholarly activity any of us can do better in secret than in public. There is none we can pursue as well, in fact, because of the implicit but inevitable restraints secrecy places on scholarship. To do research in secret, or to report it in secret, is to invite suspicion, and legitimately so because secrecy is the hallmark of intrigue, not scholarship. . . . I believe that we should make freedom from secrecy an unalterable condition of our research. (Berreman 1971c:396)

So much for secret *and/or* clandestine activity as excusable, much less legitimate, by anthropologists, and so much for an ethics that permits it.

In the new, and otherwise satisfactory (to me) "Revised Principles of Professional Responsibility," prepared after this essay was initially written (published for discussion in the November 1989 issue of the *Anthropology Newsletter*; submitted for ratification to the membership, 15 March 1990), I am alarmed to note that the issue of secrecy is side-stepped. The word does not appear, nor is the matter addressed beyond a single passing mention of a commitment to "open inquiry." This strikes me as odd, to say the least, in view of the centrality of the issue throughout the history of discussions of anthropological ethics in this country. To tolerate secret research is to sacrifice the credibility of anthropology as a research discipline and a humane science.

## *Priority of the welfare of those studied in anthropology*

Finally, I return to the first of these principles-in-absentia of the draft Code of Ethics, namely deletion from the code of the primary principle in the Principles of Professional Responsibility: that the welfare of those studied is the anthropologist's paramount responsibility. This was anticipated by a second resolution presented to the Council of the Association by Karen Sacks at the 1969 annual meetings, which was passed there and ratified in the spring of 1970:

> Resolution 14 (Karen Sacks),
>
> Resolved that fieldworkers shall not divulge any information orally or in writing, solicited by government officials, foundations, or corporation representatives about the people they study that compromises and/or endangers their well-being and cultural integrity. (*Newsletter*, 1970a; 1970c)

The Principles of Professional Responsibility stated this point unambiguously at the head of its list of principles. It is worthwhile to repeat a bit in order to remind ourselves:

> 1. Relations with those studied:
> In research, anthropologists' paramount responsibility is to those they study. When there is a conflict of interest, these individuals must come first. Anthropologists must do everything in their power to protect the physical, social and psychological welfare

> and to honor the dignity and privacy of those studied. (*Professional Ethics*, 1983:1)

Contrast that statement with the ambiguous one in the dCoE (bearing in mind that it is perhaps the *least* ambiguous of all the provisions in that document):

> 1. Anthropologists must seriously consider their own moral responsibility for their acts when there is a risk that an individual, group or organization may be hurt, exploited, or jeopardized physically, legally, in reputation, or in self-esteem as a result of these acts. (*Newsletter*, 1984:2)

Witness the fact that the people we study would be dropped not only from the highest priority (which they held in the ethical obligations specified by the PPR), but would virtually be dropped out of the draft code altogether.[7] And note that people are to be put on a par with organizations. If one were studying the Ku Klux Klan, concern for the well-being of individual informants would evidently be reduced to par with that for the KKK as an organization (or that of the organization would be raised to parity with individuals, as proponents might prefer to put it).

A more fundamental question than the quality of the proposed substitute code is why the change has been proposed – what forces have led to it?

## Reaganethics: The Temper of the Times

The history of the Committee on Ethics since 1971 has been one of rapid decline – *transformation* would be a more diplomatic word. It has evolved or devolved from a committee on ethics to essentially a grievance committee; from one concerned with ethical principles and practice to one devoted primarily to personnel matters: fairness in appointments and promotions, issues of plagiarism, priority of publication, and conflict of interest (e.g., AAA 1975:54–5). This parallels a shift in concern and involvement of faculty in a number of universities from issues of academic freedom to issues of privilege and tenure (as the respective academic senate committees are called on my campus), and a pervasive concern in our society with personal well-being at the expense of concern with broader principles of social justice.

I do not blame the members of the Association's Committee on Ethics for this situation. Those are the kinds of cases with which they have been saddled. The grievances are legitimate ones, but they should be the responsibility of a committee constituted for that purpose rather than of an ethics committee perverted to that purpose. The Committee on Ethics, which has evolved in this direction, was largely responsible for the draft code, with additional input from the significantly named "Committee on Anthropology and the Profession." The combination of an ethics committee concerned largely with professional grievances and a committee on anthropology as a profession (and, therefore, presumably focused primarily on nonacademic careers) not surprisingly proposed a code which was avowedly responsive to the "new realities" of a changing profession and, especially, as has been repeatedly asserted, to the circumstances of nonacademic anthropologists: those employed in government and in corporate and consultative agencies. But it was not only these two bodies that constructed the new code. According to the *Newsletter's* announcement of the draft code, "more than 60 members" of the association had already participated in composing the draft over a period of four years by the time it was first published in October 1984 (*Newsletter*, 1984:2). Everyone knows that a camel is a horse that was designed by a committee, as comedian Allan Sherman was wont to say, and this one was a super-committee. No wonder the result was a super-camel. The PPR was the result of a mere thirteen minds in over a year's time – a bit swaybacked and perhaps even of another color, but definitely the intended horse.

Alexander Leighton has quoted a saying popular in government circles during World War II that "the administrator uses social science the way a drunk uses a lamp post, for support rather than illumination" (Leighton 1949:128). The imperatives are different if one is providing policy support or if profits are the bottom line from a case in which one is seeking understanding. This is where the dCoE differs basically from the PPR, for the former is responsive to the needs of those who provide support; the latter to the needs of those who provide illumination. The letters in the columns of our newsletter make vivid the circumstances and ideologies that underlie both the demand for and the resistance to a revised code. One complains that the PPR

> established a set of standards so narrowly focused on a single environment that it excluded ... those of us who have chosen to work outside the grove.... Many institutions of government are charged with delivering specific services. The anthropologist can in many cases contribute to making that delivery more effective and humane. But in order to do so ... he or she ... must play by the rules of the game....
>
> This is no less the case in the private sector where there are many organizations which exist to *sell* knowledge.... Thus, many of us are conducting research the results of which are proprietary ... they belong to the firm and are simply not available except at a price.
>
> In a similar vein the prohibition of secret research simply fails to take into account the realities of today's world.... Anthropologists working in classified areas do so because they wish to influence national policy.... I welcome a Code of Ethics which considers such work a matter of personal choice. (Downs 1985:2)

Another correspondence, from a different perspective, notes the increased population of anthropologists and the diminished availability of "'classic' fieldwork experience," which raises critical problems, especially as anthropologists get involved in "the commercial market, and when they begin to reassess their ethical codes." Now that government support is drying up, he continues,

> we must seriously question whether or not the accommodation of individual economic incentives and the priorities of employment constitute an admittedly painful but critical conflict of interest for assessing the ethical directions for anthropology. It is not an oversimplified analogy to note that the wilderness bears of North America have been reduced to garbage dump scavengers. What will a century of private marketing under such priorities do to the ideals and perspectives for the practice of anthropology?
>
> ... if anthropology does not begin to secure some long-term analysis and planning, its ethical heritage will not sustain itself. (Wyoch 1986:24)

American anthropology has had a tradition of ethical concern and social responsibility of which we can be proud. The revelation of mission-oriented, counterinsurgency, and classified research and consultation undertaken by anthropologists working in Southeast Asia during the Vietnam War does not necessarily contradict this assessment. Such activities may well have been exceptional in the profession. Also, we must avoid the temptation to castigate Southeast Asianist anthropologists as unusually insensitive to ethical issues. I do not think people with that regional specialization were more ready to sell out than others; only that they had unique opportunities and inducements to do so. Had American military adventurism been elsewhere, I am sure that some anthropologists would have stepped forward to do the dirty work there. But I believe such people are in a minority in our profession. The very fact that the ethical issues were raised, the unethical activities exposed and deplored, is evidence of the profession's social conscience. The involvement of anthropologists in deplorable activities was not unusually pervasive, only unusually forthrightly condemned in the discipline. Other social scientists were doubtless equally or more frequently and deeply involved – political scientists, for certain, perhaps psychologists, economists, sociologists, geographers – but their involvement may have been more taken for granted by their colleagues as acceptable professional behavior, for they were not called to account so forcefully, if at all. Thus, the very fact and manner of revelation of anthropologists' involvement reflects an alert professional conscience that is commendable.

On the other hand, we cannot afford to be too self-congratulatory, either. We have had a tradition as well of complicity in colonial and neocolonial activity (Asad 1973; Lewis 1973), of advising exploiters of people and expropriators of their resources. Those who disclaim this have to resort to generalized denial together with claims to extenuating values and behaviors held to be traditional and pervasive in the profession that in fact may prove to have been more often implicit than explicitly manifest. For counterevidence they must rely on a very few individual and collective efforts in opposition to unethical activities, advocating and contributing to the cause of autonomy or emancipation of colonized and otherwise victimized peoples (Maybury-Lewis 1974). Nor can we deny the tendency in our association to overlook or sweep under the rug some of the most egregious of unethical involvements, while seeking to protect our collective reputation, as occurred in the instance described above of the expulsion of Boas by the council, replicated fifty years later by the effort of the Mead Committee to reprimand Wolf and Jorgensen – both instances comprising responses to courageous but potentially embarrassing revelations of unethical behavior by fellow anthropologists. On the positive side, we have the facts that the Mead Report was overwhelmingly rejected by the association and, of course, that the Principles of Professional Responsibility were enthusiastically adopted and seem still to be endorsed by an overwhelming majority of its members.

It is worth remembering, too, that for nearly two years (1972–4), there was a Committee on Potentially Harmful Effects of Anthropological Research (COPHEAR), appointed by the Executive Board at the direction of the membership as a result of a motion passed at the annual meeting of 1971 (AAA 1972:42; 1973:23, 62; 1974:72).[8] When that committee died on the vine in 1974, primarily for want of cooperation and success in collecting information sufficient to carry out its charge, there sprouted a proposal supported by many for a successor Committee on Ethnocide and Genocide to address some of the same issues but without limiting itself to anthropological culpability. This proposal was inexplicably dropped by the Executive Board shortly before the 1974 annual meetings. So far as I am aware, the important issues that it and COPHEAR were to have pursued have not been directly addressed in the association since then, but they did hold our attention for a time, and may again.

Meanwhile, such issues have been the focus of several organizations and collectivities of dedicated anthropologists operating on financial shoestrings outside of the framework of the association. Most notable among them are Cultural Survival, the International Work Group for Indigenous Affairs (IWGIA), and the currently inactive Anthropology Resource Center (ARC).[9] Probably the most useful source of information on these and others of the sort is in the appendix titled "Organizations and Periodicals" in the second edition (and, I understand, the forthcoming third edition) of John H. Bodley's *Victims of Progress* (Bodley 1982:217–20). As an example I would cite Shelton Davis and Robert Mathews (and their Anthropology Resource Center), who advocated and exemplified "public interest anthropology," which seems to be on a most positive track where ethical commitments and practicality are concerned:

> Public-interest anthropology differs from traditional applied anthropology in what is considered the object of study, whose interests the researcher represents, and what the researcher does with the results of his or her work. Public interest anthropology grows out of the democratic traditions of citizen activism rather than the bureaucratic needs of management and control. It is based on the premise that social problems – war, poverty, racism, sexism, environmental degradation, misuse of technology – are deeply rooted in social structure, and the role of the intellectual is to work with citizens in promoting fundamental change. (Davis and Mathews 1979:5)

Closely related to their stance is that of those who use the term "liberation anthropology" (Huizer 1979; cf. Frank 1969) to describe their activist approach to helping achieve the emancipation and autonomy of indigenous and other oppressed peoples – in analogy to the "liberation theology" of certain Christian clergy and lay people in Latin America (Berryman 1987). Bodley (1982:191–216) has proposed a useful typology of activist anthropological approaches and those who employ them: the "Conservative-Humanitarian," the

"Liberal-Political," and his own commitment, the "Primitivist-Environmentalist." I leave it to the interested reader to pursue the definitions and philosophies of these perspectives and their implications for, and illuminations of, the ethical practice of anthropology.

The recent impetus for redefinition of anthropological ethics comes clearly from those outside of academia who find the Principles of Professional Responsibility to be inconsistent with the demands of their employment. Often calling themselves "practicing anthropologists," they work primarily for corporations, government agencies, and other mission-oriented employers whose priorities, they point out, are not consistent with those confronted by scholar-anthropologists.[10] For them, anthropological ethics as heretofore defined are inconvenient, constraining, even threatening, both to their missions and to their careers. Corporate ends are profits; governmental goals are national political, and international geopolitical, advantage. Anthropologists have something of value to offer in the pursuit of these "real-world" ends, for they know about the peoples and cultures that will provide the work force and customers, the allies and adversaries. They know the populace that will decide the political and economic alignment of its government; who occupies the territory that contains the sought-after resources; and which will be the sites of the military bases and missile silos. As these corporate and national priorities become anthropologists' priorities, no wonder that the subjects of study no longer come first, that secrecy and even clandestinity are no longer condemned, that anthropologists become wary of being held accountable to their colleagues for the ethical practice of their profession, that they do not feel obliged to share with society as a whole the implications of their professional knowledge, and that they view the very concept of ethical standards with anxious skepticism. Anthropologists in these roles are agents of their employers, not advocates for those they study or for the principles of their profession.

It is scarcely surprising, then, that the draft code, arising as it did in response to these circumstances, devotes nine of its new provisions to matters of "professional relations" and only four to the people among whom anthropologists work, whether in practice or in research. This is a major reversal in direction from the focus of the Principles of Professional Responsibility.

Evidently, a code of ethics for the era of practicing anthropology must not subordinate the requirements of the marketplace and realpolitik to mere adherence to principle. The question then is whether this draft code speaks for anthropologists as humane students and advocates of humankind, or as bureaucrats and human engineers. Are there to be two kinds of anthropologists, practicing and humanist; two kinds of ethics, practical and humane; two kinds of anthropology, laissez-faire and principled? I think we should resist such a division as unnatural, unnecessary, and counterproductive. Resistance to it is in the public interest, in our professional interest, and in our individual interests. The demands and opportunities of careers in anthropology, be they in practice or in academia, must not substitute for ethics.

To the extent that anthropologists can remain true to their principles while practicing their profession outside of the academy, I would urge them to follow their career preferences and opportunities. To the extent that they cannot, I believe they must curtail their career choices. There is no place anywhere for unprincipled anthropology or anthropologists.

## Conclusion

In conclusion, let me emphasize that I am not opposed to revision of our code of ethics, the Principles of Professional Responsibility. I *am* opposed to the abandonment of the spirit of ethical practice of anthropology which it embodies, and the tenets which codify that spirit. Specifically, I am steadfastly opposed to compromise or elimination of what I believe to be the four fundamental principles of our ethics as anthropologists, all of which are prominent in the PPR and in the history of anthropology, and all of which are conspicuously absent from the draft Code of Ethics which was proposed to replace it. They are well worth repeating:

(1) That "anthropologists' paramount responsibility is to those they study" (PPR 1).
(2) That "anthropologists should undertake no secret research" (PPR 3a) and "should avoid even the appearance of engaging in clandestine research" (PPR 3b).
(3) That anthropologists are accountable for their professional actions: "when anthropologists... betray their professional commitments [as set forth in the PPR] ... their colleagues may legitimately inquire into the propriety of those actions, and take such measures as lie within the legitimate powers of their Association as the membership deems appropriate" (PPR Epilogue).
(4) That anthropologists bear "the positive responsibility to speak out publicly... on what they know and... believe as a result of their professional expertise.... That is, they bear a professional responsibility to contribute to 'an adequate definition of reality' upon which public opinion and public policy may be based" (PPR 2d).

I see the PPR as structurally analogous to the United States Bill of Rights or the Constitution as a whole: a basic document subject to review and revision by amendment proposed from the membership either directly or through its representatives on the Executive Board, when ratified by vote of the membership at large. But I believe it to be a drastic mistake for a committee to be appointed to create a substitute code, *in toto*, in response to what are said to be the changed realities of the moment – in this case, primarily changes in the anthropological marketplace. This is exactly what was done when the dCoE was proposed just ten years after the original code – the PPR – was drawn up and adopted. At this rate, our ethics would become simply fleeting reflections of the temper of the times, our code a weather vane shifting with every social and political wind, responsive to each economic fluctuation, political fad, or international spasm. If this were to happen, we would not be alone. As the editor of *New York* magazine wrote not long ago, "Ethics in America seem to have dropped to one of the lowest points in history," and "moral lapses blot American history.... But," she went on to point out, "today's go-go ethics are in many ways new. For one thing, there's little doubt that idealism is in decline and cynicism on the rise" (Kanner 1986:9). Surely anthropologists, as students of humankind, as practitioners of what Eric Wolf (1964:88) called "the most scientific of the humanities, the most humanist of the sciences," are more principled than to jump on that bandwagon; surely our ethical commitments are more profound, independent, and stable than that; surely those among whom and in whose interests we work deserve better than that.

In short, I believe the draft Code of Ethics constituted an evisceration of the PPR and, as such, betrayed our ethical principles and was unworthy of our discipline and our profession. It proposed to sacrifice the nobility of the politics of truth for the perversity of realpolitik. It seems that the era of Reaganomics spawned the nightmare of Reaganethics. I trust that history will soon be enabled by our profession's collective decision to relegate this episode to the dustbin of other short-lived aberrations of the 1980s.

# 22

# Healing Dilemmas

## *Donald Pollock*

Anthropologists who provide medical care in the field are confronted with at least two important types of ethical dilemma, a practical one and a representational one. The practical dilemma emerges from the fact that most of us who choose, or are called upon, to provide health care to our indigenous hosts possess neither the knowledge nor the facilities for the practice of medicine at any level of significant competence, yet we are compelled by our own faith in western biomedicine to wish for its benefits to be available to our hosts. As well, like most anthropologists we also face an ethical dilemma of representation. On the one hand, by representing indigenous healers as "shamans" or "witch doctors," we draw attention to the culturally situated nature of the roles played by such figures in any society, roles that are almost never limited to "healing" in our western sense. But we represent them in this way only at the risk of implying that, as shamans or witch doctors, indigenous healers practice in ways that are so culturally embedded that they have no relevance in other cultural contexts, while our own medical practices remain as effective in the field as they are at home. We assume that penicillin "works" *in spite of* belief in it; waving a dead chicken at the aurora borealis is effective, if at all, only *because of* belief in it. Both of these kinds of dilemmas are captured in the suggestion by Jeffrey Ehrenreich that we consider the anthropologist in the field to be the "witch doctor," a figure who may appear to possess extraordinary power, but who is, from a slightly different perspective, a rather comic manipulator of empty symbols.

The practical dilemmas of amateur doctoring are fairly common for anthropologists, certainly for those working in lowland South America, but there appear to be few discussions of the ethical issues entailed (McCurdy 1976). When I arrived in the field for the first time, the most serious medical issue I had ever dealt with was choosing among over-the-counter cold medications and antacids, yet in the field I was expected to manage a 40-day course of therapy for a three-year-old's leishmaniasis, to recommend treatment options for people I thought might have tuberculosis, to treat what I assumed to be gonorrhea, and to dispense a variety of powerful antibiotics, narcotics, and other drugs, some of them banned in the United States. Although my opportunities to do any serious damage were limited, I believe, by the basically good health of the people I worked among and the availability of useful handbooks such as *Where there is no doctor*, the ethical dilemmas remain and are more complex than the simple goal of doing no harm (Pollock 1988).

In this article I discuss a related ethical doctoring dilemma, a dilemma of power. It confronts us when we assume that many

Donald Pollock, "Healing Dilemmas," pp. 149–57 from *Anthropological Quarterly* 69:3 (1996). Copyright © 1996 by the American Anthropological Association.

indigenous communities accept our medical assistance without any special faith in its efficacy, whether that care is provided by amateur physicians or professionals. In this form it has been elevated to the status of a central colonial offense. Megan Vaughn (1991), for example, discusses the ways in which the western medicine brought by missionary physicians to nineteenth-century African peoples was used to legitimate the imposition of Victorian morality, standards of dress, sexuality, diet, and even thought on indigenous cultures. Jean Comaroff (1992), similarly, has exposed the role of nineteenth-century medicine in offering a "humane" facade to the brutalizing regimes of colonialism in South Africa. While the process of objectification that colonized peoples undergo through their experience with western medicine is one that colonizing peoples themselves undergo in every medical interaction, the potential of medicine to legitimate a moral order of humanity in which white northern Europeans occupy the highest position has also rendered the exercise of medical power in transcultural settings ethically problematic.[1]

The dilemma of power also arises for anthropologists when our hosts request or even demand the benefits of western biomedicine, and when we find ourselves coerced, manipulated, or ordered to provide medical assistance. This is a dilemma that arose for both myself and for my Kulina Indian hosts. It arose because the health of his village is the responsibility of a Kulina headman. When, as among the Kulina, illness is caused by witches in enemy villages, when it is a political force inscribed within the body and soul, the control of access to diagnosis and treatment is a significant source of power, especially for the village headman. The anthropologist, possessor of symbols of power – machines, money, medications – however powerless in her or his own belief, may represent a considerable threat to indigenous structures of power, one that must be brought under control by coercion, marginalization, or even expulsion.

This dilemma is, as far as I am aware, almost unique to anthropologists and other long-term visitors to traditional, indigenous communities, and there appear to be no good precedents in the literature on medical ethics to guide the fieldworker. Perhaps the most similar cases in medical ethics concern the paramedics or even less well-qualified individuals who are sometimes called upon to render extraordinary forms of medical care, even surgery, during war, on ships, or elsewhere. But in these cases there is usually a shared understanding between the care giver and the care receiver of the nature and level of treatment possible, and of the relative consequences of doing something versus doing nothing. This situation is unlike the anthropological setting, in which there is usually a complete lack of shared belief about the nature of health and illness, the nature and level of medical care available, and the possible outcomes of various alternatives.

Another possibly relevant ethical situation in mainstream western medicine is the reluctance or refusal of some physicians to treat patients infected with HIV, a situation that is similar through a kind of reversal or inversion.[2] In that case, physicians who possessed the technical medical skills and knowledge of disease, and who fell under an ethical obligation to treat patients, nonetheless refused. In the case of fieldworkers, anthropologists who have few or no technical skills and little knowledge of disease, and who have only the weakest ethical obligation, at best, to treat sick people (and traditionally strong ethical obligations not to impose western culture on indigenous communities), nonetheless plunge right in and play doctor. However, the example of physician refusal to treat HIV patients only allows us to delimit the ethical dilemma, not necessarily to resolve it.[3] While the physician confronting AIDS possesses very specialized knowledge and skills, these have been ineffective in curing the disease; the anthropologist, on the other hand, often feels that even the serious illnesses contracted by members of an indigenous community could be treated successfully by western medicine, if only the anthropologist had the knowledge and skill to diagnose and treat them properly. I recall all too well the frustration I felt when confronted by a case of (I believe) pneumonia and my assumption that my antibiotics could cure it, if only I knew more about the course of the disease, which drugs to use, what dosages, and so on. Further, while physicians may have responded with anxiety or fear to AIDS and the homosexuality of many of its victims, anthropologists may experience something like the opposite reaction to indigenous patients and their illnesses. After all, many indigenous Amazonians suffer greatly from introduced illnesses such as the common cold, which scarcely slow down the western anthropologist. Perhaps we are compelled to provide amateur medical care out of a combined sense, usually implicit, that most of the illnesses we confront in the field are relatively mild or self-limiting, and that our treatments are essentially benign. Far from responding with anxiety toward our indigenous hosts, anthropologists have historically been criticized for slipping into more of a paternalistic attitude, again normally tacit and unexamined. Both cases seem to involve a paradox or dilemma of power.

An incident highlighting this dilemma of power occurred early in my fieldwork among Kulina. When I first arrived in the village called Maronaua to begin my research in 1981, a resident missionary couple associated with the Catholic Conselho Indigenista Missionário (CIMI) had been providing regular, basic medical care for members of this community of about 120 people. However, a few weeks after my arrival, the couple left for an extended break and asked me to keep the key to their small storeroom of medical supplies. The day after their departure I was roused at dawn by the headman of the village, who announced that we were going to "look for sickness." My sleepy inclination, and my American cultural experience, was that sick people could make their own way to the doctor. If I was to play the role, let them come to me, I suggested to the headman. He became animated, insisting that we go from household to household, looking for the sick. I became adamant: I was pretty sure no one was sick enough to need any kind of medical care, and I wasn't about to encourage the kind of abusive over-use of health care that Americans are so notorious for, at least not at dawn. But when the headman's animation began to reach a hysterical level, it occurred to me that my welcome in the village would be rather short-lived if I failed to accept his invitation, and I climbed out of my hammock. As I expected, no one that morning reported any problem for which they wanted my treatment, and even on subsequent mornings the problems I was usually offered were minor cuts and rashes, the Kulina having noticed that western antibiotic powders and washes were often effective for treating breaks in the skin. It was otherwise difficult to convince people that anything I carried in my little medical box would be curative in any more profound sense. Despite my occasional intervention in cases of serious illness – the tuberculosis, the leishmaniasis – my medical role was generally marginal and heavily controlled.

Part of the reason for this lies in Kulina notions of illness. Kulina lack an overarching category of "illness," though they often use the Portuguese term *doença* in more or less the way a Brazilian would. Instead, Kulina recognize three major kinds of conditions that produce the physical discomforts of illness (Pollock 1994; 1996). The first of these links two potentially fatal illnesses that can only be cured by shamans: *dori*, which is caused by enemy witches, and *epetuka'i*, which afflicts infants when their parents eat the meat of male animals. Because these are illnesses that can be cured only by shamans, and even then only by considerable effort, and because I was so obviously lacking in any shamanic abilities whatsoever, Kulina never called upon me to treat anyone with dori or epetuka'i. Indeed, when Katore, an elderly man long resident in Maronaua, developed a respiratory infection that progressed to pneumonia, and my aspirins and antibiotics could not reverse his decline, the village shamans redefined his illness as dori, as a witchcraft attack, and more or less elbowed me aside to begin the curing rituals that they hoped would be effective. When Katore died a few days later, the major village headman reassured me that there was nothing that my western medications could have done to cure Katore's illness.

The second variety of illness consists of a very wide range of relatively mild conditions that include skin sores, cuts, and rashes: these share the feature of being on the outside of the body, in particular on the skin, in contrast to dori and epetuka'i, which strike deep within the body. These milder conditions are treated primarily by plant medications, often by the direct application of masticated leaves or a kind of sponge bath of a leafy infusion. This kind of medical knowledge is possessed by everyone. Many of these external conditions respond quickly to western antibiotics and antiseptics, and Kulina recognize and value the use of such medications for these non-threatening illnesses.

Finally, Kulina recognize a variety of introduced illness – measles, colds, mumps – and believe that only "foreign" medications are truly effective in curing them. One day a member of my village, for example, brought his daughter, a little girl who appeared to have contracted mumps, to me. I insisted that there was nothing I could do to cure her illness, but her father insisted that I must know how to cure this condition, just as he knew how to cure any of the wide range of "indigenous" illnesses Kulina contract. When I declined to give the little girl an injection of *penisilina* – the generic term for all injected medications – he and his family packed their belongings in a canoe and took off downriver to look for assistance "from a Brazilian." They returned more than a month later and told me that a week after leaving the village they found someone who gave the little girl an injection (of what I have no idea), and, as they expected, the next day her illness was cured. It took them nearly three weeks to paddle their canoe back upriver to the village. They never believed me when I told them that the illness cured itself in the week they were searching for someone to help, and I was left wondering if I should have saved them this extraordinary effort by simply giving their daughter an injection of distilled water with the warning that it took a week to work. I also realized that I had succumbed to the power of the headman but felt free to decline the (equally unreasonable) request of a politically powerless member of the community.

Not surprisingly, the division of illness into three varieties inscribes in the Kulina person the major dimensions of a political geography. Dori and epetuka'i are thought of as deeply internal, not only within the body, but also within the body politic. They are illnesses that represent general failures of sociability within Kulina life, within households (a child's epetuka'i is said to be caused by its parents' violation of extended postpartum food taboos, but it is diagnosed most often when a husband or wife is found to have committed some breach of domestic propriety, such as adultery), within a village, or between Kulina villages (dori represents an attack from an enemy shaman/village, but if the victim dies it is assumed that the witch is a member of the victim's village). By contrast, illness that appears on the margins of the body – the cuts, rashes, sores, and stings that are said to be on the skin – describe the limits of proper contact with the natural world: they are said to result from thoughtless or inappropriate contact with the wild domains that surround the sociable village. Finally, introduced illnesses – colds, flu, measles – are the embodiment of the consequences of contact with non-Kulina. In a cultural world that contrasts the deeply internal against the diffusely external, and ordered sociability against wild and dangerous "nature" (a category within which Kulina include non-Indians), these three varieties of illness trace a map of political geography onto the body.

One implication of this view of illness is that those whose responsibility it is to insure the political stability of the village are also those who are responsible for the treatment of illness, especially those illnesses that strike as a consequence of disrupted sociability. Among the Kulina all men are nominally shamans, but it is the headman of a village who is ultimately responsible for both its political stability and the health of its members, tasks which, in effect, are not separate (Pollock 1992). The headman who roused me from my fieldworker dreams of pizza and chocolate cake was perhaps less interested in the effectiveness of my medicine than in maintaining overtly public control over a potential source of medical treatment. My own naive assumption, that medical care had simply an instrumental function, not only is false for Kulina, but is equally false within our own culture. In that regard my dilemma of doctoring in the field takes on a different dimension. When medical care has a powerfully symbolic component, when the very provision of medical care is a deeply valued act of sociability assumed of all consociates, the question of whether to provide such care is not a simple matter of gauging how far a situation taxes one's medical knowledge. It is, rather, one of those fieldwork contexts in which we are challenged to display our fundamental humanity, and our willingness to act toward others as a fellow member of a community.

In this regard the headman of Maronaua may have had a more insightful understanding of the nature of medical practice than I did at the time. We tacitly assume that western medicine has primarily instrumental value, that it "works" independently of our belief in it. But western biomedicine is no different from any other ethnomedical system in comprising a complex set of symbolic and social practices that embed or encode a variety of cultural messages. Payer's comparison of biomedicine in the United States, England, France, and Germany (Payer 1988; cf. Lock and Gordon 1988), for example, reveals numerous ways in which cultural values in each setting differentially shape the understanding of illness and the practice of medicine among physicians who, in every other regard, appear to share the basic "scientific" knowledge of disease and therapeutics, and who may even read the same medical literature. Ohnuki-Tierney's study of biomedicine in Japan is perhaps even more revealing of the ways in which cultural conceptions of personhood, status, and power shape the ways in which scientific biomedicine is practiced (1984). In Ohnuki-Tierney's example, hospitalized patients, to take only a single instance, lose few of the signs of identity and status that are regularly stripped from the American hospital patient: Japanese patients wear their own clothing, use their own bedding, eat meals cooked by family members, and receive extensive visits from family, friends, and co-workers. The American way of hospitalization, so often justified within medicine as a set of clinically useful techniques to insure patient passivity and receptiveness to "doctor's orders," also reproduces basic dimensions of American notions of identity, personhood, and status, largely by subverting them. Western biomedicine, as a form of practice, is thus shaped by and in turn reproduces distinctly western forms of personhood and status by locating disease inside of individual bodies rather than in the social spaces between them or in groups, by surrounding medical interactions with intense privacy and secrecy, by relying on quantification of signs over subjective evaluation of symptoms, by subordinating patients to the authority of experts, and by legitimating medical competence through credentials rather than through practice.[4] The village headman appreciated the performative dimensions of medical practice and enrolled me to enact them. I naively ignored the pragmatics, the "ritual" values of medicine as a form of signifying practice, and appealed to the rather secondary instrumental values it might offer in this case. As several commentators have noted, the sugar pill is not the placebo; the *giving* of the sugar pill is the placebo (Brody 1980; Hahn and Kleinman 1983).

If the very act of "doing" medicine is potentially beneficial, the form of its doing is still culturally specific. As a typically oral American, I first brought to Maronaua medications in oral form: everything from aspirin to antibiotics in pills, tablets, and capsules. Kulina rejected most of these, preferring injectable medications; substances taken by mouth were believed to have no therapeutic potential, since they merely become transformed into rather unpleasant wastes that emerge at the other end of the body. Moreover, injectable medications seemed to "work" just like traditional medications, by entering directly through the skin to the site of the illness.[5] Ignorance of this preference for injections may have been the source of the greatest medical damage I caused in the village, when I would discover much later that people had failed to take the pills I gave them.

I've described a community in which I was seen to possess numerous signs of power – tape recorder, typewriter, camera, medications – yet I was myself a relatively powerless figure. My medical role in the village seemed to me less one that I chose than one that was assigned to me by the headman. As I reflect on my interactions with this community, it seems to me that I was not the one who felt any acute ethical dilemma about that medical role, but rather that I offered a solution to an ethical challenge for the headman, one that, as in my earlier anecdote, was negotiated through my possible medical role in the village.

Shortly before my arrival in that first village in which I worked, its population had split over a series of disputes regarding the presence of missionaries in the community, and members of one faction – though not all the members of that faction – had moved to an abandoned rubber-tapping camp about half an hour downriver, which they declared off-limits to all non-Indians, especially missionaries. The leaders of the two factions were brothers; the older brother was universally acknowledged to be the primary headman, but it was he that took the group of his supporters downriver, leaving his politically weaker younger brother behind in the main village. The younger brother had a self-satisfied kind of arrogance in the early days of the split, but this soon turned into chronic, anxious suspicion and paranoia. That he was a rather weak leader was evident whenever his older brother would return to the main village for a visit; on such occasions people openly praised his older brother's dynamic style and impressive oratorical skills, and he would be forced to take a secondary role for the duration of his brother's stay.

This was an opportune time for an anthropologist interested, as I was, in ethnomedical beliefs and practices, since the social violence done to the community provoked a massive epidemic of dori illness, of witchcraft attacks that were treated more or less every night by the major village shamans. Indeed, the village was trapped in a kind of vicious cycle in which the suspected presence of illness due to witchcraft would bring the older brother back, out of concern for his close kin, yet the tension that surrounded his visits was certainly one of the major social sources of the witchcraft illness attacks. In this context it became evident that an array of political issues were being articulated within an illness discourse. Illness became the major public language of political relations, within the village, between the two factions, between the two competing headmen, and between Kulina and Brazilians. Indeed, this was centrally a political dilemma, in which the expectations of siblingship were being pitted against political gains by two ambitious headmen. These headmen also contested for the treatment of illness in nightly sessions of shamanic healing, called *tokorime* in reference to the tokorime spirits into which shamans are transformed in the ritual. Not surprisingly, the village was plagued by illnesses that moved continually between two forms, one assumed to be caused by witches and the other by contact with non-Indians. The ambiguous form of the political debate – in which Brazilian missionaries had allies in both village factions, yet neither felt that missionaries were worth fighting over – produced a comparably ambiguous illness.

As I struggled to establish some ethnographically productive role in the village, I expected that village members, especially its leaders, would simultaneously be trying to fit me into some politically productive role. I had imagined that this effort would enlist me as a go-between with the National Indian Foundation (FUNAI) in the legal battle for land-rights, or even with the Brazilians who passed by the village from time to time. But it turned out that Kulina had little interest in my serving these functions. Rather, I was co-opted by one headman to help resolve the political dispute created over the missionaries: I would provide the medical treatment that would cure any illnesses caused by contact with outsiders, and in directing me to do so, my headman would demonstrate his control over a process of illness healing that is one of the most potent displays of a headman's power.

The other dimension of my ethical dilemma was less what I did for Kulina than my reluctance to do anything at all. Despite the fact that I did treat various illnesses occasionally during my fieldwork, I was hesitant to do so for several reasons. First, as I described at the outset of this article, I was initially unwilling to allow myself to be enrolled in the headman's fantasy of how western medical care should be provided, though in the end it was expedient for me to acquiesce. Second, I was uncertain about my ability to recognize diseases ("diagnose" is too formal a term for what I was capable of), let alone to offer much medical assistance beyond the level of simple analgesics or placebos. Third, and perhaps more importantly, I was reluctant to place myself in a role that could have profound implications for my position within this village. I want to conclude by discussing this last aspect of my ethical dilemma of power: the concern that I might endanger the health of one or more of my hosts in my effort to avoid compromising the role I felt I should be playing as an anthropologist.

The poststructuralist critique of traditional anthropological research condemns fieldworkers for exoticizing their indigenous hosts in tacit, even unconscious complicity in the neocolonial project that (re)produces the political-economic divide between the First World and the Third or Fourth. The postmodernist discussion of this problem also notes that anthropologists have had a habit of privileging their observational perspective, appealing to false stances of objectivity and disengagement and their rhetorical underpinnings in third-person, "rule" saturated ethnographic writing. From these perspectives my ethical dilemma of not doctoring in the field seems to me in retrospect to have been partly the consequence of my assumption that I could maintain the traditional detached researcher's view, marginal yet informed, observing all around me without becoming involved in, let alone a part of it.

More generally, my reluctance to become involved in medical care was only one aspect of a broader problem of negotiating a position somewhere between that of the missionaries resident in the village and that of the National Indian Foundation. In this region of Brazilian Amazonia, in the state of Acre, indigenous communities were, at that time, not yet beginning to undergo the cultural and social dislocation associated with deforestation and large-scale immigration that has threatened communities in neighboring states such as Rondônia. FUNAI, which has nominal responsibility for the welfare of indigenous groups, was badly understaffed in Acre, and was made outright unwelcome by a number of Indian communities, including the political faction that remained resident at Maronaua. With no immediate threat to the security of indigenous communities in much of the state, and with public opinion in many communities being hostile to the government, FUNAI found it expedient to tolerate the unauthorized and technically illegal presence of CIMI missionaries in many villages such as Maronaua. Provision of western-style medical care was a major priority for CIMI missionaries, a part of their overall goal of establishing economically secure, politically active, independent, and healthy indigenous villages (Pollock 1993), one that happened to coincide as well with FUNAI's mission.

I reacted to missionary involvement in providing medical care in rather cynical terms. There seemed to be few serious medical problems that required the extensive pharmacopeia and technical medical resources that the missionaries maintained; their storeroom of medical supplies reminded me of nothing so much as Malinowski's description of the large *bwayma*, the large structure in the middle of the village that Trobriand chiefs kept full of yams which would ultimately rot in a conspicuous display of power and wealth. But mostly I was reluctant to become identified with the missionaries and their project when I saw that medical care was a way that missionaries ingratiated themselves with a community.

The CIMI missionaries resident in Maronaua complained about the medical work of Protestant missionaries of the US-based Summer Institute of Linguistics (SIL) who had many years earlier established themselves in a Kulina village several days' canoe trip upriver in Peru, a village called San Bernardo. The SIL missionaries, it was said, charged Kulina for medical care. I cannot confirm this, except to say that Kulina from San Bernardo visiting in Maronaua also reported this practice, and indeed sometimes came to Maronaua (a faster trip down-river) seeking medical care for which they could not pay at home. The ethics of charging for medical care in such a setting are complex and beyond the scope of this article, but I should note that the practice has to be evaluated within the context of a larger SIL strategy of establishing small-scale, surplus-producing, market capitalist economies in indigenous communities which not only become linked to surrounding markets, but also seem to offer a more receptive set of social conditions for Christianization, especially Protestantization.[6] Nonetheless, I am not sure that CIMI generosity in freely providing medical care was not also predicated on the implicit expectation that various return prestations – of political support, shelter, food – would be made. It might be argued that the expectation of diffuse returns for medical care can be even more insidious than the expectation of immediate payment; the latter terminates an interaction with no additional obligations, while the former, in interactions with politically engaged non-Kulina such as CIMI missionaries, creates enduring, unspecified obligations that these missionaries did not hesitate to define and assert when necessary. It was this system that I was loath to enter.

The other role available to me was to become identified with the National Indian Foundation as a kind of official representative who could provide medical care as an aspect of FUNAI's mission. Indeed, FUNAI itself had authorized my initial research largely, I believe, to have a quasi-official observer in the region who could be expected to report back on both indigenous and missionary issues. This reputation preceded me, and I was greeted by Kulina upon my first arrival in Maronaua as "Funai" and was approached by one of the village leaders who demanded to know what I would do about government promises to the village. I immediately distanced myself from FUNAI associations, but then left rather open the question of my specific identity. With such an ambiguous role in the village, it was easy for Kulina to project on to me whatever they felt was appropriate or expedient, including the demand that I provide medical care during the CIMI missionaries' frequent absences. This, again, was a role I was reluctant to play.

In pondering the ethical issues of anthropologists playing amateur doctor in the field, I turned to an old friend, Dr. Howard Brody, a physician and medical ethicist who has been writing about medical ethics for twenty years. Howard's first response was to wonder if anthropologists did not adopt the same kind of radical cultural relativism about medicine that they did about other aspects of indigenous cultures: why, in other words, would an anthropologist who declines to pass moral judgment on infanticide or polygamy, who is hesitant (at least in the popular stereotype) to bring matches into a community that rubs sticks together to make fire, nonetheless be willing to provide medical care even in a professional, let alone amateur way? When I suggested that the anthropologist's own faith in the value of western medicine and concern for the well-being of indigenous hosts, friends, and neighbors seems to overwhelm this relativistic stance, he mentioned the old joke about the professor of decision theory who was offered two attractive jobs but was unable to choose between them. A colleague noted that this professor had built a career on developing sophisticated models of decision-making, so why not apply the same techniques to the two job offers? The professor replied that while one could theoretically apply the same decision-theory models to his own dilemma, this problem was really important. Howard's observation, without the humorous context, was that we sometimes set aside our academic, textbook ethics and our theoretical convictions when we are faced with the real problems of real people.

This is certainly the case in lowland South America. As Alcida Ramos (1990) has noted, Brazilian anthropology is nearly inseparable from the politics of indigenous rights in that country, and Brazilian anthropologists are, more so than their North American colleagues, actively involved in the promotion and defense of indigenous communities. Such Brazilian anthropologists have a profound sense of what is "really important," that the rights of indigenous peoples, their survival, safety, and well-being, are often incompatible with the rather old-fashioned perspective of anthropological non-involvement in local cultures. Indeed, in the context of struggles over the future of indigenous peoples in Amazonia, one may be forgiven for believing that the dangers posed by the anthropologist playing amateur doctor in the field may be insignificant. After all, anthropologists who are not economists, who are not policy experts, who are not political scientists (or politicians) nonetheless work on behalf of indigenous communities for whom political, economic, and policy decisions will have profound and long-term consequences.

This attitude also drives applied and "practicing" anthropologists, of course, but the extension of such interventionist activities in the transcultural setting of illness has been considerably more problematic. Anthropologists regularly participate in large-scale public health projects in other societies, but the ethical dilemmas they present are significantly different from those of amateur medical practice in the field. Indeed, the notion of a "clinical anthropology" that emerged in the early 1980s located this form of practice in largely western clinical settings, in which anthropologists were assumed to be part of more complex teams of providers, serving as general experts on cross-cultural communication or on the culture of specific patient groups (Alexander 1979; Chrisman and Maretzki 1982; Shimkin and Golde 1983). The ethical issues presented by such transcultural medical encounters have been discussed within the framework of informed consent (Kaufert and O'Neil 1990; cf. Kunstadter 1980), but have focused on medical practitioners rather than anthropologists acting alone.

As I reflect on my own "medical" work among Kulina, I am struck by the aptness of the "witch doctor" label. Kulina certainly thought my own doctoring had as much to do with healing the social body as with curing any individual. I suppose that when we consider how our own ethical dilemmas arise in the field, we should remember that we are also being manipulated, enticed, and coerced by our hosts, enlisted to resolve their own ethical dilemmas and social conundrums. I left the village that first time reasonably confident that my amateur doctoring had not done anybody any harm. But I also felt that my willingness to treat illness at times of social upheaval was a useful if small part of the headman's efforts to relieve a community's anxiety about some of the particularly harmful consequences of contact with non-Indians, and in the process helped two competing headmen find at least a temporary respite from the harmful consequences of political conflict. In the intervening years, it has become increasingly clear that the greatest threat to the welfare of the Kulina – as to all Brazilian Indians – lies less in the small-scale, amateur medical work of well-meaning anthropologists than in the tragic processes of deforestation, population shifts, and "development" that are devastating habitats and communities. Here again my aspirins seem no more effective than a witch doctor's rattle.

# 23

# Code of Ethics

## *American Anthropological Association*

### I Preamble

Anthropological researchers, teachers and practitioners are members of many different communities, each with its own moral rules or codes of ethics. Anthropologists have moral obligations as members of other groups, such as the family, religion, and community, as well as the profession. They also have obligations to the scholarly discipline, to the wider society and culture, and to the human species, other species, and the environment. Furthermore, fieldworkers may develop close relationships with persons or animals with whom they work, generating an additional level of ethical considerations.

In a field of such complex involvements and obligations, it is inevitable that misunderstandings, conflicts, and the need to make choices among apparently incompatible values will arise. Anthropologists are responsible for grappling with such difficulties and struggling to resolve them in ways compatible with the principles stated here. The purpose of this Code is to foster discussion and education. The American Anthropological Association (AAA) does not adjudicate claims for unethical behavior.

The principles and guidelines in this Code provide the anthropologist with tools to engage in developing and maintaining an ethical framework for all anthropological work.

### II Introduction

Anthropology is a multidisciplinary field of science and scholarship, which includes the study of all aspects of humankind – archaeological, biological, linguistic and sociocultural. Anthropology has roots in the natural and social sciences and in the humanities, ranging in approach from basic to applied research and to scholarly interpretation.

As the principal organization representing the breadth of anthropology, the American Anthropological Association (AAA) starts from the position that generating and appropriately utilizing knowledge (i.e., publishing, teaching, developing programs, and informing policy) of the peoples of the world, past and present, is a worthy goal; that the generation of anthropological knowledge is a dynamic process using many different and ever-evolving approaches; and that for moral and practical reasons, the generation and utilization of knowledge should be achieved in an ethical manner.

The mission of the American Anthropological Association is to advance all aspects of

American Anthropological Association, *Code of Ethics of the American Anthropological Association* (Washington, DC: American Anthropological Association, 1998). Copyright © 1998 by the American Anthropological Association.

anthropological research and to foster dissemination of anthropological knowledge through publications, teaching, public education, and application. An important part of that mission is to help educate AAA members about ethical obligations and challenges involved in the generation, dissemination, and utilization of anthropological knowledge.

The purpose of this Code is to provide AAA members and other interested persons with guidelines for making ethical choices in the conduct of their anthropological work. Because anthropologists can find themselves in complex situations and subject to more than one code of ethics, the AAA Code of Ethics provides a framework, not an ironclad formula, for making decisions.

Persons using the Code as a guideline for making ethical choices or for teaching are encouraged to seek out illustrative examples and appropriate case studies to enrich their knowledge base.

Anthropologists have a duty to be informed about ethical codes relating to their work, and ought periodically to receive training on current research activities and ethical issues. In addition, departments offering anthropology degrees should include and require ethical training in their curriculums.

No code or set of guidelines can anticipate unique circumstances or direct actions in specific situations. The individual anthropologist must be willing to make carefully considered ethical choices and be prepared to make clear the assumptions, facts and issues on which those choices are based. These guidelines therefore address *general* contexts, priorities and relationships which should be considered in ethical decision making in anthropological work.

## III Research

In both proposing and carrying out research, anthropological researchers must be open about the purpose(s), potential impacts, and source(s) of support for research projects with funders, colleagues, persons studied or providing information, and with relevant parties affected by the research. Researchers must expect to utilize the results of their work in an appropriate fashion and disseminate the results through appropriate and timely activities. Research fulfilling these expectations is ethical, regardless of the source of funding (public or private) or purpose (i.e., "applied," "basic," "pure," or "proprietary").

Anthropological researchers should be alert to the danger of compromising anthropological ethics as a condition to engage in research, yet also be alert to proper demands of good citizenship or host-guest relations. Active contribution and leadership in seeking to shape public or private sector actions and policies may be as ethically justifiable as inaction, detachment, or noncooperation, depending on circumstances. Similar principles hold for anthropological researchers employed or otherwise affiliated with nonanthropological institutions, public institutions, or private enterprises.

### *A Responsibility to people and animals with whom anthropological researchers work and whose lives and cultures they study*

1 Anthropological researchers have primary ethical obligations to the people, species, and materials they study and to the people with whom they work. These obligations can supersede the goal of seeking new knowledge, and can lead to decisions not to undertake or to discontinue a research project when the primary obligation conflicts with other responsibilities, such as those owed to sponsors or clients. These ethical obligations include:

- To avoid harm or wrong, understanding that the development of knowledge can lead to change which may be positive or negative for the people or animals worked with or studied
- To respect the well-being of humans and nonhuman primates
- To work for the long-term conservation of the archaeological, fossil, and historical records
- To consult actively with the affected individuals or group(s), with the goal of estab-

lishing a working relationship that can be beneficial to all parties involved

2 Anthropological researchers must do everything in their power to ensure that their research does not harm the safety, dignity, or privacy of the people with whom they work, conduct research, or perform other professional activities. Anthropological researchers working with animals must do everything in their power to ensure that the research does not harm the safety, psychological well-being or survival of the animals or species with which they work.

3 Anthropological researchers must determine in advance whether their hosts/providers of information wish to remain anonymous or receive recognition, and make every effort to comply with those wishes. Researchers must present to their research participants the possible impacts of the choices, and make clear that despite their best efforts, anonymity may be compromised or recognition fail to materialize.

4 Anthropological researchers should obtain in advance the informed consent of persons being studied, providing information, owning or controlling access to material being studied, or otherwise identified as having interests which might be impacted by the research. It is understood that the degree and breadth of informed consent required will depend on the nature of the project and may be affected by requirements of other codes, laws, and ethics of the country or community in which the research is pursued. Further, it is understood that the informed consent process is dynamic and continuous; the process should be initiated in the project design and continue through implementation by way of dialogue and negotiation with those studied. Researchers are responsible for identifying and complying with the various informed consent codes, laws and regulations affecting their projects. Informed consent, for the purposes of this code, does not necessarily imply or require a particular written or signed form. It is the quality of the consent, not the format, that is relevant.

5 Anthropological researchers who have developed close and enduring relationships (i.e., covenantal relationships) with either individual persons providing information or with hosts must adhere to the obligations of openness and informed consent, while carefully and respectfully negotiating the limits of the relationship.

6 While anthropologists may gain personally from their work, they must not exploit individuals, groups, animals, or cultural or biological materials. They should recognize their debt to the societies in which they work and their obligation to reciprocate with people studied in appropriate ways.

## B *Responsibility to scholarship and science*

1 Anthropological researchers must expect to encounter ethical dilemmas at every stage of their work, and must make good-faith efforts to identify potential ethical claims and conflicts in advance when preparing proposals and as projects proceed. A section raising and responding to potential ethical issues should be part of every research proposal.

2 Anthropological researchers bear responsibility for the integrity and reputation of their discipline, of scholarship, and of science. Thus, anthropological researchers are subject to the general moral rules of scientific and scholarly conduct: they should not deceive or knowingly misrepresent (i.e., fabricate evidence, falsify, plagiarize), or attempt to prevent reporting of misconduct, or obstruct the scientific/scholarly research of others.

3 Anthropological researchers should do all they can to preserve opportunities for future fieldworkers to follow them to the field.

4 Anthropological researchers should utilize the results of their work in an appropriate fashion, and whenever possible disseminate their findings to the scientific and scholarly community.

5 Anthropological researchers should seriously consider all reasonable requests for access to their data and other research materials for purposes of research. They should also make every effort to insure preservation of their fieldwork data for use by posterity.

## C *Responsibility to the public*

1 Anthropological researchers should make the results of their research appropriately available to sponsors, students, decision

makers, and other nonanthropologists. In so doing, they must be truthful; they are not only responsible for the factual content of their statements but also must consider carefully the social and political implications of the information they disseminate. They must do everything in their power to insure that such information is well understood, properly contextualized, and responsibly utilized. They should make clear the empirical bases upon which their reports stand, be candid about their qualifications and philosophical or political biases, and recognize and make clear the limits of anthropological expertise. At the same time, they must be alert to possible harm their information may cause people with whom they work or colleagues.

2 Anthropologists may choose to move beyond disseminating research results to a position of advocacy. This is an individual decision, but not an ethical responsibility.

## IV Teaching

### *Responsibility to students and trainees*

While adhering to ethical and legal codes governing relations between teachers/mentors and students/trainees at their educational institutions or as members of wider organizations, anthropological teachers should be particularly sensitive to the ways such codes apply in their discipline (for example, when teaching involves close contact with students/trainees in field situations). Among the widely recognized precepts which anthropological teachers, like other teachers/mentors, should follow are:

1 Teachers/mentors should conduct their programs in ways that preclude discrimination on the basis of sex, marital status, "race," social class, political convictions, disability, religion, ethnic background, national origin, sexual orientation, age, or other criteria irrelevant to academic performance.

2 Teachers'/mentors' duties include continually striving to improve their teaching/training techniques; being available and responsive to student/trainee interests; counseling students/trainees realistically regarding career opportunities; conscientiously supervising, encouraging, and supporting students'/trainees' studies; being fair, prompt, and reliable in communicating evaluations; assisting students/trainees in securing research support; and helping students/trainees when they seek professional placement.

3 Teachers/mentors should impress upon students/trainees the ethical challenges involved in every phase of anthropological work; encourage them to reflect upon this and other codes; encourage dialogue with colleagues on ethical issues; and discourage participation in ethically questionable projects.

4 Teachers/mentors should publicly acknowledge student/trainee assistance in research and preparation of their work; give appropriate credit for coauthorship to students/trainees; encourage publication of worthy student/trainee papers; and compensate students/trainees justly for their participation in all professional activities.

5 Teachers/mentors should beware of the exploitation and serious conflicts of interest which may result if they engage in sexual relations with students/trainees. They must avoid sexual liaisons with students/trainees for whose education and professional training they are in any way responsible.

## V Application

1 The same ethical guidelines apply to all anthropological work. That is, in both proposing and carrying out research, anthropologists must be open with funders, colleagues, persons studied or providing information, and relevant parties affected by the work about the purpose(s), potential impacts, and source(s) of support for the work. Applied anthropologists must intend and expect to utilize the results of their work appropriately (i.e., publication, teaching, program and policy development) within a reasonable time. In situations in which anthropological knowledge is applied, anthropologists bear the same responsibility to be open and candid about their skills and intentions, and monitor the effects of their work on all persons affected. Anthropologists may be involved in many types of work, frequently affecting individuals and groups with diverse and sometimes conflicting interests.

The individual anthropologist must make carefully considered ethical choices and be prepared to make clear the assumptions, facts and issues on which those choices are based.

2 In all dealings with employers, persons hired to pursue anthropological research or apply anthropological knowledge should be honest about their qualifications, capabilities, and aims. Prior to making any professional commitments, they must review the purposes of prospective employers, taking into consideration the employer's past activities and future goals. In working for governmental agencies or private businesses, they should be especially careful not to promise or imply acceptance of conditions contrary to professional ethics or competing commitments.

3 Applied anthropologists, as any anthropologist, should be alert to the danger of comprómising anthropological ethics as a condition for engaging in research or practice. They should also be alert to proper demands of hospitality, good citizenship and guest status. Proactive contribution and leadership in shaping public or private sector actions and policies may be as ethically justifiable as inaction, detachment, or noncooperation, depending on circumstances.

## VI Epilogue

Anthropological research, teaching, and application, like any human actions, pose choices for which anthropologists individually and collectively bear ethical responsibility. Since anthropologists are members of a variety of groups and subject to a variety of ethical codes, choices must sometimes be made not only between the varied obligations presented in this code but also between those of this code and those incurred in other statuses or roles. This statement does not dictate choice or propose sanctions. Rather, it is designed to promote discussion and provide general guidelines for ethically responsible decisions.

## VII Acknowledgments

This Code was drafted by the Commission to Review the AAA Statements on Ethics during the period January 1995–March 1997. The Commission members were James Peacock (Chair), Carolyn Fluehr-Lobban, Barbara Frankel, Kathleen Gibson, Janet Levy, and Murray Wax. In addition, the following individuals participated in the Commission meetings: philosopher Bernard Gert, anthropologists Cathleen Crain, Shirley Fiske, David Freyer, Felix Moos, Yolanda Moses, and Niel Tashima; and members of the American Sociological Association Committee on Ethics. Open hearings on the Code were held at the 1995 and 1996 annual meetings of the American Anthropological Association. The Commission solicited comments from all AAA Sections. The first draft of the AAA Code of Ethics was discussed at the May 1995 AAA Section Assembly meeting; the second draft was briefly discussed at the November 1996 meeting of the AAA Section Assembly.

The Final Report of the Commission was published in the September 1995 edition of the *Anthropology Newsletter* and on the AAA web site (http://www.aaanet.org). Drafts of the Code were published in the April 1996 and 1996 annual meeting edition of the *Anthropology Newsletter* and the AAA web site, and comments were solicited from the membership. The Commission considered all comments from the membership in formulating the final draft in February 1997. The Commission gratefully acknowledge the use of some language from the codes of ethics of the National Association for the Practice of Anthropology and the Society for American Archaeology.

## VIII Other Relevant Codes of Ethics

The following list of other Codes of Ethics may be useful to anthropological researchers, teachers and practitioners:

Animal Behavior Society
1991 Guidelines for the Use of Animals in Research. *Animal Behavior* 41:183–6.

American Board of Forensic Examiners
n.d. *Code of Ethical Conduct.* (American Board of Forensic Examiners, 300 South Jefferson Avenue, Suite 411, Springfield, MO 65806).

Archaeological Institute of America
1991 Code of Ethics. *American Journal of Archaeology* 95:285.

1994 *Code of Professional Standards.* (Archaeological Institute of America, 675 Commonwealth Ave, Boston, MA 02215-1401. Supplements and expands but does not replace the earlier Code of Ethics).

National Academy of Sciences
1995 *On Being a Scientist: Responsible Conduct in Research.* 2nd edition. Washington, DC: National Academy Press (2121 Constitution Avenue, NW, Washington, DC 20418).

National Association for the Practice of Anthropology
1988 *Ethical Guidelines for Practitioners.*

Sigma Xi
1992 Sigma Xi Statement on the Use of Animals in Research. *American Scientist* 80:73–6.

Society for American Archaeology
1996 *Principles of Archaeological Ethics.* (Society for American Archaeology, 900 Second Street, NE, Suite 12, Washington, DC 20002-3557).

Society for Applied Anthropology
1983 *Professional and Ethical Responsibilities.* (Revised 1983).

Society of Professional Archaeologists
1976 *Code of Ethics, Standards of Research Performance and Institutional Standards.* (Society of Professional Archaeologists, PO Box 60911, Oklahoma City, OK 73146-0911).

United Nations
1948 Universal Declaration of Human Rights.

1983 United Nations Convention on the Elimination of All Forms of Discrimination Against Women.

1987 United Nations Convention on the Rights of the Child.

Forthcoming United Nations Declaration on Rights of Indigenous Peoples.

# *Part VII*
# Multi-Sited Fieldwork

*Antonius C. G. M. Robben*

Long-term, face-to-face, holistic ethnographic fieldwork on an island or in a small community has been the hallmark of anthropology, the model for graduate training, and the standard by which professional careers are measured. This is not to say that anthropologists like Boas and Malinowski or Powdermaker and Mead were not aware that the people they studied were connected to their surrounding world through trade, travel, and power, but they were more interested in local than translocal cultures and connections. This research focus was in part epistemological, in part methodological, and in part a reflection of the times. Prolonged fieldwork was to enhance the scientific rigor and improve the empirical basis of anthropology, a discipline that was struggling to become accepted in the academy during the early twentieth century. The world also looked different from today. Certainly, people did not live in the splendid isolation which the early ethnographies seemed to suggest – a view that was thoroughly debunked by Eric Wolf (1982) in his anthropological world history – but travel, communication, and commerce did not yet have the vertiginous pace and transience of the late twentieth century which therefore became so conducive to multi-sited fieldwork.

Multi-sited fieldwork is not the same as fieldwork at multiple sites. The history of ethnographic fieldwork contains several examples of comparative research projects, such as Margaret Mead's (1935) study in three tribal cultures, and Robert Redfield's analysis (1941) of a rural–urban continuum in Mexico, but these comparative projects departed still from a bounded research universe, whether a region or a theme. Instead, translocal ethnographers go where their research takes them to create an emergent field and study object. The emphasis is on multiple connections rather than multiple sites. As George Marcus (1995:105) has stated in an insightful review article: "Multi-sited research is designed around chains, paths, threads, conjunctions, or juxtapositions of locations in which the ethnographer establishes some form of literal, physical presence, with an explicit, posited logic of association or connection among sites that in fact defines the argument of the ethnography." Marcus identifies seven modes or practices of constructing multi-sited ethnographies, namely by following the paths and movements of people (e.g., foreign correspondents hopping from crisis to crisis), things (e.g., worldwide commodity flows or music styles), metaphors (e.g., the proliferation of discourse about the immune system from medicine to other domains), narratives (e.g., the spreading of myths and stories), biographies (e.g., life histories as journeys through social and spatial contexts), and conflicts (e.g., the transnational wanderings of refugees). Finally, Marcus identifies a type of multi-sited ethnography which focuses on a strategically situated (single-site) research object which unfolds an emerging world (e.g., the ongoing conversations about factory work among working-class boys at school). The three selected contributions by Edwards, Hannerz, and Zabusky are exemplary multi-sited ethnographies situated around conflicts, people, and metaphors, but we begin with an important conceptual critique of traditional single-sited ethnographies by Gupta and Ferguson.

In their article "Beyond 'Culture': Space, Identity, and the Politics of Difference," Akhil Gupta and James Ferguson (1992) criticize the common presupposition that space, location, culture, society, and collective identity come together within one circumscribed complex, and that cultural difference implies the existence of identifiable boundaries, breaks, divisions, and discontinuities (see also Gupta and Ferguson 1997). This basic assumption raises questions about cultural differentiation within such allegedly bounded complexes, about the hybrid cultures of post-colonial societies, about the identity of groups of people (refugees, migrant workers, businessmen) who frequently cross borders, and about sociocultural change, power relations, and the construction of difference in border zones. These issues have become particularly pertinent since the acceleration of globalization processes after the 1989 fall of the Berlin Wall, which ended the Warsaw Pact and ushered in the disintegration of the Soviet Union. Today, people, products, and information cross borders unimpeded, under the approving eye of world powers, and even the undetected movement of the illicit (refugees, drugs, laundered money, and classified information) erases borders and erodes sociospatial territories. How are such communication networks deterritorializing the world, and what are the implications for traditional ethnographic interests ranging from tribal societies to inner-city barrios? If borderlands do not only exist at the perimeter of states but are wreathed through and across countries, then who are its inhabitants, how do they give content and form to their hybridized identities, and what distinguishes them, if at all, from the rest of society?

Gupta and Ferguson advocate a fundamental rethinking of the spatialization of culture through the notion of cultural difference. Cultural difference presupposes a dichotomy between "us" and "them" which quickly evolves into "here" and "there," even when our sympathies lie with "the Other" living "elsewhere." Instead, anthropologists should examine the processes that produce such differentiations in the first place, and be sensitive to the many interconnections, bridges, crossings, and shared spaces and histories that effectively undermine these dichotomies. In fact, so argue Gupta and Ferguson, borderlands are more representative of the globalized post-Cold War reality than stable, bounded states and communities. In many ways, we are all transworld transients without the stable identities we imagine we possess (see also Lovell 1998). We participate in assemblages of Venn diagrams, scapes, or multiple grids that intersect at unexpected locations, with unexpected persons, and in unanticipated power fields. The challenge to ethnographic fieldwork is, then, how to account adequately for the numerous interconnections that fan out across the world from hitherto more easily bounded research sites. Multi-sited ethnography is presented as the answer.

David B. Edwards (1994), in his article "Afghanistan, Ethnography, and the New World Order," reveals the haphazard ways in which such multi-locale ethnographies become constituted. Anthropologists have always used a networking method in fieldwork in complex societies. This approach forced Edwards to follow his research participants around the world and into the virtual spaces of the internet. During the 1980s, he had set out to study an isolated mountain village, as so many anthropologists had done for decades all over the world, but he and the community in south-central Afghanistan became wrapped up in the liberation war of the *mujahidin* guerrilla forces against Afghanistan's communist government, the Afghan army, and Soviet troops. Edwards' emotional and professional involvement with the mountain villagers took him to locations in Afghanistan where the *mujahidin* were fighting, to the city of Peshawar and several refugee camps in Pakistan, to a heterogeneous group of refugees in Washington, DC, and into two Afghan internet news groups.

Edwards grapples with the question of how to construct an ethnographic narrative that reflects the translocal qualities and reciprocal influences of these diverse field sites, and how to do justice to their intuitive connections and his in situ experiences. He is not looking for some overarching analytical structure or master narrative, even though that is generally regarded as what distinguishes anthropology from journalism, but he tries to find a style of ethnographic writing more congenial to his diverse research sites. The anonymous Afghan who posts a message on an electronic bulletin board seems to contrast sharply with the guerrilla commander visiting Washington, but are they really so different? Does Edwards know more about this *mujahid* than about the person who writes from a university-based email address in Europe? They are both political players out of place acting in foreign settings, yet connected and motivated by a deep attachment to Afghanistan. Edwards hopes to retain some of the vicissitudes and happenstance of today's globalized reality by juxtaposing his heterogeneous fieldwork experiences rather randomly. His ethnography would have been different if he had been to other sites and encountered other players, but he still remains convinced that this and other translocal renditions would capture similar connections, connections unseen in single-sited fieldwork.

Ulf Hannerz (2003), in his article "Being There . . . and There . . . and There! Reflections on Multi-Site Ethnography," illustrates the differences between single-sited and multi-sited fieldwork through his research on foreign correspondents, both the familiar faces on the evening news reporting from their permanent station in Moscow, Jerusalem, or Paris and those roving reporters who show up near a natural disaster and reappear only days later at the edge of a mass grave or makeshift refugee camp (see also Hannerz 2004). After carrying out some preliminary research in New York, Hannerz decides upon Tokyo, Jerusalem, and Johannesburg as his principal sites while realizing that he could have equally well chosen other places. Hannerz points out that this research perspective would better be labeled translocal than multi-sited because it is not the different localities themselves but their interconnections that matter the most.

Hannerz focuses on the national and international networks of foreign correspondents with local troubleshooters, who arrange cars, food, shelter, cameramen, and willing interviewees, and with translocal editors, bureau chiefs, and fellow-correspondents whom they meet at airports, offices, and television studios. He also pays attention to the collaboration between news agencies, and studies the career paths of reporters. Social networks and life histories are tried research instruments that continue to be important for multi-sited fieldwork but it relies much less on face-to-face contacts, while the involvement in activity fields and social domains is much more diffuse than in single-sited research. Arjun Appadurai (1996) has used the term "scapes" to describe unique spheres of circulation (such as financescapes, mediascapes, and ethnoscapes) that in today's world resemble a collection of Venn diagrams. It is typical of such translocal scapes that the community of foreign correspondents cannot be grasped ethnographically and holistically as is common in single-sited fieldwork. Fleeting relations, short research periods, and a patchwork of field sites yield different outcomes in different configurations. This partiality is less worrisome than it seems, so argues Hannerz, because the constant flux of people, things, and ideas in our globalized world does by itself give rise to constantly changing hybrid realities instead of the apparent regularities and structures of more stable societies examined by the earliest generations of anthropologists.

The research topics of multi-sited ethnographers are not bound to particular places, seasons, or people, and cannot rely on research participants immersed in closed communities. Interviews, impressionistic observations, text analysis, the internet, photography, and video are therefore far more important research tools than participant observation, ritual initiation, and apprenticeship. Hannerz's conclusion that "ethnography is an art of the possible" (2003:213) is as much true for single-sited, traditional fieldwork as for multi-sited research, but with the difference that translocal research topics are harder to pin down because of a greater variation in methods, informants, and locations.

Stacia E. Zabusky (2002; see also 1995) does not identify her ethnographic field in the interconnections of multiple spatial sites, such as towns, offices, and airports. Instead, she encounters perpetually moving flows of knowledge, communication, decisions, and power (which she calls widening gyres) that dissolve and displace unstable centers at one time, transform peripheries into shifting centers at another, and reassemble them elsewhere without any central authority controlling these centers and peripheries. Her article "Ethnography in/of Transnational Processes: Following Gyres in the Worlds of Big Science and European Integration" evolves around the metaphor of European integration and describes her journey in search of the most appropriate ethnographic perspective on the transnational relations between the European Union and space science.

Zabusky decided to situate herself at the Space Science Department of the European Space Research and Technology Center (ESTEC) in the Netherlands. She attends meetings, observes equipment tests, listens to scientific discussions, commutes from a nearby village to the gated scientific center, and reluctantly joins in social activities. She discovers that this site is just one of many ever-shifting locations in a dynamic web of emails, faxes, technical designs, budgets, board meetings, impromptu foreign travel, policy decisions, alliances, allegiances, and industrial interests. She cannot concentrate her research on people, places, organizations, or social fields because neither power and authority nor the flows and connections are vested in any one of them in particular. Europe and the transnational scientific community are elusive study objects. Zabusky therefore takes the space-science mission as her ethnographic focus to conduct on-the-ground, experience-near fieldwork.

Space missions are financed by the European Union member states, designed by scientists, run by engineers, and produced by national industries, yet the decision-making processes and information flows are not as clear-cut as they seem. There are many dispersed centrifugal forces at work (new scientific developments, technological innovations, changing national and European political agendas, ongoing budgetary disputes) that lead to considerable instability, uncertainty, and improvisation to make the mission a success. The space mission is like the Kula ring exchange in Melanesia, in the sense that neither the Trobrianders nor the European space scientists have a complete picture and understanding of the translocal undertakings in which they are involved. The major difference for the ethnographer of the two research objects is that Malinowski (see Chapter 3) did, while Zabusky did not, have a bird's-eye view of the whole process. Zabusky felt just as trapped among the uncontrollable forces and gyres of this amorphous whole as the scientists and engineers who were pursuing a concrete mission of space exploration.

Multi-sited fieldwork is still in its infancy but it exerts great ethnographic appeal and opens up new areas of anthropological knowledge. The possibilities are infinite as ethnographers are not bound to one particular location, community, or social group. The attraction of multi-sited fieldwork is that ethnographers are no longer obliged to spend one continuous year at a research site but may combine week- or month-long visits to particular locations with telephone conversations, observations in public spaces, a thorough search of internet sites and news groups, joint trips with research participants, and so on. The fear of a loss of standard, that anything goes once Malinowski's canon of a four-season fieldwork period in one place is abandoned, seems unwarranted as the research methodology becomes increasingly sophisticated, the peer-review process continues unabated, and growing numbers of ethnographers study comparable themes and subjects.

# 24

# Beyond "Culture": Space, Identity, and the Politics of Difference

*Akhil Gupta and James Ferguson*

For a subject whose central rite of passage is fieldwork, whose romance has rested on its exploration of the remote ("the *most* other of others" [Hannerz 1986:363]), whose critical function is seen to lie in its juxtaposition of radically different ways of being (located "elsewhere") with that of the anthropologists' own, usually Western, culture, there has been surprisingly little self-consciousness about the issue of space in anthropological theory. (Some notable exceptions are Appadurai 1986, 1988], Hannerz [1987], and Rosaldo [1988, 1989].) This collection of five ethnographic articles represents a modest attempt to deal with the issues of space and place, along with some necessarily related concerns such as those of location, displacement, community, and identity. In particular, we wish to explore how the renewed interest in theorizing space in postmodernist and feminist theory (Anzaldúa 1987; Baudrillard 1988; Deleuze and Guattari 1987; Foucault 1982; Jameson 1984; Kaplan 1987; Martin and Mohanty 1986) – embodied in such notions as surveillance, panopticism, simulacra, deterritorialization, postmodern hyperspace, borderlands, and marginality – forces us to reevaluate such central analytic concepts in anthropology as that of "culture" and, by extension, the idea of "cultural difference."

Representations of space in the social sciences are remarkably dependent on images of break, rupture, and disjunction. The distinctiveness of societies, nations, and cultures is based upon a seemingly unproblematic division of space, on the fact that they occupy "naturally" discontinuous spaces. The premise of discontinuity forms the starting point from which to theorize contact, conflict, and contradiction between cultures and societies. For example, the representation of the world as a collection of "countries," as in most world maps, sees it as an inherently fragmented space, divided by different colors into diverse national societies, each "rooted" in its proper place (cf. Malkki 1992). It is so taken for granted that each country embodies its own distinctive culture and society that the terms "society" and "culture" are routinely simply appended to the names of nation-states, as when a tourist visits India to understand "Indian culture" and "Indian society," or Thailand to experience "Thai culture," or the United States to get a whiff of "American culture."

Of course, the geographical territories that cultures and societies are believed to map onto do not have to be nations. We do, for example, have ideas about culture-areas that overlap

Akhil Gupta and James Ferguson, "Beyond 'Culture': Space, Identity, and the Politics of Difference," pp. 6–23 from *Cultural Anthropology* 7:1 (Feb., 1992). Copyright © 1992 by the American Anthropological Association.

several nation-states, or of multicultural nations. On a smaller scale, perhaps, are our disciplinary assumptions about the association of culturally unitary groups (tribes or peoples) with "their" territories: thus, "the Nuer" live in "Nuerland" and so forth. The clearest illustration of this kind of thinking are the classic "ethnographic maps" that purported to display the spatial distribution of peoples, tribes, and cultures. But in all these cases, space itself becomes a kind of neutral grid on which cultural difference, historical memory, and societal organization are inscribed. It is in this way that space functions as a central organizing principle in the social sciences at the same time that it disappears from analytical purview.

This assumed isomorphism of space, place, and culture results in some significant problems. First, there is the issue of those who inhabit the border, that "narrow strip along steep edges" (Anzaldúa 1987:3) of national boundaries. The fiction of cultures as discrete, object-like phenomena occupying discrete spaces becomes implausible for those who inhabit the borderlands. Related to border inhabitants are those who live a life of border crossings – migrant workers, nomads, and members of the transnational business and professional elite. What is "the culture" of farm workers who spend half a year in Mexico and half a year in the United States? Finally, there are those who cross borders more or less permanently – immigrants, refugees, exiles, and expatriates. In their case, the disjuncture of place and culture is especially clear: Khmer refugees in the United States take "Khmer culture" with them in the same complicated way that Indian immigrants in England transport "Indian culture" to their new homeland.

A second set of problems raised by the implicit mapping of cultures onto places is to account for cultural differences *within* a locality. "Multiculturalism" is both a feeble acknowledgment of the fact that cultures have lost their moorings in definite places and an attempt to subsume this plurality of cultures within the framework of a national identity. Similarly, the idea of "subcultures" attempts to preserve the idea of distinct "cultures" while acknowledging the relation of different cultures to a dominant culture within the same geographical and territorial space. Conventional accounts of ethnicity, even when used to describe cultural differences in settings where people from different regions live side by side, rely on an unproblematic link between identity and place.[1] Although such concepts are suggestive because they endeavor to stretch the naturalized association of culture with place, they fail to interrogate this assumption in a truly fundamental manner. We need to ask how to deal with cultural difference while abandoning received ideas of (localized) culture.

Third, there is the important question of postcoloniality. To which places do the hybrid cultures of postcoloniality belong? Does the colonial encounter create a "new culture" in both the colonized and colonizing country, or does it destabilize the notion that nations and cultures are isomorphic? As discussed below, postcoloniality further problematizes the relationship between space and culture.

Last, and most important, challenging the ruptured landscape of independent nations and autonomous cultures raises the question of understanding social change and cultural transformation as situated within interconnected spaces. The presumption that spaces are autonomous has enabled the power of topography to conceal successfully the topography of power. The inherently fragmented space assumed in the definition of anthropology as the study of cultures (in the plural) may have been one of the reasons behind the longstanding failure to write anthropology's history as the biography of imperialism. For if one begins with the premise that spaces have *always* been hierarchically interconnected, instead of naturally disconnected, then cultural and social change becomes not a matter of cultural contact and articulation but one of rethinking difference *through* connection.

To illustrate, let us examine one powerful model of cultural change that attempts to relate dialectically the local to larger spatial arenas: articulation. Articulation models, whether they come from Marxist structuralism or from "moral economy," posit a primeval state of autonomy (usually labeled "precapitalist"), which is then violated by global capitalism. The result is that both local and larger spatial arenas are transformed, the local more than the global to be sure, but not necessarily in a predetermined direction. This notion of articulation allows one to explore the richly unintended consequences of, say, colonial capitalism, where loss occurs alongside invention. Yet, by taking a preexisting, localized "community" as a given starting point, it fails to examine sufficiently the processes (such as the structures of feeling that pervade the imagining of community) that go into the construction of space as place or locality in the first instance. In other words, instead of assuming the autonomy of the primeval community, we need to examine how it was formed *as a community* out of the interconnected space that always already existed. Colonialism, then, represents the displacement of one form of interconnection by another. This is not to deny that colonialism, or an expanding capitalism, does indeed have profoundly dislocating effects on existing societies. But by always foregrounding the spatial distribution of hierarchical power relations, we can better understand the process whereby a space achieves a distinctive *identity* as a place. Keeping in mind that notions of locality or community refer both to a demarcated physical space *and* to clusters of interaction, we can see that the identity of a place emerges by the intersection of its specific involvement in a system of hierarchically organized spaces with its cultural construction as a community or locality.

It is for this reason that what Jameson (1984) has dubbed "postmodern hyperspace" has so fundamentally challenged the convenient fiction that mapped cultures onto places and peoples. In the capitalist West, a Fordist regime of accumulation, emphasizing extremely large production facilities, a relatively stable work force, and the welfare state, combined to create urban "communities" whose outlines were most clearly visible in company towns (Davis 1984; Harvey 1989; Mandel 1975). The counterpart of this in the international arena was that multinational corporations, under the leadership of the United States, steadily exploited the raw materials, primary goods, and cheap labor of the independent nation-states of the postcolonial "Third World." Multilateral agencies and powerful Western states preached, and where necessary militarily enforced, the "laws" of the market to encourage the international flow of capital, while national immigration policies ensured that there would be no free (i.e., anarchic, disruptive) flow of labor to the high-wage islands in the capitalist core. Fordist patterns of accumulation have now been replaced by a regime of flexible accumulation – characterized by small-batch production, rapid shifts in product lines, extremely fast movements of capital to exploit the smallest differentials in labor and raw material costs – built on a more sophisticated communications and information network and better means of transporting goods and people. At the same time, the industrial production of culture, entertainment, and leisure that first achieved something approaching global distribution during the Fordist era led, paradoxically, to the invention of new forms of cultural difference and new forms of imagining community. Something like a transnational public sphere has certainly rendered any strictly bounded sense of community or locality obsolete. At the same time, it has enabled the creation of forms of solidarity and identity that do not rest on an appropriation of space where contiguity and face-to-face contact are paramount. In the pulverized space of postmodernity, space has not become irrelevant: it has been *re*territorialized in a way that does not conform to the experience of space that characterized the era of high modernity. It is this that forces us to reconceptualize fundamentally the politics of community, solidarity, identity, and cultural difference.

## Imagined Communities, Imagined Places

People have undoubtedly always been more mobile and identities less fixed than the static and typologizing approaches of classical anthropology would suggest. But today, the rapidly expanding and quickening mobility of people combines with the refusal of cultural products and practices to "stay put" to give a profound sense of a loss of territorial roots, of an erosion of the cultural distinctiveness of places, and of ferment in anthropological theory. The apparent deterritorialization of identity that accompanies such processes has made Clifford's question (1988:275) a key one for recent anthropological inquiry: "What does it mean, at the end of the twentieth century, to speak ... of a 'native land'? What processes rather than essences are involved in present experiences of cultural identity?"

Such questions are of course not wholly new, but issues of collective identity today do seem to take on a special character, when more and more of us live in what Said (1979:18) has called "a generalized condition of homelessness," a world where identities are increasingly coming to be, if not wholly deterritorialized, at least differently territorialized. Refugees, migrants, displaced and stateless peoples – these are perhaps the first to live out these realities in their most complete form, but the problem is more general. In a world of diaspora, transnational culture flows, and mass movements of populations, old-fashioned attempts to map the globe as a set of culture regions or homelands are bewildered by a dazzling array of postcolonial simulacra, doublings and redoublings, as India and Pakistan apparently reappear in postcolonial simulation in London, prerevolution Tehran rises from the ashes in Los Angeles, and a thousand similar cultural dreams are played out in urban and rural settings all across the globe. In this culture-play of diaspora, familiar lines between "here" and "there," center and periphery, colony and metropole become blurred.

Where "here" and "there" become blurred in this way, the cultural certainties and fixities of the metropole are upset as surely, if not in the same way, as those of the colonized periphery. In this sense, it is not only the displaced who experience a displacement (cf. Bhabha 1989:66). For even people remaining in familiar and ancestral places find the nature of their relation to place ineluctably changed, and the illusion of a natural and essential connection between the place and the culture broken. "Englishness," for instance, in contemporary, internationalized England is just as complicated and nearly as deterritorialized a notion as Palestinian-ness or Armenian-ness, since "England" ("the real England") refers less to a bounded place than to an imagined state of being or moral location. Consider, for instance, the following quote from a young white reggae fan in the ethnically chaotic neighborhood of Balsall Heath in Birmingham:

> there's no such thing as "England" any more ... welcome to India brothers! This is the Caribbean! ... Nigeria! ... There is no England, man. This is what is coming. Balsall Heath is the center of the melting pot, 'cos all I ever see when I go out is half-Arab, half-Pakistani, half-Jamaican, half-Scottish, half-Irish. I know 'cos I am [half Scottish/half Irish] ... who am I? ... Tell me who I belong to? They criticize me, the good old England. Alright, where do I belong? You know, I was brought up with blacks, Pakistanis, Africans, Asians, everything, you name it ... who do I belong to? ... I'm just a broad person. The earth is mine ... you know we was not born in Jamaica ... we was not born in "England." We were born here, man. It's our right. That's the way I see it. That's the way I deal with it. [Hebdige 1987:158–9]

The broad-minded acceptance of cosmopolitanism that seems to be implied here is perhaps more the exception than the rule, but there can be little doubt that the explosion of a culturally stable and unitary "England" into the cut-and-mix "here" of contemporary Balsall Heath is an example of a phenomenon that is real and spreading. It is clear that the erosion of such supposedly natural connections between peoples and places has not led to the modernist specter of global cultural homogenization (Clifford 1988). But "cultures" and "peoples," however persistent they may be, cease to be plausibly identifiable as spots on the map.

The irony of these times, however, is that as actual places and localities become ever more blurred and indeterminate, *ideas* of culturally and ethnically distinct places become perhaps even more salient. It is here that it becomes most visible how imagined communities (Anderson 1983) come to be attached to imagined places, as displaced peoples cluster around remembered or imagined homelands, places, or communities in a world that seems increasingly to deny such firm territorialized anchors in their actuality. The set of issues surrounding the construction of place and homeland by mobile and displaced people is addressed in different ways by a number of the articles in this issue.

Remembered places have often served as symbolic anchors of community for dispersed people. This has long been true of immigrants, who (as Leonard [1992] shows vividly) use memory of place to construct imaginatively their new lived world. "Homeland" in this way remains one of the most powerful unifying symbols for mobile and displaced peoples, though the relation to homeland may be very differently constructed in different settings (see Malkki 1992). Moreover, even in more completely deterritorialized times and settings – settings where "home" is not only distant, but also where the very notion of "home" as a durably fixed place is in doubt – aspects of our lives remain highly "localized" in a social sense, as Peters (1992) argues. We need to give up naive ideas of communities as literal entities (cf. Cohen 1985), but remain sensitive to the profound "bifocality" that characterizes locally lived lives in a globally interconnected world, and the powerful role of place in the "near view" of lived experience (Peters 1992).

The partial erosion of spatially bounded social worlds and the growing role of the imagination of places from a distance, however, themselves must be situated within the highly spatialized terms of a global capitalist economy. The special challenge here is to use a focus on the way space is imagined (but not *imaginary*!) as a way to explore the processes through which such conceptual processes of place making meet the changing global economic and political conditions of lived spaces – the relation, we could say, between place and space. As Ferguson (this issue) shows, important tensions may arise when places that have been imagined at a distance must become lived spaces. For places are always imagined in the context of political-economic determinations that have a logic of their own. Territoriality is thus reinscribed at just the point it threatens to be erased.

The idea that space is made meaningful is of course a familiar one to anthropologists; indeed, there is hardly an older or better established anthropological truth. East or West, inside or outside, left or right, mound or floodplain – from at least the time of Durkheim, anthropology has known that the experience of space is always socially constructed. The more urgent task, taken up by several articles in this issue, is to politicize this uncontestable observation. With meaning making understood as a practice, how are spatial meanings established? Who has the power to make places of spaces? Who contests this? What is at stake?

Such questions are particularly important where the meaningful association of places and peoples is concerned. As Malkki (1992) shows, two naturalisms must be challenged here. First is what we will call the ethnological habit of taking the association of a culturally unitary group (the "tribe" or "people") and "its" territory as natural, which is discussed in the previous section. A second, and closely related, naturalism is what we will call the national habit of taking the association of citizens of states and their territories as natural. Here the exemplary image is of the conventional world map of nation-states, through which school-children are taught such deceptively simple-sounding beliefs as that France is where the French live, America is where the Americans live, and so on. Even a casual observer, of course, knows that not only Americans live in America, and it is clear that the very question of what is a "real American" is largely up for grabs. But even anthropologists still talk of "American culture" with no clear understanding of what that means, because we assume a natural association of a culture ("American culture"), a people ("Americans"), and a place ("the United States of America"). Both the ethnological and the national naturalisms present associations of people and place as solid, commonsensical, and agreed-upon, when they are in fact contested, uncertain, and in flux.

Much recent work in anthropology and related fields has focused on the process through which such reified and naturalized national representations are constructed and maintained by states and national elites. (See, for instance, Anderson 1983; Handler 1988; Herzfeld 1987; Hobsbawm and Ranger 1983; Kapferer 1988; Wright 1985.) Borneman (1992) presents a case where state constructions of national territory are complicated by a very particular sort of displacement, as the territorial division and reformation of Germany following the Second World War made unavailable to the two states the claims to a territorially circumscribed home and culturally delineated nation that are usually so central to establish legitimacy. Neither could their citizens rely on such appeals in constructing their own identities. In forging national identities estranged in this way from both territory and culture, Borneman argues, the postwar German states and their citizens employed oppositional strategies, ultimately resulting in versions of the displaced and decentered identities that mark what is often called the postmodern condition.

Discussions of nationalism make it clear that states play a crucial role in the popular politics of place making and in the creation of naturalized links between places and peoples. But it is important to note that state ideologies are far from being the only point at which the imagination of place is politicized. Oppositional images of place have of course been extremely important in anticolonial nationalist movements, as well as in campaigns for self-determination and sovereignty on the part of ethnic counter-nations such as the Hutu (Malkki 1992), the Eritreans, and the Armenians. Bisharat (1992) traces some of the ways in which the imagining of place has played into the Palestinian struggle, showing both how specific constructions of "homeland" have changed in response to political circumstances and how a deeply felt relation to "the land" continues to inform and inspire the Palestinian struggle for self-determination. Bisharat's article serves as a useful reminder, in the light of nationalism's often reactionary connotations in the Western world, of how often notions of home and "own place" have been empowering in anticolonial contexts.

Indeed, future observers of 20th-century revolutions will probably be struck by the difficulty of formulating large-scale political movements *without* reference to national homelands. Gupta (1992) discusses the difficulties raised in attempting to rally people around such a nonnational collectivity as the nonaligned movement; and he points out that similar problems are raised by the proletarian internationalist movement, since, "as generations of Marxists after Marx found out, it is one thing to liberate a nation, quite another to liberate the workers of the world" (Gupta 1992). Class-based internationalism's tendencies to nationalism (as in the history of the Second International, or that of the USSR), and to utopianism imagined in local rather than universal terms (as in Morris's *News from Nowhere* [1970], where "nowhere" [*utopia*] turns out to be a specifically English "somewhere"), show clearly the importance of attaching causes to places and the ubiquity of place making in collective political mobilization.

Such place making, however, need not be national in scale. One example of this is the way idealized notions of "the country" have been used in urban settings to construct critiques of industrial capitalism (cf. in Britain, Williams 1973; for Zambia, Ferguson 1992). Another case is the reworking of ideas of "home" and "community" by feminists like Martin and Mohanty (1986) and Kaplan (1987). Rofel (1992) gives another example in her treatment of the contested meanings of the spaces and local history of a Chinese factory. Her analysis shows both how specific factory locations acquired meanings over time and how these localized spatial meanings confounded the modernizing, panoptic designs of planners – indeed, how the durability of memory and localized meanings of sites and bodies calls into question the very idea of a universal, undifferentiated "modernity."

It must be noted that such popular politics of place can as easily be conservative as progressive. Often enough, as in the contemporary United States, the association of place with memory, loss, and nostalgia plays directly into the hands of reactionary popular movements. This is true not only of explicitly national images long associated with the Right, but also of imagined locales and nostalgic settings such as "small-town America" or "the frontier," which often play into and complement antifeminist idealizations of "the home" and "family."[2]

## Space, Politics, and Anthropological Representation

Changing our conceptions of the relation between space and cultural difference offers a new perspective on recent debates surrounding issues of anthropological representation and writing. The new attention to representational practices has already led to more sophisticated understandings of processes of objectification and the construction of other-ness in anthropological writing. However, with this said, it also seems to us that recent notions of "cultural critique" (Marcus and Fischer 1986) depend on a spatialized understanding of cultural difference that needs to be problematized.

The foundation of cultural critique – a dialogic relation with an "other" culture that yields a critical viewpoint on "our own culture" – assumes an already-existing world of many different, distinct "cultures," and an unproblematic distinction between "our own society" and an "other" society. As Marcus and Fischer put it, the purpose of cultural critique is "to generate critical questions from one society to probe the other" (1986:117); the goal is "to apply both the substantive results and the epistemological lessons learned from ethnography abroad to a renewal of the critical function of anthropology as it is pursued in ethnographic projects at home" (1986:112).

Marcus and Fischer are sensitive to the fact that cultural difference is present "here at home," too, and that "the other" need not be exotic or far away to be other. But the fundamental conception of cultural critique as a relation between "different societies" ends up, perhaps against the authors' intentions, spatializing cultural difference in familiar ways, as ethnography becomes, as above, a link between an unproblematized "home" and "abroad." The anthropological relation is not simply with people who are different, but with "a different society," "a different culture," and thus, inevitably, a relation between "here" and "there." In all of this, the terms of the opposition ("here" and "there," "us" and "them," "our own" and "other" societies) are taken as received: the problem for anthropologists is to use our encounter with "them," "there," to construct a critique of "our own society," "here."

There are a number of problems with this way of conceptualizing the anthropological project. Perhaps the most obvious is the question of the identity of the "we" that keeps coming up in phrases such as "ourselves" and "our own society." Who is this "we"? If the answer is, as we fear, "the West," then we must ask precisely who is to be included and excluded from this club. Nor is the problem solved simply by substituting for "our own society," "the ethnographer's own society." For ethnographers, as for other natives, the postcolonial world is an interconnected social space; for many anthropologists – and perhaps especially for displaced Third World scholars – the identity of "one's own society" is an open question.

A second problem with the way cultural difference has been conceptualized within the "cultural critique" project is that, once excluded from that privileged domain "our own society," "the other" is subtly nativized – placed in a separate frame of analysis and "spatially incarcerated" (Appadurai 1988) in that "other place" that is proper to an "other culture." Cultural critique assumes an original separation, bridged at the initiation of the anthropological fieldworker. The problematic is one of "contact": communication not within a shared social and economic world, but "across cultures" and "between societies."

As an alternative to this way of thinking about cultural difference, we want to problematize the unity of the "us" and the otherness of the "other," and question the radical separation between the two that makes the opposition possible in the first place. We are interested less in establishing a dialogic relation between geographically distinct societies than in exploring the processes of *production* of difference in a world of culturally, socially, and economically interconnected and interdependent spaces.

[...]

What is needed, then, is more than a ready ear and a deft editorial hand to capture and orchestrate the voices of "others"; what is needed is a willingness to interrogate, politically and historically, the apparent "given" of a world in the first place divided into "ourselves" and "others." A first step on this road is to move beyond naturalized conceptions of spatialized "cultures" and to explore instead the production of difference within common, shared, and connected spaces — "the San," for instance, not as "a people," "native" to the desert, but as a historically constituted and depropertied category systematically relegated to the desert.

The move we are calling for, most generally, is away from seeing cultural difference as the correlate of a world of "peoples" whose separate histories wait to be bridged by the anthropologist and toward seeing it as a product of a shared historical process that differentiates the world as it connects it. For the proponents of "cultural critique," difference is taken as starting point, not as end product. Given a world of "different societies," they ask, how can we use experience in one to comment on another? But if we question a pre-given world of separate and discrete "peoples and cultures," and see instead a difference-producing set of relations, we turn from a project of juxtaposing preexisting differences to one of exploring the construction of differences in historical process.

In this perspective, power does not enter the anthropological picture only at the moment of representation, for the cultural distinctiveness that the anthropologist attempts to represent has always already been produced within a field of power relations. There is thus a politics of otherness that is not reducible to a politics of representation. Textual strategies can call attention to the politics of representation, but the issue of otherness itself is not really addressed by the devices of polyphonic textual construction or collaboration with informant-writers, as writers like Clifford and Crapanzano sometimes seem to suggest.

In addition to (not instead of!) textual experimentation, then, there is a need to address the issue of "the West" and its "others" in a way that acknowledges the extra-textual roots of the problem. For example, the area of immigration and immigration law is one practical area where the politics of space and the politics of otherness link up very directly. Indeed, if the separateness of separate places is not a natural given but an anthropological problem, it is remarkable how little anthropologists have had to say about the contemporary political issues connected with immigration in the United States.[3] If we accept a world of originally separate and culturally distinct places, then the question of immigration policy is just a question of how hard we should try to maintain this original order. In this perspective, immigration prohibitions are a relatively minor matter. Indeed, operating with a spatially naturalized understanding of cultural difference, uncontrolled immigration may even appear as a danger to anthropology, threatening to blur or erase the cultural distinctiveness of places that is our stock in trade. If, on the other hand, it is acknowledged that cultural difference is produced and maintained in a field of power relations in a world always already spatially interconnected, then the restriction of immigration becomes visible as one of the main means through which the disempowered are kept that way.

The enforced "difference" of places becomes, in this perspective, part and parcel of a global system of domination. The anthropological task of de-naturalizing cultural and spatial divisions at this point links up with the political task of combating a very literal "spatial incarceration of the native" (Appadurai 1988) within economic spaces zoned, as it were, for poverty. In this sense, changing the way we think about the relations of culture, power, and space opens the possibility of changing more than our texts. There is room, for instance, for a great deal more anthropological involvement, both theoretical and practical, with the politics of the US/Mexico border, with the political and organizing rights of immigrant workers, and with the appropriation of anthropological concepts of "culture" and "difference" into the repressive ideological apparatus of immigration law and the popular perceptions of "foreigners" and "aliens."

A certain unity of place and people has been long assumed in the anthropological concept of culture. But anthropological representations and immigration laws notwithstanding, "the native" is "spatially incarcerated" only in part. The ability of people to confound the established spatial orders, either through physical movement or through their own conceptual and political acts of re-imagination, means that space and place can never be "given," and that the process of their sociopolitical construction must always be considered. An anthropology whose objects are no longer conceived as automatically and naturally anchored in space will need to pay particular attention to the way spaces and places are made, imagined, contested, and enforced. In this sense, it is no paradox to say that questions of space and place are, in this deterritorialized age, more central to anthropological representation than ever.

## Conclusion

In suggesting the requestioning of the spatial assumptions implicit in the most fundamental and seemingly innocuous concepts in the social sciences such as "culture," "society," "community," and "nation," we do not presume to lay out a detailed blueprint for an alternative conceptual apparatus. We do, however, wish to point out some promising directions for the future.

One extremely rich vein has been tapped by those attempting to theorize interstitiality and hybridity: in the postcolonial situation (Bhabha 1989; Hannerz 1987; Rushdie 1989); for people living on cultural and national borders (Anzaldúa 1987; Rosaldo 1987, 1988, 1989); for refugees and displaced peoples (Ghosh 1989; Malkki 1992); and in the case of migrants and workers (Leonard 1992). The "syncretic, adaptive politics and culture" of hybridity, Bhabha points out (1989:64), questions "the imperialist and colonialist notions of purity as much as it question[s] the nationalist notions." It remains to be seen what kind of politics are enabled by such a theorization of hybridity and to what extent it can do away with all claims to authenticity, to all forms of essentialism, strategic or otherwise (see especially Radhakrishnan 1987). Bhabha points to the troublesome connection between claims to purity and utopian teleology in describing how he came to the realization that

> the only place in the world to speak from was at a point whereby contradiction, antagonism, the hybridities of cultural influence, the boundaries of nations, were not sublated into some utopian sense of liberation or return. The place to speak from was through those incommensurable contradictions within which people survive, are politically active, and change. (1989:67)

The borderlands are just such a place of incommensurable contradictions. The term does not indicate a fixed topographical site between two other fixed locales (nations, societies, cultures), but an interstitial zone of displacement and deterritorialization that shapes the identity of the hybridized subject. Rather than dismissing them as insignificant, as marginal zones, thin slivers of land between stable places, we want to contend that the notion of borderlands is a more adequate conceptualization of the "normal" locale of the postmodern subject.

Another promising direction that takes us beyond culture as a spatially localized phenomenon is provided by the analysis of what is variously called "mass media," "public culture," and the "culture industry." (Especially influential here has been the journal, *Public Culture*.) Existing symbiotically with the commodity form, profoundly influencing even the remotest people that anthropologists have made such a fetish of studying, mass media pose the clearest challenge to orthodox notions of culture. National, regional, and village boundaries have, of course, never contained culture in the way that anthropological representations have often implied. However, the existence of a transnational public sphere means that the fiction that such boundaries enclose cultures and regulate cultural exchange can no longer be sustained.

The production and distribution of mass culture – films, television and radio programs, newspapers and wire services, recorded music, books, live concerts – is largely controlled by those notoriously placeless organizations, multinational corporations. The "public sphere" is therefore hardly "public" with respect to control over the representations that are circulated in it. Recent work in cultural studies has emphasized the dangers of reducing the reception of multinational cultural production to the passive act of consumption, leaving no room for the active creation by agents of disjunctures and dislocations between the flow of industrial commodities and cultural products. However, we worry at least as much about the opposite danger of *celebrating* the inventiveness of those "consumers" of the culture industry (especially on the periphery) who fashion something quite different out of products marketed to them, reinterpreting and remaking them, sometimes quite radically, and sometimes in a direction that promotes resistance rather than conformity. The danger here is the temptation to use scattered examples of the cultural flows dribbling from the "periphery" to the chief centers of the culture industry as a way of dismissing the "grand narrative" of capitalism (especially the "totalizing" narrative of late capitalism), and thus of evading the powerful political issues associated with Western global hegemony.

The reconceptualization of space implicit in theories of interstitiality and public culture has led to efforts to conceptualize cultural difference without invoking the orthodox idea of "culture." This is a yet largely unexplored and underdeveloped area. We do, clearly, find the clustering of cultural practices that do not "belong" to a particular "people" or to a definite place. Jameson (1984) has attempted to capture the distinctiveness of these practices in the notion of a "cultural dominant," whereas Ferguson (1990) proposes an idea of "cultural style," which searches for a logic of surface practices without necessarily mapping such practices onto a "total way of life" encompassing values, beliefs, attitudes, et cetera, as in the usual concept of culture. We need to explore what Homi Bhabha calls "the uncanny of cultural difference."

> [C]ultural difference becomes a problem not when you can point to the Hottentot Venus, or to the punk whose hair is six feet up in the air; it does not have that kind of fixable visibility. It is as the strangeness of the familiar that it becomes more problematic, both politically and conceptually . . . when the problem of cultural difference is ourselves-as-others, others-as-ourselves, that borderline. (1989:72)

Why focus on that borderline? We have argued that deterritorialization has destabilized the fixity of "ourselves" and "others." But it has not thereby created subjects who are free-floating monads, despite what is sometimes implied by those eager to celebrate the freedom and playfulness of the postmodern condition. As Martin and Mohanty (1986:194) point out, indeterminacy, too, has its political limits, which follow from the denial of the critic's own location in multiple fields of power. Instead of stopping with the notion of deterritorialization, the pulverization of the space of high modernity, we need to theorize how space is being *re*territorialized in the contemporary world. We need to account sociologically for the fact that the "distance" between the rich in Bombay and the rich in London may be much shorter than that between different classes in "the same" city. Physical location and physical territory, for so long the *only* grid on which cultural difference could be mapped, need to be replaced by multiple grids that enable us to see that connection and contiguity – more generally the representation of territory – vary considerably by factors such as class, gender, race, and sexuality, and are differentially available to those in different locations in the field of power.

# 25

# Afghanistan, Ethnography, and the New World Order

## *David B. Edwards*

Anthropologists do not usually – or at least they are not usually thought to – comment on global issues. The anthropological perspective is generally assumed to be a localized one. We are the resident observers of particular places, usually obscure ones, and until recently at least we have been relatively happy with our obscurity. Strange things happen, however. Events move beyond our control, and fieldwork sites that seem to be as remote and insignificant as any on the planet suddenly take on a global significance that forces the most nocturnal of researchers into the light. When I first decided to be an anthropologist, it was basically because I wanted an excuse to go back to Afghanistan to carry out a traditional sort of village study in some mountain community. I had worked for two years as an English teacher in Kabul in the mid-1970s, had fallen in love with the place, and it seemed that anthropology offered a way to spend more time in parts of the country I would otherwise not be able to visit. So, I started graduate school, and in the meantime a revolution happened that changed my plans.

Afghanistan's obscurity may have been part of what appealed to me in the first place, but for the last ten years I have been trying to come to grips with its notoriety, its confusion, and its disparate energies. Since 1982, I have carried out fieldwork in a variety of places, including the city of Peshawar, Pakistan, and various refugee camps scattered around the Northwest Frontier Province. One summer, I also traveled inside Afghanistan to observe the operations of a group of *mujahidin*, and I have spent quite a bit of time among Afghan refugees in the Washington, DC, area. Finally, and most recently, I have been monitoring the activities of an Afghan computer newsgroup.

All of these experiences have collectively contributed to what I know about Afghans and Afghan culture, and I have found that this knowledge is not easily divisible. Though distant in space, the different contexts are not isolated from one another. What goes on inside Afghanistan affects what is happening in the camps, just as both of these situations influence (and are influenced by) the lives of Afghans in more distant locales. The various attempts I have made over the years to isolate single parts of this larger totality, so that I might be able to produce a more traditional sort of community study, have always been frustrating for me. In a sense that I did not recognize until recently, these attempts represented traumatic amputations of what I know and what I have experienced, and not surprisingly, they resulted in texts that have felt partial, incomplete, and vaguely untruthful. Somehow I have needed to find a mechanism

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that would reflect more closely the whole story as I understood it, a story that is not confined to any one point in time or space.

What follows is the partial product of recent attempts to address the problem of representing the spatially discontinuous and temporally disjointed nature of my fieldwork experience. In pursuing this end, I have been led to employ what might be called a contrapuntal, rather than a traditional, linear structure of exposition, and to move into and out of ethnographic place rather more freely than is usually done. The objective in employing this style of writing is not to develop any particular theoretical point. It is rather to pull together, in one place, experiences that are otherwise separate and distinct in time and space but that seem in some marginally inchoate way to need each other's company.

One effect of assembling these vignettes in one place is to transgress the normal conception of what constitutes "fieldwork." Traditionally framed as a *rite de passage* through which all anthropologists had to pass, the idea of fieldwork carried with it certain definite obligations, for instance that it be conducted over a calendar year and that it take place in an exotic locale in some sort of bounded community like a village or a hunter-gatherer settlement. This conception of fieldwork, as venerable as it might be, is no longer adequate to the reality of shifting boundaries and migrating cultures. People don't stay in one place any more – if they ever did – and the notion that the terms *culture*, *community*, and *place* are more or less synonymous cannot be sustained.

The good news in all this, I think, is that, despite the proliferation of media-driven consumerism crossing every mountain range and border on the planet, the monoculture of our nightmares does not seem to be developing. Cultural differences still abound, and cross-cultural contact appears to be accelerating the process of hybridization as much as it is that of homogenization. One result of this – for anthropologists at least – is that, while the distant cultures we have traditionally studied are no longer so easily isolable or exotic as they were in the past, those that are closer to home are also a great deal more strange and interesting than we ever imagined.

## Paktia

It is a morning in May 1984. I have had my breakfast of bread and tea and am taking a morning walk through the tiny village of Serana, in the territory of the Zadran tribe in south-central Paktia Province, eastern Afghanistan. Paktia Province has long enjoyed the reputation of being the most fiercely independent region of Afghanistan. Time and again, the tribes of Paktia have poured out of the mountains to challenge the government of Kabul's right to rule. That is why I have come here – to see firsthand this *yaghistan* – this place of rebellion – but so far at least I have not met very many native Paktiawal. Most people have left the area, and I have seen only a handful of children and hardly any women at all since arriving.

It is cool in the mountains, cool enough that the few people I pass wear their shawls wrapped tightly around their shoulders. Climbing a short hill on the outskirts of the village, I see a mullah with a billowy white turban seated in the chair of a two-barrel Dashika antiaircraft gun. The Dashika is a Soviet design, but markings on the gun indicate that it is of Chinese manufacture. These guns are shiny and new and only recently arrived by camel caravan across the Pakistani frontier. The mullah is young – late-twenties – and he scans the sky for signs of Soviet MiGs. Only a few weeks before, mujahidin gunners had brought down a MiG-23 not too far from here, and there is fighting going on not too far away; so the mullah is keeping careful watch. As he does so, he listens to a cassette on his Japanese tape recorder of an Egyptian muezzin chanting verses from the Qur'an.

We continue on around the perimeter of the base, climbing as we do toward another antiaircraft gun emplacement located on a ridge overlooking Serana. When we reach the heights, we see another Afghan gunner manning a four-barrel Zigoyak gun, also Soviet-designed and Chinese-made, also recently arrived in the base. This is the largest-caliber gun in their arsenal, and the best gunners have been assigned to maintain and fire it. There are three gunners present, all ex-soldiers from the Afghan army. It is not surprising to find soldiers here. The area is overrun with them, and it seems that the majority of people I have talked with during my trip have been recruits and conscripts from distant parts of the country, who have taken refuge with the mujahidin after deserting from the army.

The soldier who is manning the Zigoyak when we arrive is a burly man. Like everyone else, he is bearded, but he has the additional distinguishing feature of tatooed dots across his face. He tells me that these are associated with his tribe – the Achakzais – in southern Qandahar Province. He is a friendly man, much friendlier than the mullah who had kept his cassette playing as we passed and stared vacantly at me as I took a photo of him. The Qandahari gunner is more affable and offers us tea in the Afghan refugee tent that stands off to the side of the gun emplacement.

He has been away from his home for two years now, ever since he was press-ganged into the army. Six months ago, he escaped, and he has been in Zadran ever since. He doesn't like it here. The Zadrans, he insists, are *vahshî*: wild, savage. You can't trust them. They are Muslims by day and thieves by night. At the same time, however, he also tells me that he intends to stay where he is for awhile. A mountain base like this one is relatively safe. Down in the plains, the government can capture you, and if they don't, you'll probably get killed by one of the parties. Perhaps one day this situation will change, but he will stay where he is for awhile. The roads are too unsafe to travel. The refugee camps in Pakistan are overcrowded, the summers there unbearably hot, and the local officials always want bribes.

Later in the day, I meet another ex-soldier, a Persian-speaking Tajik from the Kohistan region just north of Kabul. He is a young man – not more than 22 or 23 – and boyishly handsome. Although he looks more like a teenager than a man, a man he must be since he served as a parachute commando in the army before deserting last year. Unlike most of the other mujahidin I have met, he has little time for Islam and openly admits to me that he had been a follower of a famous leftist guerrilla leader named Majid Kalakani who had been captured and killed by the government some years before. He talks proudly of his time with Kalakani and tells me of the American sniper rifle that he used to own. It had a scope on it, and once he killed four Soviet tankists as they drove in a convoy down the main road toward Kabul. The beauty of the American rifle, he says, is its small bullets and its silent action. This means that the tankist sitting in the turret could be quietly picked off while the tank was rumbling along the road. Only later did his comrades recognize that the man on top was dead, and by then the sniper was long gone.

It is early afternoon as we talk. The young Kohistani is guarding a Communist prisoner who was captured a few weeks ago during a surprise ambush of a wedding party in the nearby town of Gardez. It is the time of early afternoon prayers, and we are near the mosque. Several mullahs pass by, and each tells my companion in rough Persian to go to the mosque: "*Namaz bokhan* (Go pray)!" To each, he smiles and replies that he is on duty and can't leave his post. He tells me that he is tired of it here. Tired of the mullahs, tired of the tribes. In a month or so, he will leave Zadran and join Khalil, a Jamiat commander, whose base is in Kohistan. Things will be better there. He will be rid of these damn Pakhtuns and return to his own people.

The next morning, I head east with my companions. We are going back to Pakistan, and every hour or so, we pass another group of 10 and 20 mujahidin who are on their way back inside the country. Most are from nearby and have only another day or so to travel. Some are from the far north and will be on the road for the next two or three weeks. All are well armed. Most have AK-47s, and almost every group has at least one rocket-propelled grenade launcher, a weapon particularly useful for ambushes along the road. It is spring, and another season of killing is about to begin.

At the end of the second day, we near the base at Zhawar. It is our last stop before crossing the border. The path takes us along a high ridge that skirts a broad plain. To the north is the garrison town of Khost, and in the distance we can see tall plumes of dust rise up from the ground. Tanks are headed toward us. An operation is going on. The government is trying to retake a post about a mile ahead of us that the mujahidin had captured some weeks earlier. Overhead, a MiG appears. Small at first, it gets larger and larger as it approaches. I can see the bombs under its wings as it banks above us. The antiaircraft guns at Zhawar begin firing. The screaming of the jet is punctuated by the dull, jackhammer thud of the antiaircraft guns. I'm not sure, but I think I can hear an occasional and distant "ting, ting, ting," as the shells ricochet off the armored belly of the jet.

Ahead of us, off to the northeast, we can just make out Laizha, the base that is under attack, and we watch as the MiG levels off and releases its bombs. They twirl off the undercarriage, pirouette for a moment, then brown clouds of dust billow up, and a few seconds later, we hear the dull concussion of metal smashing stone. I can't help myself from thinking: it's just like the movies. Then I get scared, but when I look at my companions they are grinning. One of them shouts to me, "Turn on your tape recorder!" I fumble the machine out of my bag and push the record button. "Isn't it dangerous where we are?" I ask. The older and more experienced of my companions looks at me. "Yes, very!" he says and laughs. Somehow this relaxes me. There is no place to go anyway; so I turn back to watch the MiG and listen again to the antiaircraft guns beating their rhythm against the sky.

## The Net

Last fall, the computer center at my college hooked me up to Internet, one feature of which is a bulletin board consisting of over two thousand news groups. I regularly monitor two of these two thousand groups: "Soc.Cul. Afghanistan" and "Soc.Religion.Islam". The first of these news groups is a bulletin board where Afghans and those interested in things Afghan can post messages to each other, and the second is a bulletin board that attracts messages for and by Muslims generally about issues having to do with their religion. Anyone who wants to send a message out for others to read and comment on writes up what he or she wants to say and then posts it to the bulletin board where it is available for inspection and comment until whoever manages the bulletin board decides to dump the existing postings to make space for new messages.

Those who contribute to Soc.Cul. Afghanistan and Soc.Religion.Islam are scattered rather widely over the globe, but it is difficult to know exactly how many people are out there, doing what I am, which is to say, reading other people's messages. I have to admit I am sometimes uncomfortable in this role, both because it feels rather like voyeurism and because it is strangely similar to anthropology. After all, isn't reading other people's messages what we do for a living? Only, in this case, there doesn't have to be even a pretense of reciprocity. I simply log on and click my mouse enough times to get me into the news group I want to monitor, and then I read. Since I sometimes go to my office early in the morning, I occasionally find myself staring at the screen in the twilight hours before dawn, which increases the sense that I am doing something illicit.

Eventually, I will start posting my own messages, but I haven't yet. Those who regularly post resent people like me. We are "lurkers." In the news groups I monitor, lurking takes on a political complexion, and I have sometimes come across messages warning against "foreign spies" who are out to subvert Islam and Afghanistan. My intentions seem innocent enough, to me at least, but I know that I would be included in that category. Occasionally, those who worry about foreign spies try to encode their messages, generally by transliterating Persian in English script. Some have even worked out complicated ways of combining the available character set symbols into visual approximations of the Persian script, but these systems are cumbersome, and everyone always ends up back in English.

I have only been monitoring these bulletin boards since November 1992; so I am not in a position to make great claims about what goes on these news groups. However, I have noticed certain persistent features that do stand out and are worth noting, such as the oft-expressed concern for "what kind of group we are going to be." One of the most charged issues related to this question has to do with who has the right to put their messages on the bulletin board, and I have encountered frequent complaints by regular contributors when Iranians, Pakistanis, and what are referred to by some as "Islamic fundamentalists" post to the Afghan group.

For many of the so-called fundamentalists, national boundaries are artificial distinctions, and they reflect this belief in their practice of dispatching their computer messages to multiple news groups on "the Net." Many Afghans resent these cross-postings, however, viewing them as violations of their community's boundaries. In the opinion of these Afghans, the practice of cross-posting computer messages replicates the problem that Afghans face in real life, for just as Arabs, Iranians, and Pakistanis intrude upon their news group, so, over the past 15 years, have they repeatedly interfered in Afghanistan's internal affairs.

Beyond this problem of who has the right to participate in the discourse of the news group, there are many other issues that come up on the Net that reflect the contemporary concerns not just of Afghans, but of Muslims more generally. Questions of this sort that I have noticed have included the following:

—Is it permissible to get married over the phone, and if so, how do the mullah and witnesses perform their roles?

—Does contemporary genetic science indicate that marriage to your first cousin increases risks of birth defects?

—What does Islam say about oral sex?

—Are there stipulations in Islam against investing in mutual funds and other current financial instruments?

—Should the beginning and end of the month of fasting be decided according to scientific measures now available, or by local observation, or by the calculations of Saudi officials in Mecca?

Although most of these issues are particular to the better-off immigrants coping with the modern diaspora, there are nevertheless striking similarities between these questions and those that arose in the mud-walled refugee camp where I worked. The refugee camp, like the Net, is an unprecedented place to be working out issues of identity and community, and consequently there are many matters that had previously been unproblematic that have come to take on major significance in the context of the camp. Should one arrange marriages with kinsmen who might be far away or with current neighbors from the camp? How are weddings to be celebrated in a time of jihad? Can people play music on their tape recorders? Who decides guilt and punishment when someone gets injured in a fight or accident? Who will contribute when the mosque roof gets washed out by winter rains? Since most people have to leave their compounds to defecate and there are so many people in camp, how is it going to be possible for women to go about their business without compromising purdah?

The problems that arise on the Net are obviously not the same as these questions, but there are still certain shared features. In both contexts, the usual diacritica of identity have been subjected to pressure and dislocation. In both places, strangers have come together out of common need and have been forced by circumstance to share an unaccustomed *space* in such a way that all can get along. To deal with this situation, new modes of communication and compromise have been developed, and in both contexts Islam has been critical to the effort.

When I lived in Afghanistan before the civil war, it was always my impression that Muslim identity was simply a fact of life and not a subject of contestation. The great majority of men (at least) went to the mosque, prayed, and – for the most part – seemed to accept the obligations imposed on them by religion as givens. Now, however, in contexts in which there are no givens, in which the most basic circumstances of everyday life are subject to flux and uncertainty, Islam has become the primary focus of concern. Although Islam too is subject to novel pressures, it appears to many people more stable than anything else around them. In a sea of uncertain choices, Islam is a life-ring that they can hang on to. But, of course, holding the rope at the other end are the political parties.

The belief of many Afghans that the political parties are manipulating Islam for their own purposes lends a strident political tone to almost every debate that takes place both on the Net and in the camp. However innocently a discussion begins, it always ends up enmeshed in politics. One example that comes to mind involves an American high school student who posted a message on the bulletin board asking for assistance with a homework assignment he was working on. The student seemed to want a thumbnail sketch of Afghan culture and history – presumably one he could copy directly into whatever social studies paper he was writing. However, what he actually got – whether he realized it or not – was much more revealing, for the posters who responded soon forgot about the student's inquiry and launched into a set of diatribes for and against the various Afghan political parties. Since the practice of the Net is for each poster to post his or her message interlinearly within that of the preceding poster, what one saw in following this debate was the gradual erosion of discourse as each new response was laid down on top of the previous one and the text as a whole became progressively more shrill, garbled, and incoherent.

The deterioration of discourse that one encounters graphically on the Net rather accurately approximates the general fracturing of Afghan society – inside and outside Afghanistan's borders – after more than fifteen years of war. At the same time, however, there are differences between the kinds of deterioration one finds inside Afghanistan or in the camps and in the further-flung diaspora of the Net. The bombs that I heard at a distance in Paktia were aimed at real people, and they did not discriminate whether those they struck were mujahidin or children. Likewise, when refugees crammed side by side in their squalid compounds were unable to resolve their disputes and the few avenues of mediation available to them proved unworkable, they usually ended up seeking redress in the old-fashioned way. Only now, the instruments and protocols for seeking redress have changed. Automatic weapons are plentiful, and so too are silent, stealthy paybacks: bombs tossed into compounds, the use of hired gunmen to conceal responsibility, and the proliferation of street-corner kidnappings.

Debates on the Net sometimes aspire, in their rhetorical way, to the level of violence that exists "on the ground," but the fact that these debates have been adapted to the ether space of the computer distinguishes them both in their practice and in their outcome. In the camp, a dispute, whether engendered by a personal feud or an ideological disagreement, will often lead to bitterness and killing, but in ether space words that elsewhere provoke violence are articulated through glyphs and cursors, and no one gets hurt. Violence remains finally in the minds of the beholders, and the escalation of harms and consequences that develops in the course of a normal feud never takes place because of the faceless quality of communication on the Net.

The image that comes to mind when I think about this new technology is the old technology of the panopticon. The new reverses the old, however, for with the new technology one does not have a central controlling observer looking into each of the cells located on the outer ring of the circle. Rather, one finds that the panopticon has been inverted so that all of the inmates are now allowed to look inward toward the central, illuminated space of the Net. In this space, posters dream their dreams, vent their rage, and assume their roles. The antagonisms can be vicious here, but no one really knows who it is they are striking. No one suffers the consequences of their anger, and it is all ultimately rather futile. People have often noted the proliferation of simulated sex in the era of AIDS, but the Net reveals to us that there is also a kind of simulated politics in the age of the global diaspora.

## DC

It is a Sunday in April 1991, and I am at a flea market in Georgetown. I have come to Washington, DC, to visit Shah Mahmood, my old research assistant who is now a political refugee in the States, and to interview a famous mujahidin commander who is staying with him. The day before we had conducted our interview, and today we are trying to sell a shipment of rugs that has been sent to Shah Mahmood by a friend in Pakistan. Every Sunday for the last few months, Shah Mahmood has been taking the rugs to the Sunday morning flea market in Georgetown where he hawks his wares. We are accompanied this day by the commander and another Afghan who shares Shah Mahmood's apartment in Northern Virginia. Unlike Shah Mahmood, this man is an illegal alien who has no papers and few prospects for attaining political asylum.

While we are at the flea market, an Afghan comes up to Shah Mahmood and the commander, and they embrace and chat for awhile. The man is rather chubby and wears an expensive leather coat and a rayon shirt, open at the neck. Next to Shah Mahmood, who is wearing khaki pants and a cotton sport shirt, and the commander, who wears a dark suit and white shirt, the man looks rather flashy, and his carriage is that of someone with a roll of bills in his pocket. The talk is friendly but stiff. After he leaves, Shah Mahmood tells me that this man is from the Surkh Rud district of Afghanistan, not far from where the commander lives. The commander is from the tribal areas in the mountains. This man is from the neighboring plains. His family is very wealthy, and for a long time they have had a variety of business ventures going on in Afghanistan and Pakistan. Now, they have come to the States and have managed to take control of a large percentage of the hot-dog carts operating in the capital district.

One of the secrets of their success, according to Shah Mahmood, is that they are able to hire other Afghans to operate the pushcarts at low salaries. Most of those they hire have recently arrived in the country, and many – like Shah Mahmood's roommate – are illegals. Shah Mahmood himself owned a cart for awhile, but not for long. Those with papers usually look elsewhere for work since the pushcart business doesn't pay well, requires long hours, and is thought of as rather demeaning. The job of choice for most refugees is driving a taxi. That's considered the best, even though it is also difficult work and requires long hours if the driver is going to cover expenses and make any money. But, at least when you're driving a taxi, you're your own man. For Afghans, that's important.

Apparently, the businessman has been trying to see the commander for some time; so his visit to the flea market is not coincidental. The commander is famous, and the businessman wants to show his respect. The businessman may have commercial interests in the States now, but his base of operations is still the Afghan-Pakistan frontier. He knows that the commander is a man of influence back home and may be even more important in the future; so he has reason to want to cultivate their relationship. Before leaving, he has invited us to his home for dinner. We will go there after finishing up with the carpets.

The businessman's apartment is in a highrise complex that Shah Mahmood says is called the "Watergate of the Pentagon." The complex looms over us as we approach, and the fact that we must pass through a security gate lets us know that we are going someplace very different from the dingy, low-rent apartment where Shah Mahmood currently resides. We park and enter one of the towers. A shiny elevator deposits us before tall mirrors on a long, carpeted corridor with threadbare pretensions to elegance. The hallway door opens to reveal a modern apartment unit: wall-to-wall carpeting, matching sofas upholstered in sleek black Naugahyde, a sliding door and balcony looking out over the exurban landscape of Northern Virginia. Straight ahead is the television and VCR in a black Plexiglas console. Around the corner to the right, I can make out a dining room table. In the same direction, I hear kitchen sounds – water running in a sink, dishes rattling – and also the high-pitched sound of small children playing and the hushed voice of a woman who remains unseen.

There are four men present when we arrive. We all greet, and then the men form a single line for prayers, facing the sliding glass door. It is an awkward space for this activity, and bodies inadvertently touch and jostle as the men kneel and rise and kneel again. It is the month of fasting; so, as soon as prayers are finished, all of the men immediately break fast with the sickly sweet juice that is favored for this occasion. Then, we are invited to eat. But not at the dining room table, which remains unused. Rather, we sit on the floor around a plastic sheet that has been placed between the sofas in the living room. It is a cramped space, but the food is good and abundant, and it is gone within minutes.

After the meal, the man we met at the flea market and his younger brother clear the dishes while the elder brother, who is around 45 or 50, sits in an armchair smoking a cigarette. Throughout the evening the younger brothers defer to this man, fetching matches, offering tea and sweets to the guests, and otherwise performing the standard services expected of younger brothers. Most of the conversation goes back and forth between the elder brother and the commander. The commander's opinion on the political situation is solicited, and he tells stories I have heard before about the fighting, the assassination attempts made against him, his wounds. There is a sense of resignation in the room. How many times have such stories been told? How many times have all of these people expressed the same opinions about the leaders and the parties? Their country is sunk in a quagmire, and no one has any idea how it might end. But, they are also not waiting to find out. The Surkh Rudis have their business and their green cards. They don't know it, but the commander has applied for his as well. I have promised to write a letter for him to INS. So have other Americans that the commander has met during his stay. He doesn't want to move here just yet, but everyone is trying to keep as many options open as possible. No one wants to be stuck in Pakistan for the rest of their lives.

After an hour or so, the conversation flags, and our host suggests that we watch a videotape made of his younger brother's wedding that was held in Peshawar the year before. The VCR is turned on, and all turn to watch as a static image of the Surkh Rudi businessman comes on the screen. This time, he is dressed in traditional Pakhtun clothing and wears around his neck a gaudy tinsel garland that droops almost to the floor. He is flanked by a number of other men in similar dress, and every minute or so someone new comes up to him. He rises, and they embrace. Some of the greeters carry garlands of the sort the bridegroom already has around his neck. These garlands are covered in rupee notes, and each greeter places his around the bridegroom's neck. There the garlands remain for a few seconds before a servant comes forward to take the topmost ones away.

The tape, it appears, will go on for hours. Occasionally, the camera pans over the room, but mostly it focuses on the bridegroom rising to greet his guests. There is nothing joyous or celebratory in any of this. None of those who greet the bridegroom look him in the eye or even smile. There are no jokes. No frivolity. It is entirely mechanical, and the camera has exposed that quality more vividly for me than reality ever managed to do. Strangely, it almost seems that there is more intimacy and liveliness in our gathering than at the wedding depicted on the screen, for while the guests on screen are mostly mute and expressionless, our hosts eagerly provide names, background information, and short anecdotes for virtually every guest who greets the bridegroom. This number includes prominent Afghans and Pakistanis. Some are older tribal chiefs dressed in traditional garb; others are sleek young sophisticates. All seem to be men of influence and power, and it is abundantly clear both that this family has a lot of important contacts and that they are extremely proud of this fact.

Most of the names that are mentioned mean little to me, however, and my attention begins to wander until I suddenly notice a lull in the conversation. A new face has appeared on the screen to greet the bridegroom, but for the first time our hosts have remained silent. No one says anything, and there is a palpable feeling of embarrassment that even I can sense despite my unfamiliarity with most of the personalities on the screen and my general obliviousness to the subtleties of the cultural performance I am witnessing. The tape continues, but the enthusiasm has dimmed. After a few minutes, Shah Mahmood begins to fidget and says something about my having to catch a train early in the morning. The intrusion is not unwelcome. The tape is turned off, and everyone stands, chatting and waiting for the appropriate moment to head toward the door.

The man we met at the flea market breaks off from the group and then a few minutes later brings his young son out from one of the bedrooms. He is a proud father, and the sight of the small boy reanimates the conversation. It occurs to me that it is very late for a child this age to be up, and then I realize that while we have been in the apartment, the domestic life of the family has been frozen. At least one woman has been in the kitchen the whole evening. Others (whose voices I have occasionally heard, usually scolding children) have been confined to the back bedrooms. Children have moved about, but the women, obedient to the laws of a distant homeland, have not. Custom and the configuration of the apartment have thus conspired to make their lives quite miserable, at least for this evening, and I'm sure there have been many others not unlike this one.

We embrace and say good-bye to our hosts at the door, but the middle brother, the one we first met that afternoon at the flea market, insists against our objections on escorting us downstairs. Again, there are embraces, and then Shah Mahmood gets behind the wheel of his car, a '76 Chevy Caprice Classic. Shah Mahmood's roommate, the illegal who operates the hot-dog cart, sits beside him, and the commander and I sit in the rear. As we make our way back through the maze of highways and strip malls, we talk about the gathering, and as usual, I go over all of the events and points of discussion that were unclear to me during the course of the evening. One thing that immediately comes to mind is the unnamed man in the videotape, and I ask why it was that everyone had stopped talking when he appeared.

The commander laughs and tells me that the man who had come on the screen was a well-known Communist from Surkh Rud and that he had been in a group of government security officers that the commander had captured in a big operation a few years ago. There had been over seventy men in the group, and the commander had tricked them into an ambush. Almost all of them had been executed, but he had allowed this man to be released because he came from a "good" family. He himself was no good, of course, but his people were "pure"; so he let him go. Sometime later, the man had crossed the border and become a refugee in Pakistan. Now, he's doing business again. There is no outrage, or even disapproval, in the commander's voice when he tells me this, and he goes on to explain that the man's story is in no way unusual. Lots of refugees used to be Communists, he says, including many who now present themselves as pious Muslims. Before, they carried red banners through the streets of Kabul. Now, they wear beards and say their prayers five times a day. That's the kind of war this is. It's just that kind of war.

## Orientations

In this essay, I have brought together incidents whose only apparent connection to one another is that they all involve Afghans as actors and me as witness to their actions. Why these particular moments? Why not others? I cannot say for certain. However, as I was mulling over what I should write about on the subject of "multiply-inflected cultural objects," these episodes struck me as somehow telling, and one reason for this is certainly that they all involve people who are not where they are supposed to be (or at least where they used to be). Whether it is the many mujahidin who have found their way to the mountains of Paktia from diverse regions of Afghanistan, or the graduate students on the Net at their various campuses, or the jihad commander in the high-rise apartment in Virginia, one thread running through each of these narratives is the common condition of coping in foreign places with unfamiliar people.

Another recurrent theme in these vignettes has to do with technology and the way in which technology mediates and transforms social and political relations. In Paktia, technology was represented principally by the multiple instruments of killing that were everywhere to be seen, but there were other sorts of technology present as well, such as the minitape recorder that helped to define my otherwise elusive identity to those I met and the portable radio/tape player that one of the gunners had beside him that allowed him to listen to recitations of the Qur'an, alongside broadcasts from Radio Moscow, the VOA, and the BBC. In the Net, we confront a revolutionary apparatus that makes possible interactive, unmediated communications between perfect strangers from around the globe; but, more dumbfounding than the wizardry of the technology itself is the speed with which the recombinant forms of a postmodern, postnational communicative medium have been harnessed to the ancient wagons of feud and faction. In the story of the jihad commander in Northern Virginia, we encounter the blankly staring presence of the videocassette recorder. Perhaps the most ubiquitous of all new technologies, the VCR has begun to radically reorder the protocols by which identity and community are imagined and managed in an increasingly scattered and globally decentered world.

Each of these themes can be seen in the stories I have told, but I want to reiterate a declaration made at the beginning of this essay, which is that my primary object in bringing these stories together is not to provide graphic illustrations for a particular theory or point of view. The choice of which stories to include here has been arrived at intuitively rather than analytically and has not been guided by any particular proposition. To the contrary, I simply wanted to write up accounts of several remembered incidents from the not-too-distant past, and along the way, I also decided to include a brief excursus into a current fascination – the Net – that seemed somehow to fit with the rest. As I read them now, what the memories I have included and the discussion of the Net all seem to share, to my mind at least, has less to do with definable issues like migration, dislocation, and technology, than with an essential ambiguity and estrangement that I discern in each. That, I suppose, is why they never got included in anything I had written before and why I wanted to put them together here – to make some sense of what was to me their abiding strangeness.

The trip to Paktia, for example, illustrates several obvious themes – the dislocation and realignment of ethnic groups in a time of war, the unanticipated role of the political party as safe haven in response to general upheaval – but it also seems to me to contain a more enigmatic quality, as well, that is less easy to specify. I don't know if that quality has to do with the place itself or if it derives from my perception of it, for my memories of the events described have a timeless, floating quality to them that seems to increase with distance. The experience of war, like that of a natural disaster or of more mundane births and deaths, rests outside of ordinary time. And while these experiences can be hellish, they are also intensely memorable, though in ways that sometimes distort what actually happened. In moments of stress and uncertainty, time itself gets stretched in awkward directions, and when they are recalled as memory, the experienced events are sometimes darkly compressed, and other times bathed in a saturated, unnatural sort of light.

Should these qualities of perception and distortion be suppressed, or should we seek to reflect them in the texts we write? My answer is obviously that they constitute an important part of our experience, even if we do not know at first what they might signify. In this essay, I have tried to convey a sense of the tactile, if sometimes contorted, immediacy of remembered experience by using the first-person and the present tense. The present tense, in particular, is scorned these days as reminiscent of an older style of anthropological writing in which the author bolstered the authenticity of the finished account, along with his/her own authority, by constructing a false facade of realistic, "I-am-the-camera" details. The critique was a valid one, but the present tense remains a useful tool for representing the lived-experience of fieldwork – especially the contingent quality of events as they are apprehended on the run.

Although I understand them better now for having written them down and placed them together, the episodes and situations described here contain other ambiguities, as well, beyond those derived from the deformities of perception and memory. I have no idea, for instance, who my friend the commander really is or which of the things I heard from him are true. Nor can I say with any certainty what sort of negotiations were being transacted that evening in the Surkh Rudis' apartment or how deep was the play. I also can't say for sure what was going on up in the mountains of Paktia. Men from all parts of Afghanistan had been thrown together. That much I know, but what kind of place was it? Was it the forward outpost of a group of committed Muslim warriors – the kind so feared today in the West – or was it more like a small boat seeking a calm anchorage during a very rough night of storms? In the case of the Net, a different sort of uncertainty is present, an uncertainty and a sadness, for it seems to me that obscured beneath the tumult of scrolled postings and the blinking cacophony of interlaced diatribes is a silence that surrounds every individual who logs on. Underlying the diverse communities of interest that the Net makes possible is the specter of solitude – the loneliness of the darkened cell and the humming quiet of the illuminated screen.

We all want simple truths. We want mysteries that give way to our probings. We want situations that yield to analysis. We want tried-and-true concepts to mean what they always have and the assurance that design and method can ultimately win out over randomness and entropy. And maybe more than anything, we want stories that have clear morals, heroes, and villains who are what they appear to be, and endings that finally end. If recent events tell us anything, however, it is that such illusions are vain. The announcement of a new world order and of history's death was premature, and we have come to understand that whatever transient order had existed in world affairs was largely the result of the unnatural constraints that had been imposed upon them by superpower rivalry.

Ethnographers and ethnographic writing cannot change these facts, but they should at least strive to reflect them and resist the urge to impose an overarching order when such order is not what is most apparent. One implication of this assertion is that, just as anthropologists provide a space in their texts for analysis and interpretation, so should they leave room for strangeness and uncertainty and the stuff that troubles understanding. As they enlist theories and explanatory models to specific ends, so should they make it clear that their scope is finite and that at least some of the symmetries they perceive are produced by the theoretical lenses they choose to wear. In coming to grips with the interconnected worlds we inhabit and chronicle, we must also think very carefully about the methodological practices we keep and the rhetorical ways in which we conceive and fashion the "data" of our research. Despite our best efforts to be truthful, traditional modes of organization and articulation can and often do lend to what we construe as "our research objects" a formal logic and coherence that bears little or no relationship to our original experience of those objects.

Recognizing this fact, I have tried to explore in this paper new ways of organizing and articulating my accounts of the people and places I study. What I have come up with is a more improvisational or aleatoric style of writing, the goal of which has been to convey a sense of the scattered and disjointed contexts in which I have conducted field research. In the process of putting together this text, however, I have also been faced with the fact that, however far-flung they might be, the social worlds I study nevertheless reveal between themselves certain patterns and processes despite the disjunctions and dislocations to which they are subject. While I have tried to see my fieldwork as experience rather than data collection and to avoid imposing an extraneous order upon the events I have witnessed, a certain kind of order has emerged nonetheless. Is it an order that resides out there, or has my anthropological training so conditioned me to see the world in certain ways that my own perception has become an extension of the discipline's priorities? I can't answer this question with any confidence, but I hope that I can at least contribute to the realization that such questions matter and to the experimentation that will be required for anthropologists to capture the sense and significance of the multiply-inflected, multiply-conflicted global cultures they presently confront.

## Peshawar

Since I have stated a commitment to experience over theory and to ambiguity over certainty, I will conclude by recounting a final episode that (like so many stories coming out of a war zone) has the shadow of violence upon it. The incident took place one afternoon, not long before I left Peshawar, when an Afghan friend drove up to my house in a Suzuki pickup. He worked for one of the relief organizations, and he'd told me before that he'd be coming by. He had a passenger with him, a man of about 45, I would have guessed, but his grizzled beard made it difficult to guess his age with any precision. My friend had to help him get out because his eyes could no longer see. We sat down inside, and he told me his story, what there was to tell.

He'd been a village *mulla* before the war. For the last six years, he had been leading the life of a *mujahed*, living in the mountains, laying ambushes, leading prayers, traveling back and forth to Pakistan for supplies. One day, he was walking along a path and spied a book lying on the ground nearby. From the cover, he could tell it was a copy of the Qur'an. Normally, Afghans cover their Qur'ans with cloth and keep them in special niches in the walls of their homes. This one was soiled and abandoned. He opened it, and a bomb concealed within exploded in his face. That's why he'd been brought to me. My friend knew that I had a Pakistani friend who was a doctor. I'd called him, and my doctor friend had recommended a specialist with whom I made an appointment.

The man we went to see was about my age at the time. Early thirties. He'd gotten his medical education at Khyber Medical College in Peshawar and then went on to receive advanced training in eye surgery in London. The examination was brief. He shined a light into the mulla's eyes, peered through an instrument into each of the two scarred orbs, and then signaled me to come with him back to his office. There was nothing that could be done. Both eyes were destroyed. He shook his head. I can't remember exactly what he said, but it was something about the savagery of tribal people. It is difficult for us to understand them or their violence, he told me. They're just different from you and me, and by way of elaboration, he pulled out a handful of photographs. They showed a woman in traditional tribal dress. She looked like other Pakhtun women I had seen, except that her face was horribly disfigured.

The doctor explained that her husband had slashed her repeatedly with a knife for some perceived indiscretion. She probably should have died, her wounds were so severe, but some relatives had managed to get her out of the house and had taken her to the hospital. That's where the doctor had seen her. Because she had been cut around her eyes, as well as on other parts of her face, he had examined her, and later, when she was recovering and the scars were healing, he had taken these snapshots. As I looked at the photographs, he looked at me and smiled a dry, wry sort of smile. It was not a smile of amusement, but of identification. From the doctor's surname, I knew that he was himself a Pakhtun. His people had once been tribal people, and even now, he might have women in his own extended family who looked and dressed very much like the woman in the pictures. But now he was an educated man like myself, and these photographs seemed in some sad, horrible way to join us together, in testament to our common bond, our common civilization, and our common difference.

# 26

# Being There . . . and There . . . and There! Reflections on Multi-Site Ethnography

*Ulf Hannerz*

In 1950, Professor Edward Evans-Pritchard, not yet 'Sir' but certainly a central figure in mid-century anthropology, gave a radio lecture on the BBC Third Programme where he outlined what an Oxford man (no doubt here about gender) would properly do to become an accomplished fieldworker in social anthropology. Having prepared himself meticulously for a couple of years, and if fortunate enough to get a research grant, the anthropologist-to-be would proceed to his chosen primitive society to spend there usually two years, preferably divided into two expeditions with a few months in between, if possible in a university department where he could think about his materials. In the field, Evans-Pritchard's anthropologist would throughout be in close contact with the people among whom he was working, he must communicate with them solely through their own language, and he must study their 'entire culture and social life'. For one thing, the long period in the field would allow observations to be made at every season of the year. Having returned home, it would take the anthropologist at least another five years to publish the results of his research, so the study of a single society could be reckoned to require 10 years. And then, Evans-Pritchard concluded, a study of a second society was desirable – lest the anthropologist would think for the rest of his life in terms of a particular type of society (Evans-Pritchard, 1951: 64ff).

The idea of such a thorough, formative, exclusive engagement with a single field is of course at the base of the enduring power in anthropology of the prospect, or experience, or memory, or simply collectively both celebrated and mystified notion, of 'being there'.[1]

Something much like Evans-Pritchard's prescription has very long remained more or less the only fully publicly acknowledged model for fieldwork, and for becoming and being a real anthropologist. Perhaps, it works with full force especially in the continued instruction of newcomers in the discipline – in many ways I conformed to it myself in my first field study, in an African American neighborhood in Washington, DC, although that was something quite different from Evans-Pritchard's classic 'primitive society'. Yet the hegemony of the model seems remarkable since it is fairly clear that a great many anthropologists, especially those no longer in the first phase of their careers, have long, but perhaps a bit more discreetly, been engaging in a greater variety of

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spatial and temporal practices as they have gone about their research. It may have been only Gupta and Ferguson's *Anthropological Locations* (1997) that really brought this variety entirely into the open. (I realize, certainly, that the power of the model has not been as strong among the ethnographically inclined in other disciplines, not so fully exposed to it, and obviously working under other conditions.)

So it may be, then, that when the conception of multi-site fieldwork – being there... and there... and there! – propagated most consistently by George Marcus (e.g. 1986, 1995), first gained wider recognition in anthropology in the later years of the 20th century, it was not really so entirely innovative. For one thing, in studies of migration, it was already becoming an established ideal to 'be there' at both points of departure and points of arrival (see e.g. Watson, 1977), thus working at least bilocally. Nor should we disregard the fact that the real pioneer of intensive anthropological fieldwork, Malinowski, was already going multilocal when he followed the Trobrianders along the Kula ring. Yet the very fact that this style of doing ethnography was given a label, and prominently advocated, and exemplified (if in large part by borrowing a case from journalism), and that this occurred much at the same time as ideas of place and the local were coming under increasing scrutiny in and out of anthropology, no doubt helped accelerate its recent spread, as a practice or as a topic of argument.

Whether due to convergent interests or mutual inspiration, a number of my colleagues in Stockholm and I were among those who fairly quickly saw possibilities in configuring our projects along multilocal lines. One of us studied the organizational culture of Apple Computer in Silicon Valley, at the European headquarters in Paris, and at the Stockholm regional office; another studied the occupational world of ballet dancers in New York, London, Frankfurt and Stockholm; a third connected to the Armenian diaspora across several continents; a fourth explored the emergent profession of interculturalists, what I have elsewhere a little facetiously referred to as the 'culture shock prevention industry'; and so on. We debated the characteristics of multilocal field studies fairly intensely among ourselves and with other colleagues, and a book some 10 of us put together on our projects and experiences, particularly for teaching purposes, may have been the first more extended treatment of the topic (Hannerz, 2001a). As far as I am concerned myself, perhaps lagging a little behind my more quickly-moving colleagues and graduate students, my involvement with multi-site work has been primarily through a study of the work of news media foreign correspondents which I will draw on here.[2]

## Among the Foreign Correspondents

The general background was that some 20–25 years ago I rather serendipitously drifted into the area which later came to be known as 'globalization' through a local study of a West African town, and then spent some time in large part thinking about the anthropology of the global ecumene in more conceptual and programmatic terms. By the time my itch to return to fieldwork combined with an actual opportunity to do so, several of us in Stockholm were concerned with 'globalization at work' – that is, responding to the fact that a large proportion of existing or emergent transnational connections are set up in occupational life. (This meant that we could also find food for thought in occupational ethnography outside anthropology, not least in the Chicago sociological tradition of Everett Hughes, Howard Becker and others.) More specifically, my own project could draw on the fact that I am a life-time news addict, and assumed as I began to think about it that if globalization was also a matter of becoming more aware of the world, and having more elaborated understandings of the world, 'foreign news' would be a central source of such understandings.[3] Perhaps most concretely, my curiosity fastened on some of the reporting I was habitually exposed to, for example when listening to the morning news program on the radio while having breakfast, and trying to wake up. There – this would have been in the mid-1990s – a familiar voice would report on street riots in Karachi, or the latest triumph of the expanding Taliban . . . and then sign off from Hong Kong. There are people, then, such as 'Asia correspondents', or 'Africa correspondents'. These are also people, clearly, engaged in an occupational practice of 'being there . . . and there . . . and there' – and sometimes possibly even appearing to be where they are not, if for example they can make a Karachi street scene come alive in their reporting even when they quite clearly are at a desk thousands of miles away from it. But just how do they do it?

I should say that as I was becoming seriously attracted to the idea of doing something like an ethnography of the social world of foreign correspondents, I was still a bit ambivalent. I found that on my shelves I already had some number of the kind of autobiographies some correspondents do, usually probably as their careers begin approaching an end; and I had seen most of those movies which over the years have turned the foreign correspondent into a kind of popular culture hero. As the saying goes, 'anthropologists value studying what they like and liking what they study' (Nader, 1972: 303) – and I wondered whether I would find foreign correspondents unapproachable, or perhaps arrogant prima donnas, or just possibly too suspicious of an academic who they might fear would always be inclined to carping criticisms of their work.

As it turned out, I need not really have worried. I did a series of pilot interviews in New York during a period when I found myself there as the field spouse of another multi-site ethnographer, and the journalists I talked to there, having made first contacts through anthropologist mutual acquaintances, were very hospitable and encouraging. (The only thing I found a bit funny was that so many of them were Pulitzer Prize winners.) And that is how it continued to be. In the following years I engaged in a series of conversations with foreign correspondents and, sometimes, strictly speaking, excorrespondents, mostly in Jerusalem, Johannesburg and Tokyo, but also in some number of other places including New York and Los Angeles, where I seized on the opportunity which some other kind of trip provided, to add another handful of interviews. Altogether, I talked to some 70 correspondents, and a few foreign news editors offering the perspective from headquarters.

As I see it, an ethnography of foreign news work of my kind can attempt to fill a noteworthy gap between two sets of representations of international news. At least since the 1970s, when a critical awareness grew of the communication imbalances in the world, it has been recurrently noted that the apparatus of global news flow is in large part controlled by what we have described as either 'the West' or 'the North' – the obvious examples of such dominance have been major news agencies such as Reuters or the Associated Press, with CNN more recently added as another key symbol of the apparatus. The other set of representations I have in mind consists of those memoirs by the newspeople themselves which I just referred to. These tend to be quite individual-centered, focusing on the authors as men and women of action, facing all kinds of dangers as they struggle to file their reports from the trouble spots of the world.

The gap, then, is one between foreign correspondents represented as puppets and as heroes. In the heavily macro-oriented views of media imperialism, the individuals who would be its flesh-and-blood representatives at the outer reaches of the newshandling apparatus are hardly seen as anything other than anonymous, exchangeable tools. In the autobiographical genre, in contrast, the individuals tend to the strong, the wider structure of news reporting not so noticeable.

Certainly my study of the foreign correspondents reflects the asymmetry in the global landscape of news. I deal mostly with Europeans and Americans, reporting from parts of the world which do not send out a comparable number of correspondents of their own to report from other places. In large part, this obviously matches the classic asymmetry of anthropology; and my choice of Jerusalem, Johannesburg and Tokyo as main field sites also reflects an interest in the way foreign correspondents, on a parallel track to ours, deal with issues of 'translating culture', of 'representing the other'. Apart from that, however, we face here once more the problem of striking a balance between structure and agency. What I have attempted to do in my study is to portray the networks of relationships more immediately surrounding the foreign correspondents, locally or translocally; the patterns of collaboration, competition and division of labor which organize their daily activities, formally or informally; and not least their room for maneuver and personal preferences in reporting. I have been curious about the partnerships which evolve between correspondents who prefer each other as company when going on reporting trips, and about the relationships between correspondents and local 'fixers', reminding me of the multifaceted links between anthropologists and their field assistants.

I have explored, too, the often obscure passages of news in roundabout ways between news agencies, electronic media and print media, which sometimes offer convenient shortcuts in correspondent work but which also generate tensions and now and then backstage satirical comment about recycling and plagiarism. And not least have I been concerned with the implications of career patterns and with the spatial organization of foreign correspondence. How might it matter to reporting that some correspondents spend most of a life time in a single posting, while others are rotated every three years or so, between countries and continents? When large parts of the world get only brief visits by correspondents, described on such occasions as 'parachutists' or 'firemen', and only when there is a crisis to cover, how does this shape their and our view of these lands?

I am not going to devote my space here to any great extent, however, to discuss the specifics of my own project. I will rather try, against the background of this experience and that of some of my colleagues, to spell out a few of the issues which characteristically arise in multi-site ethnography, and ways in which it is likely to differ from the established model of anthropological field study, as I have let the latter be represented above by Evans-Pritchard and his half-century old formulation. For I believe that in arguments over the worth of multilocal work, it is not always made entirely transparent how it relates to the assumptions based on classic understandings of 'being there'.

## Constituting the Multi-Site Field

In a way, one might argue, the term 'multilocal' is a little misleading, for what current multilocal projects have in common is that they draw on some problem, some formulation of a topic, which is significantly *trans*local, not to be confined within some single place. The sites are connected with one another in such ways that the relationships between them are as important for this formulation as the relationships within them; the fields are not some mere collection of local units. One must establish the translocal linkages, and the interconnections between those and whatever local bundles of relationships which are also part of the study.[4] In my foreign correspondent study, a major such linkage was obviously between the correspondents abroad and the editors at home. But then there was also the fact that the correspondents looked sideways, toward other news sites and postings, and sometimes moved on to these. They often knew colleagues in some number of other such sites, having been stationed in the same place some time earlier, or by meeting somewhere on one or more of those 'fireman' excursions which are a celebrated part of the public imagery of foreign correspondence, or by working for the same organization. In some loose sense, there is a world-wide 'community' of foreign correspondents, connected through local and long-distance ties.

These linkages make the multi-site study something different from a mere comparative study of localities (which in one classical mode of anthropological comparison was based precisely on the assumption that such linkages did not exist). Yet certainly comparisons are often built into multi-site research. My colleague Christina Garsten (1994), in her study of three sites within the transnational organization of Apple, was interested in comparing center and periphery within the corporation, as well as the way company culture in the offices was influenced by national cultures. As Helena Wulff (1998) studied the transnational ballet world she was similarly interested in national dance styles, but also in the differences between those companies in large part supported by the state and those working more entirely in the market. In my own study I could note the differences in foreign correspondent work between Jerusalem, where close at hand there was an almost constant stream of events commanding world attention; Tokyo, where it was a certain problem for correspondents that much of the time nothing really newsworthy seemed to happen; and Johannesburg, where designated 'Africa correspondents' based there would mostly travel to other parts of the continent when there was a war or a disaster to report on.

If we could make use of the possibilities for comparison, however, neither I nor my colleagues could claim to have an ethnographic grasp of the entire 'fields' which our chosen research topics may have seemed to suggest – and this tends to be in the nature of multi-site ethnography. It may be that in a migration study where all the migrants leave the same village and then turn up in the same proletarian neighborhood in a distant city, the potential and the actual combinations of sites are the same. On the other hand, a multinational corporation has many branches, ballet companies exist in a great many cities, a diaspora like that of the Armenians is widely dispersed, and foreign correspondents are based in major clusters in some 20–25 places around the world (disregarding here those temporary concentrations which result when the 'firemen' descend on a remote and otherwise mostly neglected locus of hard news). Consequently, multi-site ethnography almost always entails a selection of sites from among those many which could potentially be included. Evans-Pritchard may not actually have been everywhere in Azandeland or Nuer country, but this would hardly be as immediately obvious as the selectiveness, or incompleteness, of the multi-site study, where potential sites are clearly separate from one another.

The actual combination of sites included in a study may certainly have much to do with a research design which focuses on particular problems, or which seeks out particular opportunities for comparison. When I chose the somewhat exotic sites of Jerusalem, Johannesburg and Tokyo, it was because I was interested in reporting over cultural distances – I would have been less attracted by reporting between, say, Brussels and Stockholm, or between London and New York. Yet I wonder if it is not a recurrent characteristic of multi-site ethnography that site selections are to an extent made gradually and cumulatively, as new insights develop, as opportunities come into sight, and to some extent by chance. I had originally had in mind including India in my study, but then the first time I was planning to go a national election was called there, and while that could have been an attractive field experience, I suspected it would be a time when correspondents would have little time for me. Then the second time an ailment of my own made the streets of Delhi seem a less appealing prospect. To begin with, I had not expected to include Tokyo in my study, although it turned out to be a very good choice. But in no small part I went there because I had an invitation to a research workshop in Japan at a time when I could also stay on for some research.

## Questions of Breadth and Relationships

Evans-Pritchard's anthropologist, again, would study the 'entire culture and social life' of the people assigned to him. Being around for at least a year, he could make observations during all seasons, and he would work in the local language (although it would probably be true that it was a language which in large part he had to learn during that year). And then, having spent, everything included, a decade of his life on that study, one could hope that there would also be time left for getting to know another people.

This is the kind of image of 'real' fieldwork which tends to worry current practitioners of, and commentators on, multi-site studies in anthropology. Compared to such standards, are these studies inevitably of dubious quality? If you are involved with two, three or even more places in much the same time span that classical anthropology would allow for one, which for various practical reasons may now be the case, what can you actually do? I do not want to assert that no problems of depth and breadth arise, that no dilemmas are inevitably there to be faced. Yet it is important that we realize how one site in a multi-site study now differs from the single site of that mid-20th century anthropologist.

I was in Jerusalem and Johannesburg and Tokyo, and more marginally in several other places, but I was clearly not trying to study the 'entire culture and social life' of these three cities. I was merely trying to get to know some number of the foreign newspeople stationed in them, and the local ecology of their activities. In fact, I was not trying hard to get to know these individuals particularly intimately either; what mattered to me about their childhood or family lives or personal interests was how these might affect their foreign correspondent work.

Anthropologists often take a rather romantic view of their fields and their relationships to people there. They find it difficult to describe their informants as informants because they would rather see them as friends, and they may be proud to announce that they have been adopted into families and kin groups – not only because it suggests something about their skills as fieldworkers, but also because it carries a moral value. They have surrendered to the field, and have been in a way absorbed by it. (Evans-Pritchard [1951: 79] shared similar sentiments: 'An anthropologist has failed unless, when he says goodbye to the natives, there is on both sides the sorrow of parting'.) Perhaps it is for similar reasons that I much prefer describing my encounters with correspondents as conversations, suggesting a more personal quality, rather than as interviews, although I certainly also want to convey the idea of only rather mildly structured exchanges, with room for spontaneous flow and unexpected turns.

There is no doubt a time factor involved in how relationships evolve. Yet I believe most multi-site studies really also have built-in assumptions about segmented lives, where some aspect (work, ethnicity or something else) is most central to the line of inquiry, and other aspects are less so. The ethnographer may be interested in the embeddedness of a particular line of belief or activity in a wider set of circumstances, but this hardly amounts to some holistic ambition. It is a pleasure if one discovers a kindred soul, but one keeps hardnosedly in mind what more precisely one is after, and what sorts of relationships are characteristic of the field itself, as one delineates it.

To some extent personalizing encounters in the modern, multi-site field comes not so much from deepening particular interactions as from the identification of common acquaintances – form placing the ethnographer in the translocal network of relationships. Meeting with foreign correspondents, I have sensed that it is often appreciated when it turns out that I have also talked to friends and colleagues of theirs in some other part of the world; perhaps more recently than they have. Or even to their editor at home. As I have tried to include informants from the same news organization in different postings, to develop my understanding of its operations and as a kind of triangulation, such connections can be discovered fairly often and easily. It is a matter of establishing personal credentials.

## Site Temporalities

Anthropology's classic image of fieldwork also includes an assumption about the durability of fields, and the involvement of 'natives' in them, relative to the length of the ethnographer's field stay. At least implicitly there is the notion that the ethnographer, alone a transient, has to develop in that year or two the understandings which match what the locals assemble during a life time. That year, moreover, covers the most predictable variation that one finds in local life: that of seasons.

Obviously the people we are concerned with in present day field studies tend mostly to be less dependent on seasons and their cycles of activity – on planting and harvesting, or on moving herds to greener pastures. But in addition, these people themselves often have other kinds of relationships to the site than that of real 'natives'. In Evans-Pritchard's time, the Azande and the Nuer among whom he mostly worked were pedestrians – in a life-time they did not go all that far away. There may be some such people in Jerusalem, Johannesburg and Tokyo as well, but hardly among the foreign correspondents. And generally the people on whom we focus in multi-site field studies tend to be the more mobile ones, those who contribute most to turning the combinations of sites into coherent fields, and who also make the sites themselves, at least for the purposes of the studies, more like 'translocalities' (Appadurai, 1996). Some of the sites may even in themselves be short-lived phenomena. My Stockholm colleague Tommy Dahlén (1997), studying the making of the new interculturalist profession, found international conferences, including ritual events, workshops, exhibits and parties, central to his ethnography. And by the time his study was over, he had surely attended more of these conferences than most interculturalists. Such temporary sites – conferences, courses, festivals – are obviously important in much contemporary ethnography.

In some sites now, this goes to say, there are no real natives, or at any rate fewer of them, sharing a life-time's localized experience and collectivized understandings. There are more people who are, like the anthropologist, more like strangers. I find thought-provoking James Ferguson's (1999: 208) comment on what ethnography on the urban Zambian Copperbelt was like toward the end of the 20th century:

> Here there is much to be understood, but none of the participants in the scene can claim to understand it all or even take it all in. Everyone is a little confused (some more than others, to be sure), and everyone finds some things that seem clear and others that are unintelligible or only partially intelligible . . . Anthropological understanding must take on a different character when to understand things like the natives is to miss most of what is going on.

This can be as true in single-site as in multi-site studies, but it problematizes the relationship between 'native' and ethnographer knowledge. Do things become easier for fieldworkers if their informants also find the world opaque, or more difficult as they have to understand not only the structure of knowledge such as it is, but also the nature and social organization of ignorance and misunderstandings? In any case, we sense that we have moved away from the classic fieldwork model.

## Materials: Interviews, Observations, Etc.

Again, in my foreign correspondent project, interviews, be they long, informal and loosely ordered, were a large part of my field materials. I did sit in on a daily staff meeting of the foreign desk at one newspaper, and went on a reporting trip to the Palestinian West Bank with one correspondent. More materials of these and other kinds would no doubt have been of value, but for practical reasons I did not pursue some such possibilities, using the time at my disposal rather to ensure diversity through the interviews. (I tried to include different kinds of media, although with an emphasis on print correspondents, and I wanted to include a reasonably broad range of nationalities.) Also, as in Jerusalem, Johannesburg and Tokyo, and to a more limited extent in a couple of other places, I met with correspondents as they were immersed in the activities of a particular beat, and the interviews could be detailed and concrete.

Probably the time factor has a part in making many multi-site studies rather more dependent on interviews than single-site studies. If the researchers have to handle more places in the time classic fieldwork would devote to one, they may be more in a hurry. Language skills also probably play a part. In interviews, it is more likely that you can manage in one or two languages. My conversations with foreign correspondents were in English, except for those with fellow Scandinavians. In those sites, for many of the correspondents – particularly those who were expatriates, rotating between assignments – English was their working language as well. George Marcus (1995: 101) concludes that most multi-sited field studies so far have been carried out in monolingual, mostly English-speaking settings.

This is surely not to say that multi-site ethnography must rely entirely on interviewing and informant work (in which case some might even feel that in the field phase, it is less than fully ethnographic – the ethnographic tendency may become more obvious in the style of writing); this still depends on the nature of research topics. Studying ballet companies, Helena Wulff could view performances and sit in on endless rehearsals. Although she could not very well 'participate' in the public performances, her own dance background meant that she still had a particular empathetic insight into the more practical, bodily aspects of dancing lives.

But then if pure observation, or participant observation, has a more limited part in some multi-site studies than in the classic model of anthropological fieldwork, it may not have so much to do with sheer multi-sitedness as with the fact that they tend to involve settings of modernity. There are surely a great many activities where it is worthwhile to be immediately present, even actively engaged, but also others which may be monotonous, isolated, and difficult to access. What do you do when 'your people' spend hours alone at a desk, perhaps concentrating on a computer screen?

At the same time, whatever you may now do along more classic ethnographic lines can be, often must be, combined with other kinds of sources and materials. Hugh Gusterson (1997: 116), moving on personally from an ethnography of one California nuclear weapons laboratory to a study of the entire American 'nuclear weapons community', and looking intermittently at the counterpart Russian community as well, describes contemporary ethnography as a matter of 'polymorphous engagements' – interacting with informants across a number of dispersed sites, but also doing fieldwork by telephone and email, collecting data eclectically in many different ways from a disparate array of sources, attending carefully to popular culture, and reading newspapers and official documents. Skills of synthesis may become more important than ever. Certainly it is in considerable part relationships which are not, or at least not always, of a face-to-face nature which make the multi-site field cohere. Media, personal or impersonal, seem to leave their mark on most multi-site studies. Ulf Björklund (2001: 100), my colleague engaged in studying the Armenian diaspora, quotes an editor explaining that 'wherever in the world there are two dozen Armenians, they publish some kind of paper'. Helena Wulff describes the varied ways in which dance videos are used in the transnational dance community, including instruction as well as marketing. In my foreign correspondent study, the correspondents' reporting itself naturally makes up a large part of my materials, interweaving with my interviews. In the end, too, this means that Evans-Pritchard's words about the 'sorrow of parting' seem just a little less to the point. Just as their reporting could allow me to know at least something about them before meeting them in the flesh, so I could also to a degree keep track of them thereafter by following their reporting, from the sites where I met them or from elsewhere in the world, as I was back in Stockholm.

## An Art of the Possible: Fitting Fieldwork into Lives

The pilot interviews apart, I began field studies for my foreign correspondent project in late 1996, and did the last interview in early 2000. In a way, then, I could seem to come close to Evans-Pritchard's five-year norm for a project, but that did not really include my preparatory work, nor time for writing up. On the other hand, I was not at all working full time on the project. In between, I was back in Stockholm engaged in teaching and administration, and also had a couple of brief but gratifying research fellowships elsewhere. But all the time, of course, I was following the reporting of foreign news.

Whether it is single-site or multi-site, I am convinced that much ethnographic work is now organized rather like that. Professional or domestic obligations make the possibility of simply taking off for a field for a continuous stretch of another year or two appear rather remote. For some that means never going to the field again, so there is no 'second society' experience of the kind which would supposedly broaden your intellectual horizons. But then ethnography is an art of the possible, and it may be better to have some of it than none at all. And so we do it now and then, fitting it into our lives when we have a chance.

Often, no doubt, this will be a matter of being there – and again! and again! – returning to a known although probably changing scene. Multi-site ethnography, however, may fit particularly well into that more drawn-out, off-and-on kind of scheduling, as the latter does not only allow us to think during times in between about the materials we have, but also about where to go next. It could just be rather impractical to move hurriedly directly from one field site to the next, according to a plan allowing for little alteration along the way.

Concluding one of his contributions to a recent British volume on anthropological field-work – Oxford-based, and thus also in a way updating the classic Evans-Pritchard model – detailing his own enduring East African commitment, David Parkin (2000: 107) notes that practical circumstances such as the growing number of anthropologists, and governmental financial restrictions on purely academic research, are factors which probably matter more to changes in styles of doing research than does intellectual debate; and he suggests that if more ethnographers now actually spread their fieldwork over many shorter periods than do it in the classic way of larger blocks of time, that is one such change. That sounds very likely, for again, ethnography is an art of the possible. Yet this is not to say that intellectual argument over changes and variations in the conduct of ethnography is useless. Perhaps these notes on experiences of multi-site fieldwork can contribute to such debate.

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# Ethnography in/of Transnational Processes: Following Gyres in the Worlds of Big Science and European Integration

*Stacia E. Zabusky*

Although this essay concerns scientists and citizens in Europe, I begin not with physics, politics, or polemics but with poetry, specifically these lines from William Butler Yeats's "The Second Coming":

> Turning and turning in the widening gyre
> The falcon cannot hear the falconer;
> Things fall apart; the centre cannot hold;
> Mere anarchy is loosed upon the world

It is not Yeats's theory of the end of Christendom and the coming of some "rough beast slouching towards Bethlehem" (or, in my case, Brussels) that concerns me here. I want instead to call attention to the image of the "widening gyre" with its constant, shifting movement, pulling apart a center that becomes more attenuated with every turn of the falcon's wings, a dissipating center over which the falconer has lost all control.

I will suggest here that when doing ethnography in moments and contexts of political reconstruction and instability, we – ethnographers and participants alike – confront and move not through "social fields" but "widening gyres" in which "the center cannot hold." In such contexts, centers displace other centers, peripheries mutate into centers, and centers and peripheries pile atop one another, now dissolving the distinctions, now recreating them in another place. No one is in control of this ongoing "gyration," this making and unmaking of centers – people stumble through these gyres, improvising some place to stand for a moment, a place where they try to get something done.

What are the implications for ethnography if we recognize social spaces delimited not by fields but instead by gyres? What, more particularly, are the implications for the ethnography of the privileged and the powerful, if

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these gyres simultaneously generate and displace centers? These are the questions I will address here through a consideration of what we might call a "happy case" of reconstruction and instability: space-science mission development in Western Europe.[1] I call this a happy case because the situation I address differs in significant respects from those addressed by many of the other contributors to this volume. Their essays discuss more painful and tragic cases as they confront instabilities arising out of war, violence, dislocation, and poverty.

Space science in Europe seems, by contrast, to take shape in a context of remarkable stability and comparative calm as its participants are able to pursue technical activities undisturbed by such massive traumas as urban poverty or genocide. Space science in Europe is in fact a domain of activity characterized by tremendous productivity; every year, scientists and engineers in Europe are involved in the launch of new missions, including such projects as the orbiting Hubble Space Telescope (in which the Europeans are active and integral partners with NASA), the Giotto space probe (launched by the European Space Agency in 1985 to observe Halley's comet at close range), and the Huygens space probe (a European Space Agency mission launched in the fall of 1977 to study Titan, one of Saturn's moons). These missions have been, even with inevitable and much publicized flaws, successful in innumerable ways: they return data to scientists that result in discoveries and publications; they push the boundaries of technological capabilities for engineers; they fuel high-tech industries with multimillion dollar contracts; and they contribute to the political viability and existential reality of the European Union. I will argue, however, that it is not political or professional *stability* that leads to this productivity; instead productivity emerges from the participants' ongoing *struggle* to find and make stability while engaged in the daily work of European cooperation in space science. It is a struggle because participants find themselves working at the intersection of two powerful transnational processes: European integration and big science. These processes continually destabilize productions as varied as satellites, individual careers, and government organizations, making it difficult to identify any one domain as the centralized locus of decision and activity. Thus, for participants, productivity is achieved through what they experience as instability and uncertainty, as they improvise moments of clarity in which work can get done and lives can be lived.

This is no easy task. Participants struggle to construct missions that meet their professional or intellectual needs in the face of constant shifts in political priorities, organizational policies, and economic reallocations over which they have little or no control. Those involved in mission development thus often recognize their achievements only in the past tense; in the present moment, and in the future that stretches before them as so many technical "milestones" to be reached, they can see only a barrage of impediments, as the fluid circumstances of European integration and big science engulf them in endless gyrations, unsettling them at every turn. This constant upheaval engenders experiences of frustration, alienation, and cynicism, as participants see themselves as unable to control core resources or direct those activities necessary to complete their work. Such experiences are countered, however, by the excitement that comes from improvising in the spaces of power, where participants make use of the cultural materials provided by science and state in an oscillating dynamic of domination and resistance that ultimately leads to production.

In what follows, I tell a story about how I came to notice these improvisations in the context of space-science mission development in Europe. The story begins with the problems I encountered when conceptualizing how to frame or conduct an ethnographic project in the transnational arenas of European integration and big science. It moves on to consider how I realized that there were neither clear-cut normative communities (for example, the international scientific community or even the European Community) nor definitive social fields (for example, bounded nation-states and organizations) in which I could pursue fieldwork in any traditional sense; there were, instead, widening gyres and constant improvisations. In the course of such realizations, I found the problem of power increasingly complicated and complicating.

## Context: Transnational Processes Reviewed

When I began to plan my project in 1987, intent on conducting fieldwork in a more or less traditional vein, the prospect of studying the large-scale transnational processes of European integration and big science from inside seemed daunting, to say the least. These were momentous and gargantuan undertakings, characterized as much, if not more, by institutional and bureaucratic maneuverings of grand proportions as by any set of local, intimate practices more amenable to ethnographic analysis. Moreover, both these processes seemed defined more by constant change than by definitive structures; at every moment, political, economic, and organizational realities shifted, challenging my ability to home in on a place I could call "the field."

I had to consider, in the first place, the larger political-economic context of reconstruction that defines European integration. Coming on the heels of the devastation wrought by World War II, this "regional impulse" (Twitchett 1980:7) seeks to produce a new and permanent set of political arrangements among the states of Europe. European integration is an attempt to forge a unified Europe, to turn multiple nation-states into a superstate with a new, singular "center." It is a project that operates at the level and borders of nation-states, where national economies, political parties, and social welfare policies dominate analysis and action. Although this integration takes place in multiple venues, the primary symbolic and institutional locus of this project is the European Union or EU (known at the time of my research as the European Community or EC).

To date, this movement toward integration has been highly successful. Institutions, treaties, laws, and contracts have established increasing connections across state boundaries; common standards for industry and manufacturing have been developed and put into practice; national economies are coordinated in the European Monetary System; borders have been demolished as people show their common red passport identifying them as co-citizens of this new superstate (see Bull 1993, Twitchett 1980, Varenne 1993). Thus, since its inception, integration as a project is one that has been constantly on the move; however, it is not always forward moving. It has been challenged repeatedly by problems of both identity and control.

The identity problem is in part definitional, as member states of the European Union ponder just what criteria (beyond economic health) are necessary and sufficient for extending membership to applicant nation-states. Who can be considered part of a unified "Europe"? For a long while, the primary question was whether the Scandinavian states and the traditionally neutral states of Switzerland and Austria would be interested in joining or would be welcomed by this common European market. Another vexing question was whether Turkey, eager to join in and already a member of NATO, could really be considered "western" enough to fit in to a European alliance. These questions of belonging, of who and what was culturally, properly, and legitimately European, have been challenged anew with the advent of perestroika and glasnost in the Soviet Union in the late 1980s and the fall of the Berlin Wall in 1989. Along with the restructuring of economies, politics, and social policies in the countries of the former Eastern Bloc, have come new applications from the countries of central and eastern Europe to join what had been a purely "western" European endeavor. The identity of the evolving superstate has been in constant flux, as voters and leaders consider who can be now and in the future definitively European.

The issues promoting instability in European integration concern more than identity and belonging, however, and address the ever-thorny issue of control. In the political arena, the transnational process of integration requires individual states to give up sovereignty in at least some arenas in return for increased collective power on a world stage. It is no easy matter, however, to demand that states give up power or redefine their centers, any more than it is an easy matter to identify citizens of these different states only as "Europeans," passports notwithstanding. Some "peripheral" groups have seen in this movement a liberating potential: women's groups, peace groups, environmental groups, and ethnic groups all have begun to take their cases and their issues directly to this new, growing center, in an effort to bypass those state structures which have silenced them for so long. Such groups seek incorporation and inclusion for all the diverse "peoples" of Europe (for example, see Darian-Smith 2002; Galtung 1989; Stephens 1993). Many others see in this new center a threat to their independence and identities, and, indeed, there have been frequent paroxysms of resistance to this project, whether in the form of Britain's refusal to join in the beginning and again under Margaret Thatcher, or in Charles de Gaulle's refusal to let Britain join when it finally wanted in, or in the withdrawal of Britain from the European Monetary System – or, on a more local level, the violent protests by French farmers and fishermen, and referendum in which Danish citizens rejected the Maastricht Treaty (only ultimately to vote to accept it). These independent movements, these denials and objections to increasing union are all part of the inexorable trend toward integration (see also Gerlach and Radcliffe 1979), themselves an expression of the profound "reconstruction" that is underway as Europe seeks to redefine its center(s).

Besides European integration, there were also the complex dynamics of big science to consider as I developed a fieldwork project. Big science, too, represents a form of transnational reconstruction, albeit an ongoing, temporary one, in that it challenges the defined and fixed boundaries of nation-states in order to reach the desired end. Historically, scientists have long respected and insisted on the value of cooperating across state boundaries; in the familiar rhetoric of science, all practitioners are accorded membership in an "international scientific community" which extends its citizenship to all scientists regardless of nationality (Zabusky n.d.). Accordingly, in the interests of pursuing their research scientists have always seemed particularly able to move about the world with ease, taking up residence now in this country, now in that, regardless of their countries of origin. As scientific interests became realizable only within more and more complex technologies, states and industrial capital became enmeshed in the development of these monumental, "disinterested" projects (see, for example, Galison and Hevly 1992). The idea of international cooperation is now glorified not only by scientists but also by states and industries, as they undertake big science in a context of global capitalism. Such projects by definition depend on transnational flows of people, technology, and capital for the production of functioning artifacts, whether they are the Channel Tunnel, the Ariane rocket, or the particle accelerator at the European Center for Nuclear Research (CERN).

Space science in Europe developed in a context of superpower rivalry; the "space race" between the former Soviet Union and the United States left the Europeans in a small, virtually invisible position to begin their own space pursuits. Their efforts required the formation of alliances among states, the infusion of capital into new central locations, and the conscription of human participants focusing on the myriad technical, economic, and social details that together constitute a large, integrated artifact (Zabusky 1995). These arrangements introduced, and continue to introduce, "instability" into state goals, particular institutions, and individual lives. States continue to shift, or manipulate, national interests to meet international goals of cooperation, accommodating other centers in an effort to retain some power for themselves. The infusion of capital leads to the establishment of joint institutions that then take on lives of their own, becoming new "centers" which have to be accommodated. And people must move, accommodating themselves not only to the pressures of cooperation on the microlevel but to the pressures of reconstructing homes and selves in foreign lands (see Zabusky 1995).

These transnational processes of European integration and big science, with their mammoth proportions and rationalizing tendencies, conjure up many of the dreams and terrors of modernity including those of centralized states and technological utopias (or dystopias). At the time that I was planning my research project, such nightmares did not constitute a typical domain for inquiry in cultural anthropology; although, in the wake of the cultural studies movement, anthropologists have increasingly turned an eye toward such nightmares (or fantasies, depending on one's point of view). Nonetheless, it was out of this contradictory and constantly changing maelstrom of state-building and science that I attempted to construct a field to which I could go to conduct an ethnographic research project.

## Conceptualizing Fieldwork: Theoretical Difficulties

I faced a series of conceptual problems when imagining my ethnographic project. A major problem was that, for a lone anthropologist interested in the grounded, lived experiences of real people working out the myriad details of daily life, it was difficult to conceptualize a "cultural" study of such macro-level, large-scale, encompassing phenomena as "international scientific cooperation" or "big science" or "European integration." These transnational domains seemed to be empty of people, defined instead, as I have briefly described here, by the political-economic and practical-technical goals of states, organizations, and technology. Without people, it seemed that there could be no ethnography. Or perhaps the problem really was that the only people who were there – making policy, signing memoranda of understanding, connecting wires, studying electronic signals – were people who had "culture" squeezed out of them by their involvement in the rational, instrumental, and technical practices of state, bureaucracy, and science. Bureaucrats, technocrats, scientists, and engineers often seemed to revel in their rationality rather than their culturality; Sharon Traweek (1988) described this phenomenon, in the case of American high-energy physicists, as "the culture of no culture."

This sense that people engaged in such activities are themselves empty of culture prevented anthropologists from turning their attention to these remarkable social processes for a long time, even after anthropologists began ethnographic study of Western societies. In part this resulted from the historical legacy of the divide between sociology and anthropology, in which sociology claimed as its disciplinary territory the study of modern, complex (Western) societies and their attendant institutions (such as organizations, professions, and science), while anthropology turned its disciplinary attention to the "non-Western world." (All this was rather like the way the superpowers divided up the globe into delimiting spheres of influence.) This division of labor itself, however, led to or at least reflected a particular facet of anthropology's world-view, namely, its preoccupation with and glorification of the exotic. As so many scholars have reminded us in recent years, cultural anthropology's fascination with the primitive, the marginal, and the peripheral, caused us to construct the Other in a particular way, in a way that fed our own needs and interests (see Clifford and Marcus 1986; Taussig 1987). Simultaneously, by taking the Others as objects of cultural study, we made it possible to ignore or forget that we, too, had culture. Only those Others out there, with strange customs, bizarre rituals, and mystical beliefs, all of which required explication and analysis in rational terms, had culture.

That perspective derives in part from the ideology of rationality on which modernity depends. The ideology itself defines rationality in opposition to culture, and, as Traweek's notion of the "culture of no culture" suggests, this is the point of view to which technocrats, scientists, and engineers of all kinds subscribe. Those involved in the work of organizations and technology insist that they transcend the petty preoccupations and contaminating influences of politics, identity, and emotion in their everyday working lives, if not their personal lives, although the boundary is drawn blurrily if at all. If, then, there were no culture in the processes of bureaucracy, technology, and science, then there was also no reason for anthropologists to pay any attention to those activities, organizations, or those involved in them, since anthropologists are supposed to study culture almost by definition.

In this way, anthropologists have long been seduced by the ideology of rationality that we also endeavor to critique, embracing the exotic objects of our inquiry out of a conviction that they, alone, continued to live in worlds enchanted by culture. Even with the erosion and loss brought about by imperialism, colonialism, state-building, capitalism, and increasing tourism, it has transpired that only those Others retain and contain the comforts and excitements of culture, while we (educated, bourgeois, professional), on the other hand, have been rendered bland and dull by modernity – "no culture here," we might say, whether approvingly or in lament. As Charles Taylor (1989) has written, to many of us in the contemporary world, it appears that life at the center is fundamentally "disenchanted."

That sense of emptiness haunted my initial efforts to imagine a cultural study of scientists and engineers at work on European space science missions, especially because at this time there were virtually no such studies to emulate. What could there possibly be to look at that would interest an anthropologist? Perhaps such studies should be left to our colleagues in other disciplines, with their highly developed analytic tools for examining rational choice, decision-making, institution-building, and the like. I remained convinced, nonetheless, that there were people with culture, even here, in the midst of bureaucracies and high technology, people whose lives we needed to understand ethnographically if we were to have a complete picture of the impact of modernity on ordinary lives in the Western world. Modernity, after all, was a cultural space and not simply a rational space, even if ideologies of rationality characterized its culture. Ethnography as a method of inquiry offered an opportunity to understand peoples' experiences of modernity, in its apparently new transnational incarnation, from "the natives' point of view" – which still seems to me to be the hallmark of an anthropological approach, the problematic status of that native notwithstanding.

## Conceptualizing Fieldwork: Practical Difficulties

Theoretical convictions aside, my knowing that people doing the work of modernity also had culture that needed to be studied ethnographically did not solve a second, more practical problem: to learn about living cultural processes, I had to go do fieldwork somewhere. Indeed, the explicit rules and regulations of dissertation approval and grant writing insisted that I establish an "area" and a "site" in which to do fieldwork. Moreover, the implicit rules of the discipline of anthropology mandated that I be an ethnographer, one known primarily by my geographic area and published and hired in terms of it. My interests in transnational processes, however, made such defining difficult. Most Europeanists, after all, went to particular villages or towns in particular nation-states, and funding opportunities were typically constructed according to such criteria. Where, though, was "Europe" (as opposed to its constituent nation-states)? Where was "the international scientific community" since it was, by definition, everywhere?

The existing ethnographic literature did not provide much guidance in this regard. Europeanist studies in anthropology had by and large focused on rural villages (Cole 1977; Ennew 1980) or perhaps urban enclaves and neighborhoods (Belmonte 1979; Kenny and Kertzer 1983). There was a decided lack of interest in the high-level, political machinations to produce a united Europe; this was a topic for political scientists to study. Cultural anthropologists found it difficult even to consider turning their attention to "Europe." The only way, it seemed, for anthropologists to attack the problem of "Europe" was at the village level, where analysis could show how European integration was irrelevant to, imposed on, or resisted by local people.

The literature in science studies, too, did not afford much support. There was a plethora of studies in the international dimensions of science, but these were historical, economic, and political in orientation (for example, Ben-David 1971, Merton 1973). There was a burgeoning literature in the ethnography of scientific work, led by the pioneering laboratory study of Bruno Latour and Steve Woolgar (1979), that claimed to use an anthropological approach to the study of science. However, the majority of such "laboratory ethnographies" were ensconced in the sociology of knowledge tradition, and their arguments were developed and carried out in opposition to a nonempirical philosophy of science (for example, Lynch 1985, Knorr-Cetina and Mulkay 1983). As such, these ethnographies, while using participant-observation techniques, were not framed in the universe of cultural questions typically posed by anthropologists. More problematic from the point of view of conceptualizing a fieldwork study of big science, such small-scale, micro-oriented studies of science created a cleanly bounded social space for ethnographic work; from these studies, it was difficult to know whether there were transnational processes that established or affected such laboratories, or whether there was any structure at all beyond the microinteractions around a laboratory bench (Hagendijk 1990). All context had been radically excised, precisely the kind of context that interested me. These were not studies of how, for instance, international scientific cooperation actually worked; these were instead studies of the epistemological processes and problems confronted by generic scientists – how they knew what they knew, not how they did what they did.

Villages and laboratories, then, did not provide me with a solution of how to conceptualize an ethnographic study of the massive reconstructions accompanying European integration and big science. Moreover, these transnational processes made it difficult to identify any one space or site where the productive work got done. Yet, I had to identify some bounded site that provided a home for the people who made up the amorphous international communities of science and of Europe, a place where I could pursue ethnographic inquiry. In the end, I decided quite self-consciously to conceptualize my study in terms of "semi-autonomous social fields," which are, as Sally Falk Moore (1978:55) has written, the "most suitable way of defining areas for social anthropological study in complex societies." Social fields generate their own internal rules and customs, yet are simultaneously "vulnerable to rules and decisions and other forces *emanating from the larger world by which [they are] surrounded*" (emphasis mine). Thus defined, the image of the social field was of ever-widening concentric circles, in which interior social fields were embedded.

I slowly narrowed my focus from the widest concentric circles of "European integration" and "scientific cooperation," until I found a circle small enough to do fieldwork in. The social field I first identified was the European Space Agency (ESA). Founded out of two progenitor organizations in 1974, ESA was dedicated to the production of space technology to benefit European commercial and intellectual life. At the same time, it proclaimed itself proudly to be (and was regarded as) a participant in the political efforts to forge a united Europe. For instance, Helmut Kohl, chancellor of Germany, stated during an address at the jubilee celebration marking ESA's twenty-fifth anniversary that "the joint European conquest and utilisation of space also strengthens the European identity, and this makes ESA's activities a major factor in building Europe as a political entity" (European Space Agency 1989:20). ESA appeared, thus, as just the kind of "semi-autonomous social field" Moore had in mind: a definable social, political, and economic organization embedded in the larger fields of European integration and international big science. But selecting the ESA did not immediately solve the fieldwork problem. The organization presented formidable methodological challenges since it was made up of many smaller establishments spread throughout Europe; the four primary ones included the headquarters in Paris, a telemetry center near Frankfurt, an archives and computation center near Rome, and the research and development center near Amsterdam, along with numerous subsidiary locations in Nice, Cologne, Madrid, Redu in Belgium, Sweden, and so on. Where to go?

I chose the European Space Research and Technology Center (ESTEC), the main research and technology center located in the Netherlands, where most of the technical and scientific work was carried out. In order to find a manageable social field in which to do my fieldwork, I narrowed my focus even further to a particular department in this institution – the Space Science Department (SSD). Here, ESA staff scientists were charged with the responsibility of coordinating the efforts of other scientists, engineers, and technicians from across Europe in designing, developing, and manufacturing ESA space-science missions. This was where I eventually took up residence to carry out ethnographic fieldwork from 1988 to 1989. In the time-honored manner of participant-observers since Malinowski, I spent my days watching and listening to the European professionals who worked together daily. My informants were engaged in their own magical and practical tasks: designing and testing spacecraft and instruments such as mass spectrometers, telescopes, and imagers; convening, attending, and complaining about meetings; and gathering data and dreaming about magnetic fields, cosmic radiation, stars, galaxies, and the solar wind. It was this work, this quotidian, technical, focused work, that turned out to be the stuff of transnationalism. This was where it all happened – transnational forces were produced in the turn of a screw, the click of a mouse, an argument in the corridor. Indeed, without the ordinary routines and decisions worked out here at this confluence of transnational streams, there would be no ESA, no European integration, no international scientific community.

## Executing Fieldwork: Ethnographic Challenges

I settled into a daily fieldwork routine at SSD to explore the contours of this social field. Overtly, my daily experience was straightforward and clear. There I was, undeniably in some *place* watching people design and develop some *thing*. Everyday, I traveled by bus to ESTEC, located by a small coastal village near the Hague, where scientists, engineers, computer specialists, technicians, statisticians, lawyers, and accountants from thirteen different European countries worked together to produce space missions for ESA. The physical plant occupied thirty-five beautifully landscaped hectares and included several multistory buildings of wood, concrete, metal, and glass. Inside the buildings, there was a proliferation of technical equipment: computer terminals in every office, huge laboratory facilities filled with instruments, and in-production satellites that protruded wires, antennae, and the shimmering blue panels of power-generating solar arrays. Inside the buildings, I observed countless meetings of teams, working groups, and departments, yawned during tedious sessions in the laboratories as technicians and engineers tested equipment, listened to arguments, and participated in conversations about frustrations and dreams.

It all seemed quite solid. I had found a clearly defined social field where work got done, a social field circumscribed by and embedded in larger, contextualizing fields, and a semiautonomous social field party inventing its own rules, partly dependent on the rules emanating toward it from some exterior, more encompassing, circle. Yet I was plagued by a vague sense that SSD was not far enough inside, that I needed to move in closer to a circle that was more relevant, more defined and confined than that of SSD. One reason for my unease was that, despite the presence of many technological artifacts, most of those relevant to space science missions rarely, if ever, made a physical appearance at ESTEC. Most of the time, spacecraft, instruments, and telescopes for space science missions were simply *represented*, visually in models, photographs, blueprints, and computer-generated designs, and in talk about specifications, delays, and testing.

Not only were the technological components not in evidence, but more and more it appeared that, organizational charts notwithstanding, neither was SSD. This administrative unit held little or no experiential significance for the scientists who worked at ESTEC. No one except upper management conceived of SSD as a meaningful social entity. Indicative of this lack was the fact that staff members routinely experienced tremendous difficulty in organizing department-wide activities. (For instance, the year that I was there, the SSD Christmas holiday party was canceled due to insufficient interest.) Moreover, people never referred to themselves as members of SSD but instead identified themselves by their profession or discipline (technician, engineer, astronomer, plasma physicist) or by the particular missions on which they were working (the Hubble Space Telescope, the Infrared Space Observatory, and so forth).

Eventually I realized that SSD was not a relevant or recognized, perhaps not even a genuine, social field for European scientists; rather, missions were what really mattered to people. Instead of caring about or orienting themselves toward the activities of others in SSD, SSD scientists focused on the activities of the teams and working groups of professionals in diverse disciplines, departments, organizations, and countries who were involved in making the mission a reality. These teams and working groups comprised networks of people in different locations but all working toward a common goal – the production of a particular spacecraft. In this way, missions served as, or perhaps produced, a significant kind of semi-autonomous social field, even though they did not exist in any single physical space but rather came to exist through the work of collaboration. The spacecraft in particular, with their technological components, scientific specifications, and data generation, seemed to focus everyone's attention inward to the kind of bounded space that constituted a clearly delineated field site at the heart of the powerful transnational processes I was trying to understand.

For this reason, after my initial exploration of the SSD environment, I decided instead to focus on space-science missions as the relevant social field of ethnographic inquiry. But this final, narrowing step, made in an effort to get to the inside of these concentric circles of social fields, proved to be my undoing. The further inside I got – the closer to the careful working out of such details as mass budgets, payload configuration, ground system planning, and the like – the less and less it seemed as if I were inside anything at all. In the day-to-day routines of SSD, I felt, as did participants, that the walls were dissolving. Mission work carried people off in different directions and oriented them toward other people and places even as they sat at their desks.

All around me, people were acting not as if they were at the interior of anything, not of SSD, nor ESTEC, nor even ESA. Instead, they treated such entities as opportunities, as resources, as gyres upon which they could hitch a temporary ride, only to get off again when other needs arose. They did this by literally breaching the walls of ESTEC: by traveling, phoning, faxing, and e-mailing. They did this symbolically, as they manipulated, constructed, and unraveled reams of documentation and the objects that these represented. Indeed, the visual and discursive representations of the technological artifacts that consumed participants' constant attention condensed and resonated with the participation of multiple professionals from multiple sites (institutions, countries, disciplines), making it impossible to think of myself as being inside a field in which I might either harvest or do ethnography; fields were too definite, places where things grow inside clearly marked boundaries. What could be considered boundaries here? The staff scientists were in constant contact with all kinds of "outsiders": other ESA staff members at other establishments, engineers in industry, scientists in European organizations, scientists in the United States, yet these were only "outsiders" to a social field called SSD. From the perspective of the social field called a "mission," these others were intimate and integral participants in the ongoing process of space-science mission development. Their work, moreover, created independent sites (or gyres) of mission-related activity. Missions depended on a process that could not be confined in or defined by any single organization, any more than the people who worked at that particular physical plant could be singularly identified with it. Every detail – from what materials to use, to what temperature to test them at, to where to buy them, to how many to include – required consideration and manipulation of social connections, alliances, and allegiances outside the offices of SSD, through the walls of ESTEC, beyond the limits of ESA, past even the borders of European states.

This was transnationalism in action. The process of space-science mission development contains no clear-cut interior or exterior; instead, every social field cuts across other social fields, creating areas of overlap at the moments and points at which they intersect. Overlapping is not embedding. This is not a series of regular concentric circles but instead is a wild ride on a gyre. To put it another way, instead of marking the limits and describing the interior features of some bounded field – with hedges, trees, rows of plants – enclosed within yet other territorial borders, I found myself standing in a field that was being inundated by streams of water flowing from multiple unseen points, washing away all trace of boundaries. The experience was not of "doing fieldwork" but of trying to "stem the tide" that seemed to sweep away physical or even discursive evidence of solid physical or social worlds. This is, as Emily Martin (1994) writes, the only experience possible in the "complex systems" which characterize the contemporary world. She argues that "the complexly interconnected world in which we now live seems to say that ... the current nature of reality ...

[is one in which all] is in flux, order is transient, nothing is independent, everything relates to everything else, and no one subsystem is ever necessarily continuously in charge" (250). Productivity, in such a maelstrom of interconnections, is vulnerable to even the smallest fluctuations in the most distant reaches of the system; thus, "the enormity of the 'management' task . . . becomes overwhelming. Who will manage all this? Is anyone in control?" (122).

Participants in ESA space-science mission development, too, recognized and articulated this experience of vulnerability, where the possibility of "catastrophic collapse" (Martin 1994:130) looms menacingly. As one ESA scientist said to me about the mission he was working on: "This mission is one of the most complicated things ever put together by mankind. It has lots of bells and whistles; it is really a big technological experiment which represents the limits of what humanity can handle. That's why no one knows everything about it, and there are always problems lying in corners waiting to be stumbled on." One person's corner often turned out to be another person's center, and there was no falconer to call anyone back from the spiraling gyres, no one to say, definitively, that this was it, the center, the source, the eye overseeing it all. In the dynamic currents of European integration, big science, and international scientific cooperation, technological components and human participants were swept along without regard to such social or physical boundaries as organizations or countries or corners.

This experience repeated itself over and over during my stay in SSD and posed significant challenges to the ethnographic endeavor. For instance, after I had been at ESTEC for some months and had made the transition to a focus on missions rather than the department, I finally felt that I had figured out all the important players and groups and technologies. But once again I had the familiar sensation of vertigo during an interview with an ESA scientist, Ian (a pseudonym), at SSD. Ian was responsible for coordinating the scientific aspects of an instrument that ESA was providing to a mission being sponsored by another European national space agency. In our conversation, Ian told me about the process of building this instrument, which included a prototype design that had been created in SSD, but which could be built in industry. The work on the electronics for this instrument had to be "contracted out," which mean that an industrial firm had to be hired to do the work. Selecting this firm was not in the hands of the scientists and engineers in SSD, however, but was rather in the hands of an international review board that was charged with the responsibility of ensuring that ESA distributed its contracts in a geographically fair manner: the principle of *juste retour* stipulated that the member states of ESA receive a "fair return" in proportion to their contributions to the agency's operating budget in the form of contracts for national industrial firms. This board, consisting primarily of industrial policy experts, reviewed documents provided by the scientists and engineers, who prepared these documents with the assistance of an ESA contracts officer; its decisions were primarily based on "bottom-line" issues. This board set up yet another review board under its auspices to review both the financial and the technical and quality control issues on which the scientists and engineers also reported. This board was known to be "fussy about format" and could conceivably ignore scientific or technical issues in favor of geography and/or budget.

I had never heard about these review boards before, even though this configuration of people, capital, artifacts, and services was quite significant in determining the current arrangements that I saw around me, since together these boards made all decisions about major contract allocations for components or other technological development costing more than U.S. $800,000. I could not have encountered these boards earlier as they did not exist anywhere in particular but appeared only at the moment when called into being by a set of rules and regulations and by ongoing practices of participants. Only at that moment would the members of these boards, scattered in different countries throughout Europe, appear as "the board," a social group as ephemeral as the meeting they would hold, not scientists versed in the intimate details of astronomy and detectors, yet powerful in determining the outcome of Ian's request. As I sat in that office talking to Ian, I had the sensation that a hole was forming in the office wall, and through that hole I could see an entire world that had been invisible to me, but where Ian entered and circulated with ease. This was no field; this was a gyre, and the acceleration of that widening arc threatened to catapult me through that hole and into some strange, new land. I struggled with that force, trying to decide whether to let go and follow Ian or whether to hold on tight, and try to keep the boundaries around this field intact.

My methodological and ethnographic efforts to limit, confine, and contain people in this or that community or to confine them or their artifacts to this or that field was thus rendered impossible as participants transcended, breached, or crossed such boundaries at every moment. Doing ethnography of the transnational, then, required not just identifying a "site" in the sense of a social field where I could settle down to do fieldwork; doing ethnography of the transnational meant instead being poised for movement, sensitive to flows and trajectories, to instability and reconstruction, to disruptions and reorientations. I had to be prepared to ride the ongoing currents of capital, people, and services, and to see that what was significant was not the organizations, the communities, or even the artifacts, but instead the practices that created all of these things. There was, in the end, no single, paramount place from which to observe these processes, no moment in time, no edifice, no organization that would provide the definitive view. Instead, I had to improvise my way through and to ethnographic research.

## Practices of Transnationalism: Improvisation and Cynicism

My efforts to discover the appropriate social fields for ethnographic inquiry in the complex transnational arenas of European integration and big science had led me to search for my own "village" deep inside more powerful forces. In the process, I made solid and structured the organization of ESA (and with it the establishment that was ESTEC and the department that was SSD). In the end, however, these entities turned out to have less experiential reality for participants than those villages of the proverbial "community studies" that were once the mainstay of ethnographic research in Europe. Indeed, physical edifices and the strictures of bureaucratic rules and regulations aside, in the day-to-day process of working together, ESA appeared almost as a mirage, constantly disappearing in the swirling currents. These currents created, recreated, and dissolved ESA over and over again. In response, everyone had to keep improvising some common ground on which to stand.

### *Structure and practice*

Before I explore the intricate dance of improvisation, I want briefly to turn my attention to structure, which certainly played a role both in the production and the ethnography of space-science missions. Structure, as the "outcome" and "medium" of ongoing practices (Giddens 1984), is reflected, for instance, in the accumulation of capital in certain places where "gyres" overlap (often instantiated as organizations and institutions). It was also apparent in the credentials and status accumulated by those who moved among various institutional and social networks. These credentials and status, often instantiated as titles like Project Manager or Principal Investigator or as academic degrees like PhD, signified access to capital and other resources. For instance, it was senior scientists with PhDs and high titles in university or research laboratories who served on ESA Science Teams; their status reflected their ability to control social and financial resources not available to those with less status or to those from poorer or marginal countries or institutions.

Despite the undeniable significance of such structural aspects of this mode of production, it is not enough to demarcate such structures and then to assume stability in production. These are, after all, complex systems in which multiple networks and institutional structures are at work. No matter how much status participating scientists might have accrued in one institutional domain (for example, as department head of a national research laboratory) or social network (for example, as leading scientist in the field of infrared astronomy), this status did not translate into power or even influence in the arenas through which these same scientists moved to produce a particular mission (for example, the Infrared Space Observatory Project managed by an ESA engineer and in which the scientists' role was advisory not supervisory).

Even in the local, day-to-day work on ESA space-science missions, participants felt this structural tension in their positions acutely. Although these were "scientific" missions, scientists were not in control of the development process. In concrete terms, this lack of control manifested itself in the fact that the budgets for space-science mission development were controlled by senior engineers (project managers) rather than scientists. During the course of development, scientists and engineers repeatedly found themselves pitted against each other in the ongoing negotiation of fund allocation that characterized the development process. Funds were always scarce, and unanticipated delays or errors in design or manufacture could drain the budget rapidly. Scientists routinely complained that all project managers wanted to do when faced with rising costs was to "descope the payload," that is, to cut down on the number and size of the scientific experiments that could be carried on board the satellite. These instruments were critical to the interests of the scientists since it was the instruments, rather than the satellite itself, that would collect and transmit the data that the scientists depended on for their research. Scientists thus complained that the engineers' efforts to save payload costs would in the end result in the launch of a "pointed brick," a hunk of metal incapable of detecting or imaging anything, even though it could be "pointed" at distant stars or galaxies. There was, thus, a structural tension at the core of this mode of production, a tension that was in part responsible for the dynamism of the process of space-science mission development.

## *Improvisation*

Despite these significant structural constraints – of status, time, money, and personnel – "in the end of the day," as one of the scientists I knew was fond of saying to me, the scientists could nonetheless be proud of the first-rate missions that they produced. They produced them, however, not because of the structures that determined their positions in various networks and flows of capital, goods, and services, but rather almost in spite of them. They produced them by exploiting the multiple structures, with concomitant contradictions and tensions, that constituted their domain of practice. Thus, just as my own ethnographic technique had turned out to depend on split-second decisions to leap onto other gyres or to ride out the trajectories I was on, so too did ESA mission participants' practices take shape as an endless series of ongoing improvisations, across dangerously flooded terrain, in which fields lay submerged beneath currents of uncertainty.

In certain respects, then, the participants' primary challenges were to establish some stability in the midst of this complexity so that they could get things done and move their projects forward. Participants resorted to various strategies for defining, even momentarily, some stability, as they tried to create spaces and moments where and when people could put their feet down. For instance, participants made every effort to construct discursive representations of stable social networks, in large part for the purpose of getting work done. Once constructed, they would step into them, or define themselves in relation to them, even if in the next moment these entities would evaporate. An example of this can be found in the way that the staff scientists in SSD, those who were the focus of my ethnographic inquiry, acknowledged their responsibility to "glue together a community." They talked about how "part of our work is to be out and about," meeting with scientists in other institutions and engineers in industry. This made sense to them "because the [scientific] community is all over the place, [so] you have to keep talking." By being "out and about," they demonstrated the ephemerality of ESTEC, despite its physical presence. It is not location with definable "insides" and "outsides" that determines a social field then, but rather "talk," at least in this case. In fact, as one participant said, "the only thing to do is to keep talking" if you want to get anything done. Thus, any community they attempted to forge could exist only in the moment, convened in talk to solve a particular problem, or to produce a specific result. "Community," said one SSD scientist, "is only the guys you know, sitting around a table, working together"; once "the guys" went off to their own offices, departments, institutes, or countries, whatever social group had appeared in that social space no longer existed.

In their ongoing efforts to make sense, to make connections, and to make stability, participants manipulated and asserted their commitment to various ideas and ideals, only to undermine their own assertions in the next moment. Nothing ever seemed secure. Instead of the rational practices ideologically associated with bureaucracy, state, science, and technology, in which conscripts harmoniously follow rules and enact regulations in the instrumental interests of efficiency and orderliness, there was instead constant argument, negotiation, and the bending of rules as people struggled, with politics, with scarcity, with emotional unpredictabilities, to produce artifacts that worked. Their struggles were carried out in terms of organizations (ESA, the European Union), of communities (the international scientific community and various national communities), and of other artifacts (detectors, computers, cryostats), but these solid structures did not define their practices, could not contain them, could not dictate them. They provided the terms in which argument, negotiation, and rule-bending occurred; they provided the resources with which people could do their arguing, negotiating, and manipulating; they appeared as the outcomes of these same practices (Giddens 1984) – but they were not the practices themselves.

In practice, people took these materials of power and improvised with them. These improvisations appeared as contradictory orientations in the practices and discourses of participants. For example, at some moments, SSD scientists and their colleagues would argue that nationality did not matter in the intense concentration required to design and build scientific instruments that would permit their users to discover the secrets of nature. ("When we get together in a meeting . . . it's not a question of being British, French, or whatever; we're just a group of scientists doing a job," said one SSD scientist.) In this gyre, science and technology defined a powerful center, the source of a widening spiral that pushed aside nations, people, politics, and money.

At other moments, these same scientists would describe their endeavors in terms of national differences, ascribing successes, failures, and strategies to different national customs or to different state interests. ("Italians are good at a lot of things, but not at making decisions," said one ESA engineer, while another SSD scientist opined that "if you think of German efficiency, Dr Schmidt is it" [Zabusky 1995].) In this gyre, nation-states and their attendant "mentalities" defined a powerful center, the source of a widening spiral that pushed science, technology, and nature into the margins. Sometimes, these same scientists would describe themselves as those who safeguard ESA's interests in the face of constant demands from naive scientists in the academic world, from the excesses and incompetencies of industrial firms, from the parochial concerns of separate European states (see Zabusky 1992). In this gyre, ESA defined a powerful center, the source of a widening spiral that pushed science and state to the edges. In still other moments, these same scientists would insist that industrial firms determined the missions selected and developed and that neither science, nor technology, nor national interests had much to do with it. In this gyre, capital defined a powerful center, the source of a widening spiral that pushed national pride, scientific interest, and bureaucratic mandates into the margins.

It is such protean discourses of identity and difference that are the improvisations through which participants cleared out spaces in which to do their work. At every moment, different gyres carried participants away from one center, making them more peripheral the further they followed its path. Every turn called up another improvisation, and, as multiple gyres spun of out of control, participants found themselves traveling from one to another, maintaining balance only by moving with the flows.

## *Cynicism*

It is no wonder, given the participants' sense of shifting and unstable centers, that participants also insisted that power was always somewhere else. Indeed, a critical part of life and work in the intersection of transnational flows is the overwhelming sense that the center – that place from which power emanates, dictating rules and actions – is always under someone else's control. Yet this sense that participants had of being out of control may seem paradoxical, or even hypocritical, given that the scientists and engineers who were the focus of my study worked for or at an organization that clearly appeared to be a source of power. In concrete, physical terms, this status was manifest at ESTEC in the chain-link security fence that surrounded the entire site, by the security policy that required visitors to leave their passports at the guardhouse, and by the multitude of BMWs, Mercedes, and Cadillacs found parked in its parking lots.

ESA is an undeniably wealthy organization – its budget in 1988, at the time of my fieldwork, was almost two billion dollars, amassed from the contributions made by its thirteen member states, and much of it was disbursed in turn to national industrial firms (Longdon 1989). It is also undeniably an organization run by and dedicated to the interests of the elite – a site for science research and high technology production, "inhabited" by scientists, engineers, and lawyers who oversee the work of less powerful personnel such as technicians and custodians, and oriented toward producing competitive technologies to help national industries compete in a global marketplace. It is also undeniably a dominant political player – its governing body is made up of high-level national politicians whose presence underscores ESA's role in the construction of a new European union. In all these ways, ESA seems to exist exclusively to further the interests of capital and state.

Nonetheless, the SSD scientists in particular often felt at odds with ESA, and they made great efforts to distinguish themselves from its powerful grip. They articulated their resistance to such interests often in terms of "science" (versus politics) and sometimes in terms of "community" (versus bureaucracy). Throughout, they struggled for legitimacy and control of a technical process in which they were integral and crucial participants yet, from their perspective, undervalued and marginalized.

The public rhetoric of ESA officials certainly made science seem significant in the overall scope of the agency's activities. A most eloquent statement of science's role in the agency was made by Reimar Lüst, director general of ESA during the time of my fieldwork, in a 1987 address:

> To me, European space is a living vibrant entity, and like all living things it has a heart. For European space, that heart is the Space Science Programme [the ESA directorate responsible for space science missions]. It pumps out the blood of new ideas, fresh challenges, and technical innovation to the limbs of the application programmes. (5)

As a living entity, it needed "oxygen," which could only be provided by more data produced by more missions that demanded more funds. This was not, in the history of the agency, easy to obtain. ESA had been established in 1964 at the instigation of scientists who, in Lüst's words, "jogged the political elbow" (2) to get a regional European space organization started. Lüst remarked that "in the beginning there was some doubt that the enthusiasm of the scientists could be turned into a political reality" (2), but he went on to describe the many successes of the early incarnation of ESA (then the European Space Research Organisation). By 1974, however, member states had grown restless, tired of expensive missions that did not produce a clear benefit to them in terms of improved industry or increased capital. As a result, they lobbied for the creation of a new organization that would focus on "applications missions," in other words, missions with commercial application, and out of this "revolt" came ESA (Russo 1993). The tension between missions dedicated to "pure" scientific research and "applied" commercial ventures remains within ESA; member states and industrial concerns do not see science as the lifeblood of the agency but as a drain on the agency's potentially more lucrative undertakings, and they still prefer to put their francs, marks, and pounds toward "useful" and "practical" missions such as heavy launchers (rockets), meteorological satellites, and communications satellites. For many years, indeed, the budget of the Science Programme did not increase with inflation; even in 1987, despite Lüst's impassioned rhetoric, science missions accounted for only 10 percent of ESA's overall budget (Longdon 1989).

This state of affairs – the disjunction between official rhetoric and actual conditions – generated feelings of alienation among the scientists I knew. Some non-ESA scientists talked about ESA as a "black cloud" that hung over their heads as they endeavored to make an instrument for a space-science satellite. One SSD scientist grumbled about the way science was treated as "a jewel in the crown" for ESA, an ornament that the organization used to legitimate its choices and its expenditures as being "for the good of human kind," without matching such rhetorical emphasis with significant resources. In more cynical terms, another SSD scientist complained to me that ESA was "just a money laundering scheme for government and industry," such that it was the needs of national industries that drove decisions about scientific missions rather than the needs of scientists.

For the scientists, this sense of not being where the power was, yet at the same time being called on to legitimate the capital expenditures of ESA and European states, engendered cynicism in spades. Cynicism appeared in various guises. Often, it appeared as complaints, whether about daily work practices such as those concerning the "pointed brick," or about the disparity between rhetoric and circumstances; these complaints were tempered by a shrug of shoulders, a knowing glance, a remark made to the naive ethnographer that "of course" this is the way it has always been: "I'm forty, so I'm more cynical," an SSD scientist commented wryly during an interview. At other times, it appeared as a kind of disengagement, as an inability to believe in or even dream of possibilities, whether technical, political, or cultural. For instance, one ESA publication made much of the fact that:

> The Agency itself, with its staff and committees made up of representatives of the Member States, constitutes one of the melting pots for the material from which Europe is gradually being forged, and in which nationalist preoccupations have to give way to wider, more promising vision. All who contribute to the life of the Agency have a sense of belonging to a European unity.... (Longdon and Guyenne 1984:229)

Such romantic imagery was easily and often countered by SSD scientists, who talked about the internecine conflict between the various member states and their representatives in the agency. There were stories about French politicians putting pressure on French scientists to vote for the selection of French-led missions. There was laughter about the insistence of Germany on having German treated as a third official language (the two official languages of ESA are French and English); several scientists remarked to me that the presence of multiple languages was evidence of political maneuverings and not of technical cooperation, where one language would not only do, but one was in fact necessary for successful working together. These were not stories of "more promising visions" but the weary remarks of embattled participants whose ability to determine what really mattered was severely restricted.

Discourses of cynicism can be understood as cultural expressions of the contradictions inherent in transnational processes, contradictions that arise as centers are continually undermined and power seems always to impinge on people from somewhere else as they travel in directions not of their own making. The scientists I watched had created elaborate space missions that produced transnational flows when they were first dreamed up as mission scenarios. However, once these missions entered into the development process, once put in motion, the scientists no longer directed the flows themselves, any more than the falconer directs the falcon how to fly. Instead, they were caught up in powerful forces over which they had no control, and they struggled, improvised, and complained their way to some stable center.

## Dreaming of Freedom in the Iron Cage

The shifting centers that accompany the transnational processes of big science and European integration produce not only improvisation and cynicism but dreams and domination as well. As participants slip from the grip of one source of power, they often – perhaps inadvertently – assume the mantle of power over yet another momentary collection of people, people who in turn evade this manifestation of power. The result is that everyone feels out of control, pushed and dominated by forces that they cannot see and over which they have no control. For this reason, scientists working on ESA space-science missions dreamed of freedom all the time. Their dreams of freedom often took the form of appeals to "science" to help them clear out a space where they could get something done. These appeals depicted a clean and pure science that needed to be unfettered and free if it were to lead all humanity to the truth. Scientists insisted on science's power to transcend the petty and confining details of national, political, economic, bureaucratic, and industrial interests. This idea of science at times insinuated itself as ideology, as a discourse of power that enabled participants to dominate others. Yet, at the same time, this idea of science subverted the power of others and served instead as a discourse of resistance that enabled SSD scientists to withstand domination from others. In this way, the interests of capital and state – which science was supposed to serve – were carried along by the transnational processes of big science and European integration and were transformed from materials of power to cultural resources that participants used to resist those same interests. Thus, these improvisations provided participants with a measure of freedom.

Nonetheless, the participant's experience was not of freedom but of struggle and confinement. Although they appeared to be working in a privileged world, full of money, power, and limitless possibilities, SSD scientists talked endlessly of constraints, obstacles, and impediments to their dreams. For them, the forces of big science and European integration seemed, on the one hand, to make their work possible and, on the other, to lock them in prisons – silencing, inhibiting, and oppressive. Why, then, continue to work within them? Because the scientists at SSD, as professionals everywhere, were called to their vocation with "passionate devotion" (Weber 1946). Most of the scientists I knew in SSD desperately wanted to *be* scientists, to work in their calling, to discover truth, to work for the social good. To be a scientist was not a choice but a duty; indeed, as Weber remarked, "The Puritan wanted to work in a calling; we are forced to do so" (1958:181). The hegemony of modernity thus forces all of us who work in the modern age to want to work, even to *need* to work, if we are to have any identity, any authenticity, at all. The scientists were, thus, trapped by their own desires, an outcome Weber saw clearly when he wrote that the "cosmos of the modern economic order . . . determine[s] the lives of all the individuals who are born into [it], with irresistible force" (1958:181). The scientists in SSD struggled every day to find the key to unlock this "iron cage" of modernity in which they felt themselves trapped, dreaming, as do all of us who are so caught, of freedom – from constraints, from contamination, from need, from power.

## *Reflecting*

Doing ethnography in the space of the transnational brings one to a confrontation with power. How to read that power, however, or where to locate it comes down, in the end, to a matter of taste or perhaps political preferences – which, in Pierre Bourdieu's (1984) terms, amounts to the same thing. In this project I found myself unable to exercise the kind of critique of power that seemed called for by other scholarly study, in part because I could find no steady field in which to stand and from which to exercise such a critique.

Anthropologists today call for reflexivity and show how studies of the Other – the subaltern, marginal, non-Western – can and must provoke us to reflect on ourselves, our own aims, the consequences of our actions. In doing so, many of these writers seem to claim for themselves a moral status built on suffering, a suffering they have not themselves endured, but which those they study or work with have endured. I cannot lay claim to such moral authority. The people among whom I pursued my studies are privileged and powerful compared to the typical subjects of ethnographic inquiry – just as I am. Indeed, although at times I felt uncomfortable conducting ethnographic research among such an elite stratum of an already privileged world, and I wanted to distance myself from the seductions of power and the complicity of privilege, most of the time I felt a kind of familiarity. I felt, indeed, that I was looking at myself. My informants' dreams were my dreams; their desires, my desires; their cynicism, my cynicism. As I rode the gyres with these scientists, watching them make their satellites (which fly higher than any falcon ever dreamed of flying), I realized that I was trapped as well, trapped in an iron cage of my own making, but from which, nonetheless, there could be no escape.

Confronting this reality was profoundly disturbing. By doing ethnography among people like me – participants in and producers of the transnational flows of modernity – I was forced to the kind of reflexivity for which anthropologists of non-Western people have argued. In the process, I learned that hegemony works on all of us, poor and privileged alike, propelling us to think its thoughts, to work for its goals – its goals become our goals; this is its power. Yet my experience was not only of unremitting power and privilege, secure at the center overseeing and controlling all that went on. As did the scientists, I, too, longed to resist those forces that pushed me, willy-nilly, against or even along with my will. Hegemony, whether or not it is in our interests, engenders a desire to escape, to be free. The paradox is that even our dreams (of liberation, of equality, and of diversity) are formed in the images of this power. Hegemony works just so, by making us all desire what it claims. The iron cage closes in on us more tightly. And yet there are those gyres, those unending spirals, catching us up and whisking us away to another space. There are those improvisations, that playfulness, that cynicism which dissolves the bars of the prison, even if for just a moment. Acknowledging that the center has vanished affects ethnographic choices. No fields exist but rather something less sure, less stable. In the end, at work among scientists and engineers in Europe, I recognized that there were places and moments when moral demands and aspirations emerged from the interstices of transnational processes and privileges and connected us through what we hoped for rather than through what we resisted. These are the gyres I prefer to follow in the improvisational flight that is ethnography in/of transnationalism.

# *Part VIII*
# Sensorial Fieldwork

*Antonius C. G. M. Robben*

Ethnographic fieldwork is generally presented in a written text, even though people's sensory experience of the world reaches far beyond verbal or written expression. This literary bias has resulted in the ethnographic neglect of the senses and the privileging of writing over other means of representation, such as photography, film, and sound recordings. Part VIII gives a sample, too small to our taste, of the ethnographic study of the senses, while adding a final reflexive article on this research field.

The Western classification of the senses into sight, touch, taste, smell, and hearing is not universal. For example, the Javanese do not recognize taste, but talking, as their fifth sense. Hausa emphasize sight as one sense but have only one word for the remaining senses, while some cultures add clairvoyance as a sixth sense (Howes and Classen 1991:257–8). Likewise, the Andaman Islanders perceive and organize time and space through an annual cycle of smells and a shifting olfactory landscape, while the Amazonian Suyá classify their flora and fauna into olfactory classes (Classen, Howes, and Synnott 1994:95–101, Seeger 1981). Some anthropologists have proposed the writing of ethnographies which describe all sensory experiences because people construct the world and relate to one another within an integrated sensorium (see Howes 1991). Furthermore, cultures may combine sensory perceptions in unique ways, and even individuals may do so when equipped with the synaesthetic ability to taste, smell, or see sounds. The difficulty of conveying the senses in a written narrative has drawn anthropology to other media such as photography, filmmaking, and sound recording.

Photography and moving pictures were used by ethnographers soon after they were invented. George Catlin took what are believed to be the first ethnographic photographs, only six years after the daguerreotype was invented in 1839, and Bronislaw Malinowski and Claude Lévi-Strauss were avid photographers (Prins 2004, Lévi-Strauss 1995, Young 1998). The first ethnographic film was shot in 1895 by the French physician-turned-anthropologist Félix-Louis Regnault. Alfred Haddon used film as a recording device during the 1898 Torres Straits expedition, and so did Franz Boas among the Kwakiutl (De Brigard 1995). These ethnographers used pictures and film mainly for reasons of illustration. Gregory Bateson and Margaret Mead were perhaps the first to employ photography and film as scientific instruments to analyze and interpret cultures. Bateson and Mead had been looking for ways to avoid the detachment of a too analytic etic approach and the inaccessibility of a too emphatic emic approach. Furthermore, they were struggling with the difficulties of expressing foreign cultural meanings, social practices, and intangible intracultural connections in a scientific (English) language. They took recourse to film and photography. The selection from their groundbreaking photo-ethnography *Balinese Character* (Bateson and Mead 1942) demonstrates that photographs can be more than an *aide-mémoire* for fieldworkers or a pictorial illustration for readers. Photographs become the primary causeway into Balinese culture and thus organize the data analysis, the written commentary, and the ethnographic interpretation.

The fieldwork approach, for Margaret Mead, consisted of taking notes, and for Gregory Bateson, recording on film. This division of labor continued upon their return home: Mead wrote the ethnographic interpretation and Bateson the photoanalysis. Mead's so-called "running field notes" followed the flow of observation and included detailed cross-references to the stills or moving pictures taken by her husband. She also kept a diary to record the principal occurrences in the village, and showed the footage to the Balinese for further commentary (Jacknis 1988).

Visual anthropology took flight after *Balinese Character* with the photoethnography on mortuary rituals in rural Greece by Loring Danforth and Alexander Tsiaras (1982), and the ethnographic documentaries by filmmakers such as Robert Gardner, Timothy Asch, John Marshall, and Jean Rouch (see Edwards 1992, Hockings 1995, Rouch 2003). Anthropologists soon realized that the visual image is not an accurate registration of reality, as early practitioners believed, but that "raw" photos and footage, just like "raw" field notes, are constructions encapsulating the intention, agency, and objectives of the ethnographer, and thus subject to reinterpretation. The finished product is the outcome of an elaborate editing process informed by an ethnographic understanding of the culture under study (Heider 1976, Scherer 1992).

*Balinese Character* was more than an experiment in ethnographic representation and analysis because of its attention to the senses. Margaret Mead explains in the introduction that she and her husband were interested in how pre-contact Balinese culture (i.e., before the arrival of Buddhist, Hindu, Christian, and Islamic influences) was reproduced and visibly manifested in everyday life. Bateson and Mead were under the sway of the American culture-and-personality school, and they believed that the study of postures, gestures, and interpersonal communications could uncover sedimented cultural features about Balinese personality, character, and ethos. Their reference to a hyperthyroid epidemic among the population, as if shedding the foreign cultural veneer to reveal some sort of ground plan of Balinese culture, fits this assumption. The three series of photographs selected here demonstrate how learning in Balinese culture is visual rather than verbal, and kinesthetic (body learning) instead of disciplinary. Bateson and Mead pay a great deal of attention to the cultural significance of nonverbal cues, subtle gestures, postures, the bending of joints, and the touching and spatial projection of the body, thus including a large part of the Balinese sensorium in their ethnography.

Paul Stoller and Cheryl Olkes, in their article "The Taste of Ethnographic Things" (1989), taken from Stoller's similarly titled book, show how a research interest in the senses opens up a whole new realm of anthropological understanding and interpretation. Something as seemingly trivial as serving bad sauce is full of cultural meaning and social relevance. In this way, the authors criticize the generalizing and objectifying aims of one particular approach in sensorial anthropology which removes the senses from people's daily experience, and situates them in a knowable social and cultural reality to develop a theory of the Senses, of Smell, Sight, Hearing, Touch, and Taste. After a deconstruction of Taste in Western philosophy and anthropology, Stoller and Olkes advocate a "tasteful ethnography" which focuses on subjectivity, sensuality, meaningfulness, authenticity, and dialogic and ironic qualities of taste. This mélange evokes the lived experience of taste, blending interpretation with description, evocation with insight, and representation with sensibility (see also Geurts 2002, Rasmussen 1999).

Stoller and Olkes accomplish such tasteful ethnography by revealing the significance of being presented with foul-smelling and foul-tasting sauces by their Songhay hosts in Niger. Not poor culinary skill, but sibling rivalry, ethnic tension, distrust, resistance to paternal authority, social transgression, a violation of custom, jealousy, frustration, and unsatisfactory reciprocity were the backdrop to the bad sauces. Hospitality turned into hostility through shameful sauces. Thus, the ethnographers' palate disclosed a new understanding of Songhay representation of domestic and fraternal conflict.

In the 1980s, there was a major shift from the anthropology of music to musical anthropology, as Seeger (1987:xiii) has pointed out. The anthropology of music studies what music adds to an existing culture (see Merriam 1964), while musical anthropology examines how music creates culture and social life itself. Steven Feld has been at the forefront of this shift, and his work is of particular importance to sensorial anthropology because of his interest in sound rather than music. In his article "Dialogic Editing: Interpreting How Kaluli Read *Sound and Sentiment*" (1987), Feld analyzes the reception of his ethnography *Sound and Sentiment* (Feld 1982) by the Kaluli people of Papua New Guinea. This fieldwork encounter involved "dialogic editing" in which informants reshape the ethnographer's interpretation of their culture in word, image, or sound. This fieldwork technique is particularly complex in a society as that of the Kaluli, which does not share our Western emphasis on the verbal expression of culture but structures storytelling, aesthetics, and sentiment through song and sound. The Kaluli and Feld agreed on the central importance of natural and human sounds to Kaluli culture. Nevertheless, Feld organized his ethnography around the aesthetics of local sentiment in singing and weeping, even though the people themselves were more concerned with memorable personal experiences than with ethnographic analysis and generalization.

The translation of English into the Bosavi language spoken by the Kaluli was the first dialogic obstacle between Steven Feld and his research participants because the word "translation" has three local connotations: transformation ("turnaround"), exposure ("turnover") and meaning ("underneaths"). Feld renarrated his ethnography by revealing submerged layers of meaning, recounting stories, redescribing activities, and reassigning ethnographic illustrations to their original actors. These conversations also demonstrated that every ethnography is a narrative construction which may not coincide with how people sense and represent their culture. The anthropologist is faced with restrictions imposed by publishers and peers with regard to length, composition, subject matter, analysis, logic, generalization, and theorization that are irrelevant to the people described.

These representational problems are magnified for sensorial anthropology. How could an ethnography ever do justice to the cultural prominence of sound in Kaluli culture? The Kaluli criticized Feld's ethnographic selections and the omission of everyday sounds, such as rain, thunder, and the morning noises of the awakening village. Feld resorted therefore to CD recordings. However, reproducing sound as a cultural system on CD is just as complicated as producing an ethnography through words and musical scores. After his rewarding experience with dialogic editing, Feld added dialogic auditing to yield a mélange of sounds that tried to evoke within the span of one hour the Kaluli experience of their local universe (Feld and Brenneis 2005). Like Stoller's tasteful ethnography, this approach prompted Feld (1991) to make a recording which represented a 24-hour period in the Kaluli village.

The sensory experience of the natural and social world passes through culture. The sensorium fuses physiology with culture in specific forms of classification, encoding, social practice, power, agency, and negotiation. This cultural mediation of sensory perception is, as Michael Herzfeld (2001) explains in the chapter "Senses" from his book *Anthropology: Theoretical Practice in Culture and Society*, the principal premise of the anthropology of the senses. The sensorium is the proverbial hardware which needs cultural software to become meaningful to people and allow them to interact. How people look, talk, sound, smell, or touch can influence whether they are stigmatized or treated with respect, and whether they are identified with one social group, community, and class or another.

Michael Herzfeld argues that the Cartesian bias of Western science, namely the separation of body (the sensorial domain) and mind (the ideational domain), is the principal obstacle to a mature anthropology of the senses. Western culture came to regard the mind as the seat of reason, and vision as its principal judge. Science became dominated by verbal and visual representation, while smell, touch, and taste were dismissed as too subjective. This bias became reinforced by the rapid development of technology which registered the world through visual means, whether in writing and diagrams or in photography and test equipment. We can also add Descartes's conviction that knowledge advances through ideas as a second influence, thus giving further credence to the superiority of the written word. Herzfeld encourages sensorial fieldworkers to make the study of the entire sensorium indispensable to other domains of ethnographic inquiry, such as economics or politics, in order to lift the anthropology of the senses out of its relative isolation. Just as attention to gender and reflexivity is now part and parcel of most ethnographic work, so the entire range of senses should become of similar concern. The rapid development of technical equipment and the declining costs and improved quality of sonic and visual reproduction are contributing to visual and musical anthropology in ways that may someday also benefit the ethnography of smell and taste.

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# Balinese Character: A Photographic Analysis

*Gregory Bateson and Margaret Mead*

## Introduction

The form of presentation used in this monograph is an experimental innovation. During the period from 1928 to 1936 we were separately engaged in efforts to translate aspects of culture never successfully recorded by the scientist, although often caught by the artist, into some form of communication sufficiently clear and sufficiently unequivocal to satisfy the requirements of scientific enquiry. "Coming of Age in Samoa," "Growing up in New Guinea," and "Sex and Temperament"[1] all attempted to communicate those intangible aspects of culture which had been vaguely referred to as its *ethos*. As no precise scientific vocabulary was available, the ordinary English words were used, with all their weight of culturally limited connotations, in an attempt to describe the way in which the emotional life of these various South Sea peoples was organized in culturally standardized forms. This method had many serious limitations: it transgressed the canons of precise and operational scientific exposition proper to science; it was far too dependent upon idiosyncratic factors of style and literary skill; it was difficult to duplicate; and it was difficult to evaluate.

Most serious of all, we know this about the relationship between culture and verbal concepts – that the words which one culture has invested with meaning are by the very accuracy of their cultural fit, singularly inappropriate as vehicles for precise comment upon another culture. Many anthropologists have been so impressed with this verbal inadequacy that they have attempted to sharpen their comment upon other cultures by very extensive borrowing from the native language. This procedure, however, in addition to being clumsy and forbidding, does not solve the problem, because the only method of translation available to make the native terms finally intelligible is still the use of our own culturally limited language. Attempts to substitute terms of cross-cultural validity, while they have been reasonably successful in the field of social organization, have proved exceedingly unsatisfactory when finer shades of cultural meaning were attempted.

Parallel with these attempts to rely upon ordinary English as a vehicle, the approach discussed in "Naven"[2] was being developed – an approach which sought to take the problem one step further by demonstrating how such categories as ethos, there defined as "a

Gregory Bateson and Margaret Mead, pp. xi–xvi, 49–51, 13–17, 84–90, and Plates 15–17 from Gregory Bateson and Margaret Mead, *Balinese Character: A Photographic Analysis* (New York: New York Academy of Sciences, 1942). Copyright, 1942, by The New York Academy of Sciences.

culturally standardized system of organization of the instincts and emotions of individuals," were not classifications of items of behavior but were abstractions which could be applied systematically to all items of behavior.

The first method has been criticized as journalistic – as an arbitrary selection of highly colored cases to illustrate types of behavior so alien to the reader that he continues to regard them as incredible. The second method was branded as too analytical – as neglecting the phenomena of a culture in order to intellectualize and schematize it. The first method was accused of being so synthetic that it became fiction, the second of being so analytic that it became disembodied methodological discussion.

In this monograph we are attempting a new method of stating the intangible relationships among different types of culturally standardized behavior by placing side by side mutually relevant photographs. Pieces of behavior, spatially and contextually separated – a trance dancer being carried in procession, a man looking up at an aeroplane, a servant greeting his master in a play, the painting of a dream – may all be relevant to a single discussion; the same emotional thread may run through them. To present them together in words, it is necessary either to resort to devices which are inevitably literary, or to dissect the living scenes so that only desiccated items remain.

By the use of photographs, the wholeness of each piece of behavior can be preserved, while the special cross-referencing desired can be obtained by placing the series of photographs on the same page. It is possible to avoid the artificial construction of a scene at which a man, watching a dance, also looks up at an aeroplane and has a dream; it is also possible to avoid diagramming the single element in these scenes which we wish to stress – the importance of levels in Balinese inter-personal relationships – in such a way that the reality of the scenes themselves is destroyed.

This is not a book about Balinese custom, but about the Balinese – about the way in which they, as living persons, moving, standing, eating, sleeping, dancing, and going into trance, embody that abstraction which (after we have abstracted it) we technically call culture.

We are interested in the steps by which workers in a new science solve piecemeal their problems of description and analysis, and in the relationship between what we now say about Balinese culture, with these new techniques, and what we have said with more imperfect means of communication about other cultures. A particular method of presentation has therefore been agreed upon. Margaret Mead has written the introductory description of Balinese character, which is needed to orient the reader so that the plates may be meaningful. She has used here the same order of vocabulary and the same verbal devices which have been made to do service in earlier descriptions of other cultures. Gregory Bateson will apply to the behavior depicted in the photographs the same sort of verbal analysis which he applied to his records of Iatmul transvestitism in "Naven," and the reader will have the photographic presentation itself to unite and carry further these two partial methods of describing the ethos of the Balinese.

Former students of Bali have approached Balinese culture as peripheral to and derivative from the higher cultures of India, China, and Java, carefully identifying in Bali the reduced and residual forms of the heroes of the Ramajana or of the Hindoo pantheon, or of the characters of the Chinese theater. All those items of Balinese culture which could not be assimilated to this picture of Asiatic diffusion have been variously classified as "Polynesian," "Indonesian," "animistic," or "*Bali aga*" (a term which some Balinese have learned to use in contradistinction to "*Bali Hindoe*"). We, however, always approached the material from the opposite point of view; we assumed that Bali had a cultural base upon which various intrusive elements had been progressively grafted over the centuries, and that the more rewarding approach would be to study this base first. We accordingly selected for our primary study a mountain village, Bajoeng Gede, near Kintamani in the District of Bangli, where most of the conspicuous elements of the later, intrusive culture were lacking. In Bajoeng Gede one does not find use of Hindoo names for the Gods, the importance of color in relation to direction in offerings, cremation, caste, the taboo upon eating beef, or any relationship to a Brahman priestly household. Writing there was, but only a half-dozen semi-literate individuals who were barely able to keep records of attendance, fines, etc. The village boasted one calendrical expert who was skilled enough to advise the village officials on the intricacies of the calendar of multiple interlocking weeks and "months." Furthermore, Bajoeng Gede was ceremonially bare, even compared with other Balinese mountain villages. These was a minimum of that reduplication and over-elaboration of art and ceremonialism which is such a marked characteristic of Balinese culture. Reliance on the calendar and complication of offerings and *rites de passage* were all reduced to a meager and skeletal minimum – a minimum which would nevertheless seem highly complex in comparison with most of the known cultures of the world. In this locality, it was possible in the course of a year to get a systematic understanding of the ground plan of the culture.

This undertaking was facilitated by two circumstances: the population of Bajoeng Gede suffered from a pronounced thyroid condition, with about 15 per cent of the population showing various degrees of simple goiter; and the whole population was markedly slow both in intellectual response and in speed of bodily movement. These circumstances, which are no doubt interrelated, provided us with a community in which the cultural emphases were schematically simplified, and upon our understanding of this base it was possible to graft – as the Balinese had before us – an understanding of the more complex versions of the same essential forms which we encountered on the plains. (It is important to remember that Hindoo culture came by way of Java, where the culture was related to that of Bali, and that most of the elements probably reached Bali in a partially assimilated form, already somewhat adapted to Balinese emphases and social structures.)

After an initial two months of exploration and work on the language in Oeboed (district of Gianjar), we selected Bajoeng Gede, and we worked there with only a few short absences from June 1936 to June 1937 and intermittently till February 1938. In November 1936, we established a second camp in Bangli in a palace built by a former Rajah, from which we were able during various short stays to participate in the family ceremonies of the ruling caste of Bangli. Finally in 1937, we built a pavilion in the courtyard of a Buddhistic Brahman family in the village of Batoean, from which position we participated in and studied Brahman family life, simultaneously collecting the work and studying the personality of the large group of Brahman and casteless painters in the school of art which had sprung up in Batoean during the last ten years.

Through Miss Belo's work in Sajan, a peasant plains village dominated by feudal Kesatrya nobles; Mrs Mershon's work in Sanoer, a coastal fishing village consisting mainly of Sivaistic Brahmans and casteless people; and from material provided by our Balinese secretary who came from a rising casteless family in Singaradja, the Dutch capital in North Bali, we were able to gather various sorts of comparative materials to round out the picture of Balinese culture which we had developed on the basis of observations in Bajoeng Gede. The discussions of Balinese culture in this book are based on these experiences, and on short excursions by ourselves and our collaborators to other villages and cities in Bali.

It is true that every village in Bali differs from every other in many conspicuous respects, and that there are even more striking differences between districts, so that no single concrete statement about Bali is true of all of Bali, and any negative statement about Bali must be made with the greatest caution. But through this diversity there runs a common ethos, whether one is observing the home of the highest caste, the Brahman, or of the simplest mountain peasant. The Brahman's greater ease, due to the fact that there are fewer of those who know much more than he, is but another version of the peasant's unwillingness to commit himself, of his "lest I err, being an illiterate man." The most conspicuous exceptions to this common ethos are the culture of the ruling caste, the Kesatryas, and the culture of North Bali which has been exposed to strong foreign influences during the last sixty years. In both of these groups may be found an emphasis upon the individual rather than upon his status, an element of social climbing and an uneasiness of tenure which contrast strongly with the rest of Bali. For this reason, reference to these two groups, except for occasional bits of ceremonial which they hold in common with the rest of Bali, has been excluded from this discussion.

In the Plates, each single illustration is dated and placed, and it is not safe to generalize from its detailed content for other parts of Bali. The form, however, the ethological emphasis which is implicit, may be taken to apply to all those parts of Bali of which we have any knowledge, except for North Bali, the Kesatryas, and the Vesias, a lower caste which mimics the Kesatryas and upon which we did very little work. These groups we explicitly exclude and we avoid all detailed negative statements as such statements are virtually impossible to make about a culture which has found it possible to combine such extraordinarily divergent content with such a consistent ethological emphasis. There is no apparent difference in the character structure of the people in villages where trance is shared by all and those in villages where no one ever goes into trance; people in villages where every other woman is believed to be a witch and those in villages where no one is believed to be a witch. In most of the cultures of which we have systematic knowledge, such matters are intricately and inextricably part of the personality of every participant member of the culture, but in Bali the same attitude of mind, the same system of posture and gesture, seems able to operate with these great contrasts in content with virtually no alteration in form. So also for climatic contrasts, and contrasts in wealth and poverty: the mountain people are dirtier, slower, and more suspicious than the plains people; the poor are more frightened than the rich, but the differences are in degree only; the same types of dirtiness, of suspicion, and anxiety are common at all levels.

This volume is in no sense a complete account of Balinese culture, even in its most general outlines. It is an attempt to present, at this time when scientific presentations are likely to be widely spaced, those aspects of our results and those methods of research which we have judged most likely to be of immediate use to other students. A less pregnant period of history might have dictated another choice of subject matter for our first presentation. Balinese culture, even that of Bajoeng Gede, is very rich and complex, and our two years' work, with two American collaborators and three Balinese secretaries, can only claim to be a "sampling" of the Balinese scene. We attempted to make systematic samples of village organization, calendrical ceremonial and *rites de passage*, trance, painting, carving, the shadow-play puppets, death rituals, and child behavior, so as to provide a series of crosscutting pictures of the culture which could be fitted together and cross checked against each other. The discussion which follows is a synthetic statement based upon these various samples; the photographs are a carefully selected series, analyzed on the basis of the same sampling.

Finally a word about the relevance of such researches to the period of history in which we find ourselves. Balinese culture is in many ways less like our own than any other which has yet been recorded. It is also a culture in which the ordinary adjustment of the individual approximates in form the sort of maladjustment which, in our own cultural setting, we call schizoid. As the toll of dementia praecox among our own population continues to rise, it becomes increasingly important for us to know the bases in childhood experience which predispose to this condition, and we need to know how such predisposition can be culturally handled, so that it does not become maladjustment.

Meanwhile, we are faced with the problem of building a new world; we have to reorient the old values of many contrasting and contradictory cultural systems into a new form which will use but transcend them all, draw on their respective strengths and allow for their respective weaknesses. We have to build a culture richer and more rewarding than any that the world has ever seen. This can only be done through a disciplined science of human relations and such a science is built by drawing out from very detailed, concrete materials, such as these, the relevant abstractions – the vocabulary which will help us to plan an integrated world.

## Notes on the Photographs and Captions

### *Taking the photographs*

. . . We tried to use the still and the moving-picture cameras to get a record of Balinese behavior, and this is a very different matter from the preparation of "documentary" film or photographs. We tried to shoot what happened normally and spontaneously, rather than to decide upon the norms and then get Balinese to go through these behaviors in suitable lighting. We treated the cameras in the field as recording instruments, not as devices for illustrating our theses.

Four factors may be mentioned which contributed to diminish camera consciousness in our subjects:

A. The very large number of photographs taken. In two years we took about 25,000 Leica stills and about 22,000 feet of 16mm. film, and it is almost impossible to maintain camera consciousness after the first dozen shots.

B. The fact that we never asked to take pictures, but just took them as a matter of routine, wearing or carrying the two cameras day in and day out, so that the photographer himself ceased to be camera conscious.

C. We habitually directed attention to our photographing of small babies, and the parents overlooked the fact that they also were included in the pictures (as even American parents will, in similar circumstances).

D. We occasionally used an angular view finder for shots when the subject might be expected to dislike being photographed at that particular moment.

We usually worked together, Margaret Mead keeping verbal notes on the behavior and Gregory Bateson moving around in and out of the scene with the two cameras. The verbal record included frequent notes on the time and occasional notes on the photographer's movements, such as the direction from which he was working and which instrument he was using. Whenever a new roll of film was inserted in the camera, the date and time of insertion were scribbled on the leader; and when the film was removed, the date and time were again recorded, so that the film could be accurately fitted to the notes.

For work of this sort it is essential to have at least two workers in close cooperation. The photographic sequence is almost valueless without a verbal account of what occurred, and it is not possible to take full notes while manipulating cameras. The photographer, with his eye glued to a view finder and moving about, gets a very imperfect view of what is actually happening, and Margaret Mead (who is able to write with only an occasional glance at her notebook) had a much fuller view of the scene than Gregory Bateson. She was able to do some very necessary directing of the photography, calling the photographer's attention to one or another child or to some special play which was beginning on the other side of the yard. Occasionally, when we were working on family scenes, we were accompanied by our native secretary, I Made Kaler. He would engage in ethnographic interviews with the parents, or take verbatim notes on the conversations.

In a great many instances, we created the *context* in which the notes and photographs were taken, e.g., by paying for the dance, or asking a mother to delay the bathing of her child until the sun was high, but this is very different from posing the photographs. Payment for theatrical performances is the economic base upon which the Balinese theater depends, and the extra emphasis given to the baby served to diminish the mother's awareness that she was to be photographed. A visit "to photograph the baby being bathed" would last from fifteen minutes to two hours, and the greater part of the time after the bathing would be spent watching the family in a large variety of types of play other behavior. In such a setting, a roll of Leica film (about 40 exposures) lasted from five to fifteen minutes.

### *Selection of photographs*

Selection of data must occur in any scientific recording and exposition, but it is important that the principles of selection be stated. In the field, we were guided first by certain major assumptions, e.g., that parent-child relationships and relationships between siblings are likely to be more rewarding than agricultural techniques. We therefore selected especially contexts and sequences of this sort. We recorded as fully as possible what happened while we were in the houseyard, and it is so hard to predict behavior that it was scarcely possible to select particular postures or gestures for photographic recording. In general, we found that any attempt to select for special details was fatal, and that the best results were obtained when the photography was most rapid and almost random. . . .

One rather curious type of selection did occur. We were compelled to economize on motion-picture film, and disregarding the future difficulties of exposition, we assumed that the still photography and the motion-picture film *together* would constitute our record of behavior. We therefore reserved the motion-picture camera for the more active and interesting moments, and recorded the slower and less significant behaviors with the still camera. The present book is illustrated solely by photographs taken with the latter, and as a result, the book contains no photograph of a father suckling his child at the nipple, and the series of kris dancers leaves much to be desired.

After taking the photographs, a further selection occurred. On returning to America, we had the entire collection of 25,000 frames printed as diapositives on strips of positive film, and in planning this book we made a list of categories which we intended to illustrate – a list similar to, but not identical with, the grouping of the plates in the table of Contents. We then projected all the diapositives, one by one, and wrote category cards for those which seemed to merit further consideration for inclusion in the book. We thus obtained a list of about 6,000 frames. Of these, we enlarged approximately the first 4,000 in chronological order, desisting at this point because time was short. From these 4,000, the majority of the prints reproduced here were selected, and we only drew upon the later negatives for a few special points which were not represented in the earlier series. The book thus contains a disproportionate number of photographs taken in the first three-quarters of our time in Bali.

The final choice of photographs for each plate was in terms of relevance, photographic quality, and size. In a number of cases, relevance to a problem is necessarily two-sided; there would be some photographs making one half of a psychological generalization, and others making a converse or obverse point. In these cases, we have tried to arrange the photographs so that most of the plate is occupied with the more typical aspect, while a statement of the obverse is given by one or two photographs at the bottom (usually in the right-hand corner) of the plate. In other cases, it has seemed worth-while to devote two plates to the contrasting aspects of the same generalization.

Conflict between scientific relevance and photographic merit has usually been easily settled in favor of the former, and a large number of pictures have been included in spite of photographic faults. Selection by size was more distressing. Each plate was to be reproduced as a unit and therefore we had the task of preparing prints which would fit together in laying out the plate. Working with this large collection of negatives, it was not possible to plan the lay-out in advance, and therefore, in the case of the more important photographs, two prints of different sizes were prepared. Even with this precaution, the purely physical problems of space and composition on the plate have eliminated a few photographs which we would have liked to include.

## *Learning* (Plates 15 to 17)

When the Balinese baby is born, the midwife, even at the moment of lifting him in her arms, will put words in his mouth, commenting, "I am just a poor little newborn baby, and I don't know how to talk properly, but I am very grateful to you, honorable people, who have entered this pig sty of a house to see me born." And from that moment, all through babyhood, the child is fitted into a frame of behavior, of imputed speech and imputed thought and complex gesture, far beyond his skill and maturity. The first time that he answers "*Tiang*," the self-subordinating ego pronoun, to a stranger, he will be echoing a word that has already been said, on his behalf and in his hearing, hundreds of times. Where the American mother attempts to get the child to parrot simple courtesy phrases, the Balinese mother simply recites them, glibly, in the first person, and the child finally slips into speech, as into an old garment, worn before, but fitted on by another hand.

As with speech, so with posture and gesture. The right hand must be distinguished from the left; the right hand touches food, and the right thumb may be used in pointing; the left hand is the hand with which one cleanses oneself, or protects one's genitals in bathing, and must never be used to touch food, to point, or to receive a gift. But the Balinese mother or nurse carries a child, either in or out of a sling, on her left hip, thus leaving her own right hand free. In this position, the baby's *left* arm is free, while the right is frequently pinioned in against the breast, or at best extended behind the mother's back. Naturally, when a baby is offered a flower or a bit of cake, it reaches for it with the free left hand, and the mother or the child nurse invariably pulls the left hand back, extricates the baby's right hand – usually limp and motiveless under this interference with the free gesture – and extends the right hand to receive the gift. This training is begun long before the child is able to learn the distinction, begun in fact as soon as the child is able to grasp at a proffered object, and discontinued usually when the child is off the hip. A three-year-old may often err and receive a casual present in his left hand, with no more punishment than to have some older child or nearby adult shout "*Noenas!*" ("Ask!") which means "Cup the right hand in the left," but the baby of four months is permitted no such leeway. Over and over again, the first spontaneous gesture is clipped off, and a passive, plastic gesture is substituted.

Meanwhile, the child in the sling, or supported lightly on the carrier's hip, has learned to accommodate itself passively to the carrier's movements; to sleep, with head swaying groggily from side to side, as the carrier pounds rice; or to hang limp on the hip of a small girl who is playing "crack-the-whip." Surrendering all autonomy, and passively following the words spoken in its name or the rhythm of the person who carries it or the hand which snatches its hand back from a spontaneous gesture, the child's body becomes more waxy and flexible as it grows older; and gestures which are all echoes of an experienced pattern replace such spontaneous gestures of infancy as the pounding of the child's silver bracelets on any convenient board. This accommodation to the movements of others, to cues that come from a pattern rather than from a desire, is facilitated by the extent to which a Balinese child is carried. There is a strong objection to letting a child be seen crawling – an animal activity – by any but the family intimates; and babies, even after they are able to crawl and toddle, are still carried most of the time. The position on the hip limits spontaneity to the arms and the carrier's repetitive interference with hand gestures reduces it there.

Even at its 105-day birthday, the infant is dressed in full adult costume. The infant boy is seated in a parent's arms, and a headcloth ten times too large for him is arranged at least for a moment on his head. The infant's hands are put through the gestures of prayer, of receiving holy water, and of wafting the essence of the holy offering toward himself. By the 210-day birthday, the child will repeat these gestures himself, sitting dreamily, after the ceremony, clasping and unclasping his tiny hands, and then speculatively examining them, finger by finger. At this age also, before he can walk, he will be taught simple hand dance gestures, first by manual manipulation, and later he will learn to follow visual cues, as the parent hums the familiar music and gestures before the baby's eyes with his own hand. This situation, the child dancing in the sustaining arm of the parent and that arm vibrating rhythmically to the music, becomes the prototype of Balinese learning in which as he grows older he will learn with his eyes and with his muscles. But the learning with the eyes is never separated from a sort of physical identification with the model. The baby girl climbs down off her mother's hip to lift a bit of an offering to her head, when her mother or elder sister does the same.

Learning to walk, learning the first appropriate gestures of playing musical instruments, learning to eat, and to dance are all accomplished with the teacher behind the pupil, conveying directly by pressure, and almost always with a minimum of words, the gesture to be performed. Under such a system of learning, one can only learn if one is completely relaxed and if will and consciousness as we understand those terms are almost in abeyance. The flexible body of the dancing pupil is twisted and turned in the teacher's hands; teacher and pupil go through the proper gesture, then suddenly the teacher springs aside, leaving the pupil to continue the pattern to which he has surrendered himself, sometimes with the teacher continuing it so that the pupil can watch him as he dances. Learning with the eyes flows directly from learning passively while one's own body is being manipulated by another.

The Balinese learn virtually nothing from verbal instruction and most Balinese adults are incapable of following out the three consecutive orders which we regard as the sign of a normal three-year-old intelligence. The only way in which it is possible to give complex verbal instructions is to pause after each detail and let the listener repeat the detail, feeling his way into the instruction. Thus all orders tend to have a pattern like this. "You know the box?" "What box?" "The black one." "What black one?" "The black one in the east corner of the kitchen." "In the east corner?" "Yes, the black one. Go and get it." "I should go and get the black box in the east corner of the kitchen?" "Yes." Only by such laborious assimilation of words into word gestures made by oneself, do words come to have any meaning for action.

This same peculiarity is found in the pattern of story telling. The Balinese story teller does not continue gaily along through a long take, as the story tellers of most cultures do, but he makes a simple statement, "There was once a princess," to which his auditors answer, "Where did she live?" or "What was her name?" and so on, until the narrative has been communicated in dialogue. A thread, even a simple verbal thread, in which one's body plays no role, has no continuous meaning.

There is rarely any discernible relationship between the conversation of a group of Balinese and the activity which they are performing. Words must be captured and repeated to have meaning for action, but there is no need at all to translate action into words. One might listen at a spy hole for an hour to a busy group, hearing every word spoken, and be no wiser in the end as to whether they were making offerings, or painting pictures, or cooking a meal. The occasional "Give me that!" is interspersed with bits of comic opera, skits and caricatures, songs and punning and repartee. As Americans doodle on a piece of paper while attending to the words of a lecture, so the Balinese doodles in words, while his body flawlessly and quickly attends to the job in hand.

All learning in Bali depends upon some measure of identification, and we may consider as prototype of such learning, the child's continuous adaptation to movements into which it is guided by the parent who holds it. Lacking such identification, no learning will occur, and this becomes specially conspicuous when one attempts to teach a Balinese some new foreign technique. Most Balinese will balk and make no attempt to copy a European, or perform any act, no matter how simple, which only a European has been seen to perform. But if once one can persuade one Balinese to master a European skill, then other Balinese of the same or superior caste position will learn it very quickly. So in training our Balinese secretaries, we had no difficulty because I Made Kaler, our secretary, educated in Java, believed that he could do what Europeans did, just as he could speak their language, sit on their chairs and handle their tools. Other Balinese boys, seeing Made Kaler use a typewriter, learned to type accurately and well in a few days.

This particularistic identification with the movement and skill of other bodies, socially comparable to one's own, has undoubtedly served as a conservative element in Bali, maintaining the division of labor between the sexes, and partially limiting certain skills, like writing, to the high castes. Only by invoking some such explanation can we understand the division of labor in Bali. The system works smoothly and accurately but with a total absence of sanctions. In the few cases of women who become scholars or musicians, or men who become skilled in weaving, no one even bothers to comment on the odd circumstance. And those who cross the sex division of labor are not penalized; they are not regarded as more or less masculine or feminine nor confused with the occasional transvestite, although the latter includes the occupations of the opposite sex in his transvesticism. But without sanctions, with freedom to embrace any occupation, ninety-nine out of a hundred Balinese adhere simply to the conventions that spinning, weaving, making most offerings, etc., are women's work, whereas carving, painting, music, making certain other offerings, etc., are men's work.

Combined with this kinaesthetic type of learning and with the continuous insistence upon levels and directions, there is a preoccupation with balance, which expressed itself in various ways. When the young male child is still learning to walk, loss of balance or any other failure evokes a regular response: he immediately clutches at his penis, and often, to be sure of balance, walks holding on to it. Little girls clasp their arms in front of them, and sometimes hold on to their heads. As they grow older, an increased sense of balance makes it possible to stand motionless for quite a long time on one foot; but dancing on one foot, playing too freely with a preciously achieved and highly developed balance is associated with witches and demons. Just as in witchcraft, right and left are reversed, so also in witchcraft, the decent boundaries of body posture are trespassed upon.

Balinese children, especially little Balinese girls, spend a great deal of time playing with the joints of their fingers, experimenting with bending them back until the finger lies almost parallel with the back of the hand. The more coordinated and disciplined the motion of the body becomes, the smaller the muscle groups with which a Balinese operates. Where an American or a New Guinea native will involve almost every muscle in his body to pick up a pin, the Balinese merely uses the muscles immediately relevant to the act, leaving the rest of the body undisturbed. Total involvement in any activity occurs in trance and in children's tantrums, but for the rest, an act is *not* performed by the whole body. The involved muscle does not draw all the others into a unified act, but smoothly and simply, a few small units are moved – the fingers alone, the hand and forearm alone, or the eyes alone, as in the characteristic Balinese habit of slewing the eyes to one side without turning the head.

## Plate 15 Visual and Kinaesthetic Learning I

An individual's character structure, his attitudes toward himself and his interpretations of experience are conditioned not only by what he learns, but also by the methods of his learning. If he is brought up in habits of rote learning, his character will be profoundly different from what would result from habits of learning by insight.

Among the Balinese, learning is very rarely dependent upon verbal teaching. Instead, the methods of learning are visual and kinaesthetic. The pupil either watches some other individual perform the act or he is made to perform the act by the teacher who holds his limbs and moves them correctly. During this process the pupil is entirely limp and appears to exhibit no resistant muscular tensions. A Balinese hand, if you hold it and manipulate the fingers, is perfectly limp like the hand of a monkey or of a corpse.

1, 2, and 3. Learning to carry on the head. These three photographs were all taken on the same occasion and show a girl (fig. 2) preparing to go home from a temple feast, carrying on her head the offerings which her family sent to the ceremony. Figs. 1 and 3 show two smaller girls imitating her and so beginning to participate in the ceremonial life of the village.
Fig. 1, I Djani; fig. 2, I Maderi (unrelated); fig. 3, I Djana (younger sister of I Djani).
Bajoeng Gede. June 23, 1937. 11 Z 30, 26, 33.

4 and 5. A father teaches his son to dance, humming a tune and posturing with his hand. In the first picture, the father shapes his facial expression to a typical dance smile and the son looks at the raised hand. In the second picture, the son tries to grasp the arm, and the father's expression becomes inter-personal instead of stylized.
Nang Oera, the father; I Karba, the son, aged 265 days.
Bajoeng Gede. Oct, 1, 1936, 2 U 30, 31.

6. The same father teaches his son to play the xylophone.
Nang Oera; I Karba, aged 393 days.
Bajoeng Gede. Feb. 5, 1937. 4 S 1.

7. A child nurse teaches the same baby to walk. She holds the baby by the upper part of the arms. There was no baby in her household and she spent a great part of her time looking after her father's step-brother's child. This photograph of learning to walk was taken five months later than the photographs of the same child learning to dance.
I Djeben teaching I Karba, aged 414 days.
Bajoeng Gede. March 26, 1937. 6 F 15.

8. Small high-caste boys learning to draw in the sand. The boy in the center was the most skilled and the others stopped their own drawing to watch him. All three boys show the typical Balinese high kinaesthetic awareness in the hands, and this is heightened by their using very small twigs for their drawing.
I. B. Saboeh; I Dewa Moeklen; I Dewa Loepiah.
Batoean. Oct. 5, 1937. 16 M 2.


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## Plate 16 Visual and Kinaesthetic Learning II

Teaching by muscular rote in which the pupil is made to perform the correct movements is most strikingly developed in the dancing lesson.

Mario of Tabanan, the teacher in this sequence, is the dancer chiefly responsible for the evolution of the *kebiar* dance which has become very popular in Bali in the last twenty years. The dance is performed sitting in a square space surrounded by the instruments of the orchestra, but though the principal emphasis is upon the head and hands, the dance involves the whole body, and Mario has introduced a great deal of virtuosity into the difficult feat of rapid locomotion without rising from the sitting position. The chief faults in the pupil's dancing are that he dances only with his head and arms, and does not show the disharmonic tensions characteristic of the dance.

This sequence of photographs illustrates two essential points in Balinese character formation. From his dancing lesson, the pupil learns passivity, and he acquires a separate awareness in the different parts of the body.

1. The pupil dances alone while Mario watches in the background. Note the imperfect development of the pupil's finger posture.

2. Mario comes forward to show the pupil how it should be danced.

3. Mario urges the pupil to straighten up the small of his back. Note that this instruction is given by gesture rather than by words.

4. Mario's hand position and facial expression while demonstrating.

5. Mario takes the pupil by the wrists and swings him across the dancing space.

6. Mario makes his pupil dance correctly by holding his hands and forcing him to move as he should. Note that Mario is actually dancing in this photograph, and that he postures with his fingers even while holding the pupil's hands. The position of Mario's left elbow in these photographs is characteristic of the tensions developed in this dance.

7. Mario even assumes the conventional sweet impersonal smile of the dancer while he moves the pupil's arms and holds the pupil tightly between his knees to correct his tendency to bend the small of his back.

8. Mario again tries to correct the pupil's tendency to bend his back.
I Mario of Tabanan teaching I Dewa P. Djaja of Kedere.
Tabanan. Dec. 1, 1936. 3 O 11, 13, 14, 17, 21, 22, 23, 25.

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## Plate 17 Balance

Plates 15 and 16 taken together give us indications about the Balinese body image. We have, on the one hand, the fantasy of the inverted body with its head on the pubes; and on the other, the Balinese methods of learning through their muscles, the discrepant muscular tensions which are characteristic of their dancing, and the independent movement and posturing of the separate fingers in dance. We have, in fact, a double series of motifs – indications that the body is a single unit as perfectly integrated as any single organ, and contrasting indications that the body is made up of separate parts, each of which is as perfectly integrated as the whole.

This plate illustrates the motif of the perfectly integrated body image.

1 and 2. A small boy learns to stand and walk. His father has set up for him in the houseyard a horizontal bamboo supported on two posts (*penegtegan*). The boy learns to walk by using this as a support.

The topology of this arrangement is the precise opposite of that of the play-pen of Western culture. The Western child is confined within restricting limits and would like to escape from them; the Balinese child is supported within a central area and is frightened of departure from this support.

In fig. 2, when unsure of his balance, he holds onto his penis. This method of reassurance is common in Balinese baby boys.

I Karba, aged 414 days; I Kenjoen, his cousin, aged 317 days, behind him.

Bajoeng Gede. March 26, 1937. 6 F 20, 21.

3. A baby girl unsure of her balance. She clasps her hands in front of her abdomen.

I Kangoen.

Bajoeng Gede. April 21, 1937. 7 A 15.

4. A child nurse picks a baby from the ground. Note the straightness of the small of the back and the resulting emphasis on the buttocks.

I Njantel picks up I Karba; I Dani watches.

Bajoeng Gede. May 13, 1937. 8 U 30.

5. A girl stoops to pick up part of an offering. The flexibility of the body and the emphasis on the buttocks continue into later life, and occur even in those who are unusually heavily built.

I Teboes; I Tjerita behind her.

Bajoeng Gede. April 26, 1937. 7 H 18.

6. Decorative panel on a temple wall. This figure stands as one of a series of representations of transformed witches (*lejak*) and graveyard spirits (*tangan-tangan, njapoepoe*, etc., cf. Pl. 20, fig. 5).

Poera Dalem, Bangli. Nov. 23, 1936. 3 J 5.

7. A small boy scratches his leg. He was waiting in the road, uncertain whether his playmate was following. His natural movement is to raise his leg, rather than to stoop.

Bajoeng Gede. April 19, 1937. 6 W 19.

8 and 9. Paintings of a woman transforming herself into a witch (*anak mereh*). She goes out alone at night, sets up a little shrine and makes offerings on the ground to the demons. She dances before the shrine with her left foot on a fowl, and becomes transformed into supernatural size and shape. The fantasy that the body is as integrated as a single organ is here danced out in grotesque balance, and leads to a nightmare transformation or ecstatic dissociation of the personality. The drawings illustrate the close association between grotesque posture and the ecstasy of witchcraft (cf. figs. 6 and 7).

Paintings by I. B. Nj. Tjeta of Batoean.

Purchased Feb. 2, 1938. Reduced x 1/3 linear. Cat. Nos. 545 and 548.


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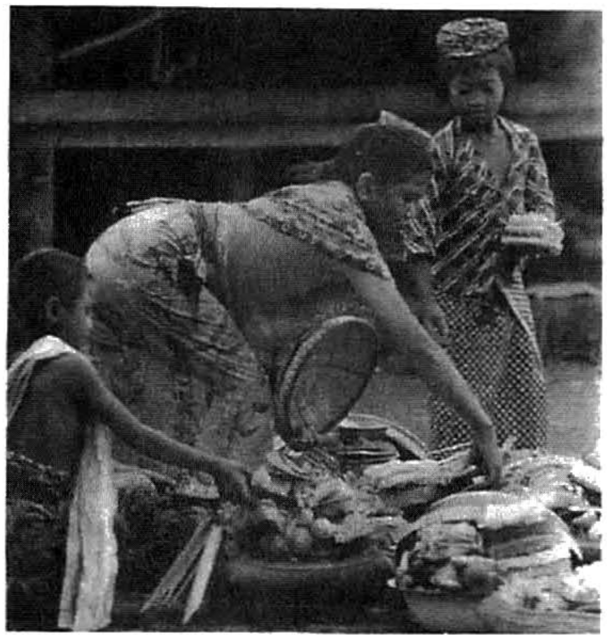
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# 29

# The Taste of Ethnographic Things

## *Paul Stoller and Cheryl Olkes*

All meats that can endure it I like rare, and I like them high, even to the point of smelling bad in many cases.[1]

Montaigne

Like other peoples in Sahelian West Africa, the Songhay take great pride in their hospitality. "A guest is God in your house," goes the Songhay adage, and so when strangers are accepted as guests in most Songhay compounds they receive the best of what their hosts can afford to offer. The host displaces his own kin from one of his houses and gives it to the guest. He removes the mattress from his bed and gives it to the guest. And then he orders the kinswoman who prepares the family meals to make her best sauces for the guest.

In 1984 Paul Stoller, an anthropologist, and Cheryl Olkes, a sociologist, traveled to Niger to conduct a study of the medicinal properties of plants used in Songhay ethnomedicine. Since both Stoller and Olkes were seasoned fieldworkers among the Songhay, they had experienced the pleasures of Songhay hospitality. And so when they came to the compound of Adamu Jenitongo, in Tillaberi, they were not surprised when Moussa, one of Adamu Jenitongo's sons, insisted that they stay in his mudbrick house. They were not surprised when Adamu Jenitongo, an old healer whom Stoller had known for fifteen years, gave them his best straw mattresses. "You will sleep well on these," he told them. They were not surprised when the old healer told Djebo, the wife of his younger son, Moru, to prepare fine sauces for them.

Stoller and Olkes had come to Tillaberi to discuss the medicinal properties of plants with Adamu Jenitongo, perhaps the most knowledgeable healer in all of western Niger. They planned to stay in Tillaberi for two weeks and then move on to Mehanna and Wanzerbé, two villages in which Stoller had won the confidence of healers. During the two weeks in Tillaberi, Stoller and Olkes ate a variety of foods and sauces. Some days they ate rice with black sauce (*hoy bi*) for lunch and rice with a tomato-based sauce flavored with red pepper and sorrel for dinner. Some days they ate rice cooked in a tomato sauce (*suruundu*) for lunch and millet paste with peanut sauce for dinner. All of these sauces contained meat, a rare ingredient in most Songhay meals. When

Paul Stoller and Cheryl Olkes, "The Taste of Ethnographic Things," pp. 15–34 from Paul Stoller, *The Taste of Ethnographic Things: The Senses in Anthropology* (Philadelphia: University of Pennsylvania Press, 1989).
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Songhay entertain Europeans – Stoller and Olkes, for example – the staples of the diet do not change, but the quality of the sauces does. Europeans are guests in Songhay compounds; people do not prepare tasteless sauces for them!

People in the neighborhood had the same perception: "They have come to visit Adamu Jenitongo again. There will be good food in the compound." In good times a host spares no expense. In bad times Stoller and Olkes quietly slipped Adamu Jenitongo money so he could fulfill his ideal behavior.

The arrival of Stoller and Olkes in Tillaberi that year, in fact, was a bright beacon that attracted swarms of the "uninvited" in search of savory sauces. At lunch and dinner time visitors would arrive and linger, knowing full well that the head of a Songhay household is obliged to feed people who happen to show up at meal times.

The "men who came to dinner" were so many that poor Djebo had to double the amount of food she normally prepared. Djebo was a mediocre cook, but the uninvited guests didn't seem to mind as they stuffed their mouths with rice, meat, and sauce.

There was one particular guest, whom everyone called *Gao Boro* (literally "the man from Gao"), who unabashedly came to breakfast, lunch, and dinner every day of Stoller and Olkes' visit. This man, a refugee (or was it a fugitive?) from Gao, in the Republic of Mali, had been living hand-to-mouth in Tillaberi for four months. He had perfected a terrific rent scam to cut his expenses. In Tillaberi, landlords will let their properties to anyone who promises to pay the rent money at the end of the month. Paying at the end of the first month is a matter of Songhay honor. At the time of our visit, *Gao Boro* was on his third house. When a landlord would come for his money, *Gao Boro* would say he was broke. The owner would throw him out, and *Gao Boro* would find another unsuspecting landlord. Stoller and Olkes soon realized the direct relationship between *Gao Boro*'s neighborliness – he lived 50 meters from Adamu Jenitongo's compound – and his ability to stretch his food budget.

Most people in the compound were reasonably happy with the food in 1984. Adamu Jenitongo's wives – Jemma and Hadjo – did complain about the toughness of the meat. So did Adamu Jenitongo. The problem, of course, was that Djebo refused to tenderize the meat – which had come from local stock – before cooking it in the sauce. Olkes suggested that Djebo marinate the meat. Djebo smiled at Olkes and ignored her advice. The toughness of the meat notwithstanding, everyone ate Djebo's sauces – until the last day of Stoller and Olkes' visit, when Djebo served bad sauce.

The last day in Tillaberi had been exhausting. Stoller had had two long sessions with Adamu Jenitongo during which they discussed the medicinal properties of plants and the Songhay philosophy of healing. Olkes had seen people in town and at the market. She had walked a good eight kilometers under the relentless Sahelian sun. At dusk, they each washed in the bath house: a three-foot-high square mudbrick enclosure equipped with a stool, a five-liter bucket, soap, and a plastic mug. Refreshed, they sat on one of their straw mattresses and waited for Djebo. Smiling, she brought them a large casserole of rice and a small one of sauce, set them at their feet, and gave them two spoons. When Stoller opened the small casserole, a sour odor overwhelmed them. Stoller saw the nightly procession of uninvited guests sauntering into the compound. Olkes wrinkled her nose.

"What is it?"

"It's *fukko hoy* [a sauce made by boiling the leaves of the fukko plant]," Stoller said.

"*Fukko hoy?*"

Stoller stirred the sauce with his spoon; it was meatless. "Shine your flashlight on the sauce, will you?" Stoller asked Olkes.

Olkes' flashlight revealed a viscous green liquid. "You can take the first taste," Olkes told Stoller.

"Wait a minute." Stoller picked up the small casserole and poured some of the *fukko hoy* over the rice. He put a spoonful of the rice and sauce into his mouth. "It's the worst damn sauce I've ever eaten," he told Olkes. "Straight *fukko hoy* seasoned with salt and nothing else!"

Olkes tasted the rice and sauce. "It's absolutely awful."

Like diplomats, Olkes and Stoller ate a little bit of the meal before pushing the casseroles away. Other people in the compound were less polite. Saying the sauce smelled and tasted like bird droppings, Moru, Djebo's husband, took his rice and sauce and dumped it in the compound garbage pit, a two-foot-deep hole about six feet in diameter that was littered with date palm pits, orange rinds, gristle, bones, and trash. "Let the goats eat this crap," he said.

Jemma, one of Adamu Jenitongo's two wives, said: "This sauce shames us. Djebo has brought great shame upon this compound." Hadjo, Adamu Jenitongo's other wife, echoed Jemma's comments. "How could anyone prepare so horrible a sauce for the guests in our compound?"

*Gao Boro*, the refugee-fugitive from Mali, arrived for his nightly "European" meal. He took one taste of the bad sauce, stood up and declared: "I refuse to eat sauce that is not fit for an animal. I'm going to Halidou's for *my* dinner tonight." From everyone's perspective, the bad sauce was in bad taste.

## The Etiology of Bad Sauce

Djebo, a young Fulan (Peul) woman, came to live in Adamu Jenitongo's compound in the summer of 1982.[2] She had formed an attachment to Moru, a drummer in the possession cult, and had spent months following him to possession ceremonies. Eventually, she moved in with him – shameless behavior for a never-married 15-year-old girl. Still considered too young for marriage (most Songhay men do not marry until they are 30) 21-year-old Moru was a musician whose earnings were erratic. When he did have money it flew from his hands, which were always open to his "friends." Moru and Djebo brought much shame to Adamu Jenitongo's compound. Although first-time brides are not expected to be virgins, they are expected to avoid shaming their families. Adamu Jenitongo could have asked Djebo to leave, but he did not. By the time Stoller arrived in December 1982, Djebo was visibly pregnant. Now, all the neighbors could see that Djebo and Moru had been living in sin. What to do? One option was abortion, a long-standing though unpopular Songhay practice. Another option was to send Djebo home to have her "fatherless" child, the usual Songhay practice. The final option was, of course, marriage. No one wanted an abortion. Moru wanted to marry his love. Adamu Jenitongo and his wives wanted the pregnant girl to return to her mother's compound.

During Stoller's visit, there were many arguments in the compound about Moru and Djebo.

"What would you do with her?" Adamu Jenitongo asked Stoller.

"You're asking me?"

"Moru should marry a Songhay woman," Adamu Jenitongo stated. "He should marry one of the girls from our home near Simiri. If he marries one of our people, everyone will be happy. Do you not agree?" he asked Stoller.

Concealing his uneasiness, Stoller said that he agreed.

Moru, who had been inside his hut, overheard the discussion between Stoller and his father and ran out to confront them.

"And what about me, Baba? Doesn't anyone ask me, Moru, about my feelings? I want Djebo. I want to marry her. I want her to have my child."

Adamu Jenitongo scoffed at Moru. "Marry her! First you bring this Fulan woman into my compound. Then you make her pregnant, and now you want to marry the worthless bitch." Adamu Jenitongo turned to Stoller. "What is this world coming to? The young people have no respect." He turned now to Moru. "You live in my household, you eat my food, you learn from me our heritage, but you have no heart and no mind. You are still a child."

Moru stormed off to his hut, fuming. Jemma, his mother, returned from the market with meat and spices. Hadjo, her co-wife, informed her of the most recent confrontation in the compound. Jemma looked at Stoller.

"Don't you think it is wrong for that worthless Fulan woman to be here? Look at her," she said loudly, pointing at the girl, who was sitting on the threshold of Moru's hut. "She's pregnant, but she's here with us. Pregnant women must live with their mothers so they give birth to healthy babies. Does that worthless Fulan do this? No! She sits here. She follows Moru to possession dances. Sometimes she walks for hours – she and the baby in her belly."

"Is this bad?" Stoller asked Jemma.

"They say that a mother who wanders with a baby in her belly will produce a monster child. That worthless Fulan is breeding a monster. I am certain of it."

"She should be with her mother," Hadjo reiterated.

During Stoller's visit there were also daily arguments between Jemma, Moru's mother, and Ramatu, Djebo's mother. On one occasion Ramatu attempted to drag her daughter back to her compound. Djebo broke her mother's grip and cursed her. Jemma cursed Djebo for cursing her mother. And Ramatu cursed Jemma for cursing her daughter. As the two older women traded ethnic slurs in Songhay, Fulan, and Hausa, a sobbing Djebo told Moru, her love, that she was walking into the bush to die. Since no one took Djebo at her word, they watched her walk toward the mountain. Ramatu returned to her compound, Jemma got back to her food preparations, and Moru went into his hut.

Two hours passed and Djebo had not returned. Moru entered Stoller's hut. "Should we go and look for her?"

"I think so, Moru."

Stoller and Moru left to search for Djebo. They returned with her two hours later. Everyone in the compound scolded the young girl.

"You are a hardheaded bitch," Jemma said.

"You are a worthless Fulan, who brings us heartache," Adamu Jenitongo said.

Djebo cried and Moru followed her into his hut.

When Stoller and Olkes returned to Adamu Jenitongo's compound in 1984, a child no more than a year and a half old waddled over to them. Laterite dust powdered her body. Mucus had caked on her upper lip.

"That's my daughter, Jamilla," Moru proclaimed.

Jamilla burst into tears when Olkes approached her.

"She's not used to white people," Jemma said.

"She's a monster child," Hadjo declared.

The term "monster child" swept Stoller back to his previous visit and the long discussions that had raged about women who wander when they are pregnant. Had the prediction come true?

"And no wonder," said Jemma, "with a mother who wandered the countryside with a child in her belly."

Moru told Stoller that he and Djebo were married shortly after his departure the previous year.

"And you didn't write?" Stoller joked.

Moru shrugged. Djebo pounded millet next to the compound's second mudbrick house, which Moru had built for his family. "Djebo," Moru called to his wife, "prepare a fine meal for them. They are tired from their trip, and we must honor them."

Adamu Jenitongo gave Djebo money and told her to go to the market and buy good spices and a good cut of meat. Djebo took the money and frowned. When she had left, Moussa (Adamu Jenitongo's other son), Jemma, and Moru complained about her. She was lazy. She was quarrelsome. They didn't trust her. She didn't know how to cook – probably because she hadn't listened to her mother long enough to learn. When she prepared meat it was so tough that even Moru couldn't chew it. The sauces were tasteless even though Adamu Jenitongo gave her money to buy the best spices. But no one had done anything to improve the domestic situation.

"Why don't you teach her how to cook?" Olkes asked.

"Hah," Jemma snorted. "She doesn't want to learn."

"Why don't you show her the right spices to buy?" Olkes persisted.

"She doesn't care. She doesn't care," Jemma answered.

Olkes felt sorry for Djebo. She was, after all, a teenager living among people who seemed set against her and who bore longstanding prejudice against her ethnic group. As the youngest affine in the compound, moreover, Djebo was expected not only to cook, but to buy food in the market, take care of her infant, fetch water from a neighborhood pump, clean pots and pans, and do the laundry. From dawn to dusk, Djebo performed these tasks as Jemma and Hadjo sat in front of their huts and criticized her.

Olkes decided to befriend Djebo. She accompanied Djebo to the pump and to the market. On market day, Olkes bought Djebo a black shawl, the current rage in Tillaberi. For whatever reason – culture, age, or personality – Djebo did not respond to these overtures. She socialized outside of the compound and did not participate in the rambling conversations of the early evening.

One day before Stoller and Olkes' departure, Djebo prepared a wonderful sauce for the noon meal. She made a locust bean sauce flavored with peanut flour. Olkes and Stoller ate with abandon. When Djebo came to their house to collect the empty casseroles, Olkes complimented her on the meal.

Stoller raised his arms skyward and said: "Praise be to God."

Saying nothing, Djebo smiled and left their house. Thirty minutes later, Djebo returned to see Stoller and Olkes – her first social visit in two weeks. Saying little, she looked over their things. She opened the lid of their non-fat dry milk and tasted some. She touched their camera, and ran her fingers over their tape recorder. Olkes and Stoller had seen this kind of behavior before. A person in Niger rarely asks for money directly; rather, he or she lingers in the donor's house and says nothing. Djebo lingered in Stoller and Olkes' house for thirty minutes and left.

"Do you understand the reason for that scrumptious meal?" Olkes asked Stoller.

Stoller nodded. "She isn't satisfied with the black shawl?"

"I guess not."

"Damn her! We can't give her money. We have to give money to Adamu Jenitongo."

"She doesn't want to follow the rules of custom, does she?"

"I just bet that she has been pocketing some of the money given to her for food," Stoller said. "That's why the sauces have been mediocre."

That night Djebo's horrible *fukko hoy* expressed sensually her anger, an anger formed from a complex of circumstances. She wanted her sauce to be disgusting.

## The Etiology of Taste

Djebo prepared a sauce to be rejected, cast away, spit out. Put another way, Djebo's sauce was the symbolic equivalent of vomit, something that our bodies reject. In the most literal sense Djebo's sauce was distasteful.

How does Djebo's sour sauce – her calculated distastefulness – fit with the conception of Taste in the Western philosophical tradition? In a word, it is different; it is non-theoretical.

One of the earliest writers on taste was Seneca. In his *Epistulae morales* he wrote that food not only nourishes our bodies, but also

> nourishes our higher nature, – we should see to it that whatever we have absorbed should not be allowed to remain unchanged, or it will be no part of us. We must digest it; otherwise it will merely enter memory and not the reasoning power. Let us loyally welcome such foods and make them our own, so that something that is one may be formed out of many elements.[3]

Seneca was among the first of the classical philosophers to write of judgment with digestive metaphors. "For Seneca, the proper digestion of received ideas both educates and is the result of an independent faculty of judgment, and this in turn is the precondition of right action."[4] These metaphors stem from the classical notion that the mouth and tongue enable us to "ingest" the outside world. Physical tasting is extended to mental tasting, the classical notion of judgment.[5]

In his *Critique of Judgment*, Kant rejects the classical notion that the faculty of taste can be extended to social, political, or scientific matters. In fact, he removes taste entirely from the domain of science, preferring to consider it a purely aesthetic sense.

> In order to distinguish whether anything is beautiful or not, we refer the representation, not by the understanding to the object for cognition, but by imagination (perhaps in conjunction with the understanding) to the subject and its feeling of pleasure and pain. The judgment of taste is therefore not a judgment of cognition, and is consequently not logical but aesthetical, by which we understand that those determining grounds can be *no other than subjective*.[6]

Kant's passage suggests that the faculty of taste should be restricted to the apprehension of objects of beauty. Following the publication of the *Critique of Judgment* in 1790, taste was no longer considered an appropriate concept in the classically approved domains of politics, society and science – domains that were restricted to the logical, objective, and scientific reflection of the Enlightenment.

## *The etymology of taste in English*

Raymond Williams writes that the word taste came into the English language around the thirteenth century, but that its earliest meaning was closer to "touch" or "feel."[7] "Taste" comes to us from the Old French *taster*, and from the Italian *tastare*, which translates to "feel, handle, or touch." "Good taast" in the sense of good understanding was recorded in 1425.[8] But the metaphoric extensions of the word became confused in the latter part of the seventeenth century and the eighteenth century, when it was associated with general rules. In English, then, the sensual aspects of taste were gradually replaced by the more general and rule-governed notion. Perhaps due to the Kantian influence, the meanings of *taste* and *good taste* are even today far removed from their sensual attributes. Djebo's sense of taste is sensual and subjective; Kant's sense of Taste is rarefied and objective.

## *The sensual tastes of Montaigne*

Djebo's non-theoretical sense of taste is similar to Montaigne's. The final section of his *Essais*, entitled "Of Experience," is a compendium of Montaigne's physical tastes: what he likes to eat, how often he likes to eat, how much he likes to eat. In this final book, Montaigne discusses his sleeping habits, his kidney stones, his medicines, his squeamishness, his hatred of sweets as a child and his love of sweets as an adult, his digestion, his indigestion, and even his bowel movements. On the subject of bowels, Montaigne also writes that "both Kings and philosophers defecate, and ladies too. . . . Wherefore I will say this about that action: that we should relegate it to certain prescribed nocturnal hours, and force and subject ourselves to them by habit, as I have done."[9] Montaigne's "father hated all kinds of sauces; I love them all. Eating too much bothers me; but I have as yet no really certain knowledge that any kind of food intrinsically disagrees with me."[10] Alas, Montaigne never ate Djebo's *fukko hoy*.

## *Derrida's dregs*

Montaigne's sensuality has had a minimal influence on Western thought, however. More prevalent today are the rarefied Enlightenment metaphors of composition and construction. In Hegel's constructive system, for example, "the material of ideality is light and sound. Voice, in the relation to hearing (the most sublime sense), animates sound, permitting the passage from more sensible existence to the representational existence of the concept."[11] Sight and hearing are theoretical senses that represent the attempt of the Enlightenment philosophers to create from the chaos of appearances constructed systems of "reality," wherein one might Taste the Truth.

In sharp contrast to historical and modern masters of philosophy, Derrida stands for sensuality as opposed to rarefaction, for deconstructionism as opposed to constructionism, for decomposition as opposed to Taste. In *Of Grammatology* and in *Glas*, Derrida indicates a philosophical system based upon such non-theoretical senses as taste (also smell and touch) which depend upon a part of the body, the tongue, which is primary in speech production:

> The dividing membrane which is called the soft palate, fixed by its upper edge to the border of the roof, *floats* freely, at its lower end, above the base of the tongue. Its two lateral sides (it is a quadrilateral) are called "pillars." In the middle of the floating end, at the entrance to the throat, hangs the fleshy appendage of the uvula, like a small grape. The text is spit out. It is like a discourse in which the unities model themselves after an excrement, a secretion. And because it has to do here with a glottic gesture, the tongue working on itself, *saliva* is the element which sticks the unities together.[12]

As Ulmer suggests, Derrida's texts condemn Hegel's assertion that odor and taste "are useless for artistic pleasure, given that esthetic contemplation requires objectivity without reference to desire or will, whereas 'things present themselves to smell only to the degree in which they are constituted by a process, in which they dissolve into the air with practical effects.'"[13] For Derrida there should be no separation of the intelligible from the sensible. Since Kant, he argues, Taste has been an objective, rarefied distancing from an object of art. Using the sensual Montaigne as one of his models, Derrida opposes *gustus* with disgust and taste with distaste. The key concept of Derrida's writing on taste is *le vomi*, "which explicitly engages not the 'objective' senses of hearing and sight, nor even touch, which Kant describes as 'mechanical,' all three of which involve perception of or at surfaces, but the 'subjective' or 'chemical' senses of taste and smell."[14] For Derrida, then, Djebo's *fukko hoy* should not only be spit out into an ethnographic text, but should be done so with sensual vividness, for Djebo's bad sauce is gloriously disgusting; it reeks with meaning.

## Taste in anthropology

Beyond the sensual descriptions in anthropological cookbooks, most anthropologists have followed Hegel's lead in separating the intelligible from the sensible. This Hegelian tendency is evident from even a cursory examination of ethnographic writing. Like most writers, most ethnographers tacitly conform to a set of conventions that colleagues use to judge a work. Marcus and Cushman have suggested that conventions governing ethnographic representation devolve from realism. They argue that realist ethnographic discourse seeks the reality of the whole of a given society, and that "realist ethnographies are written to allude to the whole by means of parts or foci of analytical attention which constantly evoke a social and cultural totality."[15] In an article in *L'Homme*, Stoller describes the philosophical development of realism in ethnography.[16] That development eventually resulted in a set of conventions that Marcus and Cushman have analyzed:

1 a narrative structure which devolves from cultural, functionalist, or structuralist analytical categories to achieve a total ethnography;
2 a third person narrative voice which distinguishes realist ethnographies from travel accounts;
3 a manner of presentation in which individuals among the people studied remain nameless, characterless;
4 a section of text, usually a Preface or Afterword, which describes the context of investigation;
5 a focus on everyday life contexts representing the Other's reality to justify the fit of the analytical framework to the ethnographic situation;
6 an assertion that the ethnography represents the native's point of view;
7 a generalizing style in which events are rarely described idiosyncratically, but as typical manifestations of marriage, kinship, ritual, etc.;
8 a use of jargon which signals that the text is, indeed, an ethnography as opposed to a travel account;
9 a reticence by authors to discuss their competence in the Other's language.[17]

While most ethnographers religiously followed these conventions of realist representation in the past, there are a growing number of scholars who are worried about the epistemological and political ramifications of ethnographic realism. Directly and indirectly, their ethnographic and theoretical writings reflect these philosophic issues.[18] Fabian writes of his concern about anthropology's intellectual imperialism: "Perhaps I failed to make it clear that I wanted language and communication to be understood as a kind of praxis in which the Knower cannot claim ascendency over the Known (nor, for that matter, one Knower over another). As I see it now, the anthropologist and his interlocuters only 'know' when they meet each other in one and the same contemporality."[19]

Although the new "experimental" works have been provocative, most of them consider typical anthropological subjects of study, albeit through partially altered conventions of representation. How could it be otherwise, when disciplinary constraints force most writers to concentrate on certain kinds of subjects: the theory of Taste instead of the taste of bad sauce, the theory of the family instead of texts that familiarize the reader with family members, the theory of experimental ethnography instead of experimental ethnographies. How can it be otherwise, when disciplinary constraints impose form and order on what is published. Take, for example, the *Abstracts of the Annual [Anthropology] Meetings:*

> Name, institution, and title of paper or film must precede narrative portion: Put last name first; use capital letters for author's last name and title of paper; do not include 'university' or 'college' with institution name given in parentheses. . . . Write the text in complete sentences. Use the present tense; use only third person.[20]

In addition, the American Anthropological Association gives prospective participants some useful tips on writing a "good abstract":

> A "good" abstract should be an informative summary of a longer work. It should state the central topic at the beginning; it should clearly indicate the nature and extent of the data on which it is based; it should outline the nature of the problem or issue and delineate the relevant scientific argument; and it should show how the content relates to the existing literature. Where helpful, citations may be used. The abstract must be typed *double spaced*, and it must fit *within* the box provided below.[21]

This prescription may be a fine model for terse scientific writing, but it discourages unconventionality in ethnographic writing with the message it sends to potential Annual Meeting participants: "We are a scientific organization. We sponsor scientific papers in our scientific program." Good or "beautiful" abstracts, in the sense of Kant and Hegel, are written in the present tense (the ethnographic present?) and in the third person (a marker of objectivity?) Even today, Hegel's *Esthetics* casts a long shadow over anthropological representation.

Despite the difficulties precipitated by a long entrenched philosophical tradition, it is altogether certain that the pioneering and courageous efforts of contemporary ethnographers have forced anthropologists to ponder the nature of both their scholarship and their being. But do these writers take us far enough? Are there other dimensions of ethnographic discourse, other conventions of representation which may carry anthropology deeper into the being of the others? Are there other modes of representation that better solve the fundamental problems of realist ethnographic representation: voice, authority, and authenticity?

## Tasteful Ethnography

How does a piece of ethnographic writing get published? Here the digestive metaphors are particularly relevant. An author submits her or his manuscript to a publisher or to a journal. Editors ingest the manuscript. If the material falls within the conventions of representation of a discipline, the editors are likely to digest what they have taken in and the manuscript will eventually be published. If the material violates those conventions, the editors may well find the piece hard to swallow and the manuscript is returned to the author, a case of Derrida's *vomi.* Indeed, when editors write comments to authors of rejected (vomited) manuscripts, they often suggest how the author might transform his or her piece from disgusting vomit into digestible food for thought. Examples of these comments from Stoller's files illustrate how readers and editors reinforce conventional anthropological tastes.

> *Example 1* [Letter from acquisitions editor to Stoller]. I have just received two reviews of your manuscript, and I'm sorry to have to tell you that these have not been sufficiently encouraging for me to feel able to offer to consider the work further. Both reviewers thought that the script contained some interesting data, but felt that the theoretical argument was insufficiently well developed.

> *Example 2* [Comments from Stoller to anonymous author]. The author of this article suggests that anthropologists consider music more seriously, less tangentially, in their analyses of sociocultural systems. Merriman made more or less the same statement in his pioneering *Anthropology of Music* (1964), which the author unfortunately does not cite. . . . The author leads one to believe that we should consider the sociocultural aspects of music seriously. I fully agree. After reading the piece, however, I feel the author has fallen into the trap he/she says other anthropologists have fallen into. Music is not the central concern of this article; it is of secondary or perhaps tertiary importance when compared with the author's overriding concern with subsistence and the materialist perspective. . . . In short the author fails to highlight the importance of music in the cultural scheme of things. . . .

*Example 3* [Comments of Stoller to anonymous author]. In this piece the reader is treated to a plethora of excellent ethnography in which the author develops the sociological context of x's name change. But the author does not blend this rich material with other studies in Africa or elsewhere which are on similar topics.... What kind of contribution does this piece make to ethnological theory, method, compared to other works on the topic?...

*Example 4* [Reader's comments to Stoller]. I sympathize with the author's desire to go beyond the limits of positivism and enter into the mental set of the people he studies, though he might take note that this is the point of departure espoused by such diverse scholars as Boas, Malinowski, and Radin, among others. My objection to the work is not in his effort to seek an inside viewpoint, but in his failure to demonstrate its value, and, above all, in his failure to meet the canons of academic evidence. One must presume that the young man in his narrative was himself – but his unwillingness to communicate in the scientific mode and to adhere to the Songhay rules, deprives us of direct evidence for this insight and makes us wonder at the source and character of his information.

*Example 5* [Reader's comments to Stoller]. This is basically a suitable article for the ..., since it has an interesting and significant point to make concerning the need to recognize the importance of sound in many societies. I feel, however, that many readers would not be gripped enough at the beginning of the article [a narrative with dialogue]...to see it through to the end.... My personal preference is for less humanistic and subjective language.

*Example 6* [Reader's comments on Stoller's manuscript].... There is no question that the subject matter is important and underrepresented in the literature, or that the author has some very valuable field data in hand. It would be quite useful to have a good study of Songhay religion and culture.... While there is some interesting description of possession rituals, and of Songhay religion and history, if I have to judge it frankly I must say that at this point it is a half-baked manuscript.

The weakness of its theoretical grounding leads to the lack of any real integration of the descriptive material beyond the repeated (and ultimately somewhat boring) assertion that the cults are forms of cultural resistance.... I think there are two central points of weakness, which the author glides over at the beginning where he casually dismisses psychological and functional accounts: he shows no evidence of having read the work of Victor Turner ... and he shows no evidence of familiarity with the *recent* studies of the psychophysiology of trance which have made such rapid advances in our understanding of these phenomena....

And so it goes in the modern era of anthropology, an era that in many ways is past its shelflife.[22] One way of freeing ourselves from the constraints of Taste in Anthropology is to engage fully in a tasteful ethnography. Freed from the social, political, and epistemological constraints of realism, a tasteful ethnography would take us beyond the mind's eye and into the domain of the senses of smell and taste. Such an excursion into sensuality would complement the rarefied Hegelian senses of sight and sound.

## Tasteful fieldwork

In tasteful fieldwork, anthropologists would not only investigate kinship, exchange, and symbolism, but also describe with literary vividness the smells, tastes, and textures of the land, the people, and the food. Rather than looking for deep-seated hidden truths, the tasteful fieldworker understands, following Foucault, "that the deep hidden meaning, the unreachable heights of truth, the murky interiors of consciousness are all shams."[23] From the sensual tasteful vantage, the fieldworker investigates the life stories of individual Songhay, Nuer, or Trobrianders as opposed to totalized investigations of the Songhay, the Nuer, or the Trobriander. This recording of the complexities of the individual's social experience lends texture to the landscape of the fieldworker's notes. In this way, seemingly insignificant incidents as being served bad sauce become as important as sitting with a nameless informant and recording genealogies – data – that eventually become components in a system of kinship. In this way ethnographic research creates voice, authority, and an aura of authenticity.

## *Tasteful writing*

There are probably many anthropologists who do engage in tasteful fieldwork. Despite their scientific objectives, they become sensually immersed in their field surroundings. These impressions, however, are usually cast aside – becoming vomit – in their published theoretical and ethnographic writings. Like Djebo's bad sauce, conventions of representation governing genre selection could be thrown into a trash pit.

Acknowledging the diverse collection of refuse, the tasteful writer uses the notion of melange as his or her guiding metaphor for producing tasteful ethnographic writing.

In Derrida's *Glas*, the writing on the pages is arranged in two columns. In the left-hand column is prose representing Hegel (rarefaction, Taste, the Enlightenment and its theoretical senses). In the right-hand column, by contrast, is prose representing Genet (sensuality, taste, post-modernism and its non-theoretical senses). Within this revolutionary stylistics is a powerful indirect challenge to the fundamental metaphors of the Western philosophic tradition. Derrida's *Glas* is in bad Taste. But Derrida's bad Taste – his vomit – provides a point of reference for tasteful ethnographic writing that incorporates the non-theoretical senses.

Consider first an example from James Agee's *Let Us Now Praise Famous Men*, in which he describes the odors of a tenant farm house in Alabama.

> These are its ingredients. The odor of pine lumber, wide thin cards of it, heated in the sun, in no way doubled or insulated, in closed and darkened air. The odor of woodsmoke, the fuel being again mainly pine, but in part also, hickory, oak and cedar. The odors of cooking. Among these, most strongly, the odors of fried salt pork and of fried and boiled pork lard, and second, the odor of cooked corn. The odors of sweat in many stages of age and freshness, this sweat being a distillation of pork, lard, corn, woodsmoke, pine and ammonia. The odors of sleep, of bedding and of breathing, for the ventilation is poor. The odors of all the dirt that in the course of time can accumulate in a quilt and mattress. Odors of staleness from clothes hung or stored away, not washed. I should further describe the odor of corn: in sweat, or on the teeth and the breath, when it is eaten as much as they eat it, it is of a particular sweet stuffy fetor, to which the nearest parallel is the odor of the yellow excrement of a baby. . . .[24]

Consider next an example from John Chernoff's *African Rhythm and African Sensibility*, in which he describes how music and African social life interpenetrate.

> At the beginning of each year the *harmattan* winds blow a fine dust from the Sahara Desert across the Sudan and over the coastal areas of the Gulf of Guinea. In Bamako, capital of Mali, you might observe the evening traffic as if through a reddish brown filter which softens and mutes the sights and sounds of the crowded streets. The atmosphere is tranquil, and standing on the long bridge over the Niger River, with cars passing just a few feet behind you, you might look at a lone fisherman in his graceful canoe and feel that only the lovely melodies of the harp-like *kora* could capture and convey the unity of the scene. At night the temperature drops until you might wonder why you ever thought you missed winter, and if by chance you found yourself in an isolated village at the right time and you looked up at the multitude of stars, you might hear the music of xylophones through the crisp air and believe that the clarity of the music was perhaps more than superficially appropriate to the stillness of the night.[25]

Consider finally an example from Lévi-Strauss's *Tristes tropiques*, "which, though it is very far from being a great anthropology book, or even an especially good one, is surely one of the finest books ever written by an anthropologist."[26] The example is an exegesis on the South American equivalent of bad sauce.

> There had been no rain for five months and all the game had vanished. We were lucky if we managed to shoot an emaciated parrot or capture a large *tupinambis* lizard to boil in our rice, or managed to roast in their shells a land tortoise or an armadillo with black, oily flesh. More often than not, we had to be content with *xarque*, the same dried meat prepared months previously by a butcher in Cuiaba and the thick worm-infested layers of

which we unrolled every morning in the sun, in order to make them less noxious, although they were usually in the same state the next day. Once, however, someone killed a wild pig; its lightly cooked flesh seemed to us more intoxicating than wine: each of us devoured more than a pound of it, and at that moment I understood the alleged gluttony of savages, which is mentioned by so many travellers as proof of their uncouthness. One only had to share their diet to experience similar pangs of hunger; to eat one's fill in such circumstances produces not merely a feeling of repletion bur a positive sensation of bliss.[27]

These examples are only a slice of the life that lives in the tasteful ethnographies of Agee, Chernoff, and Lévi-Strauss. In all the examples, the writers season their prose with the non-theoretical senses to evoke a world. Agee masterfully uses a melange of smells to evoke the habitus of southern tenant farmers – their fatty diet, their filthy clothes, their stuffy houses, their abject misery. In one smelly paragraph we have a memorable portrait of the lives of these people. Chernoff records the interpenetration of sound and sight in African social life. This paragraph evokes an African world in which "participatory" music gives shape ro a people's system of values as well as to their manner of living-in-the-world. With Lévi-Strauss we come back to the sensual notion of taste. In one vivid paragraph he ruminates on the link between deprivation of diet and gluttony in the Amazon. Even European intellectuals can descend into gluttony!

Should this kind of writing be excised from the ethnographic manuscripts of the future? Aren't expositions on odors, sounds, and tastes extraneous to the ethnographic message? What can these details reveal about a sociocultural system? In terms of systematic analysis, these kinds of evocative details do not uncover a system of kinship or exchange or symbolism; hence Geertz's critique of *Tristes tropiques* as not even a good anthropology book. Tasteful anthropology books are analytic, theoretical, and ephemeral; tasteful ethnographies are descriptive, non-theoretical, and memorable. Writers of tasteful ethnographies mix an assortment of ingredients – dialogue, description, metaphor, metonomy, synecdoche, irony, smells, sights, and sounds – to create a narrative that savors the world of the Other. And just as Chernoff's drumming in Ghana once inspired members of an audience to say: "'Oh, the way you played! It moved me. It was sweet,'" so a well constructed narrative moves the listener or the reader to say: "Can I tell you a terrific story?" Indeed, there is life in the words of a good story; there is life in the prose of a tasteful ethnography.

In his monumental essay *L'Oeil et l'esprit*, Merleau-Ponty states that we lose much of the substance of life-in-the-world by thinking operationally, by defining rather than experiencing the reality of things.

> Science manipulates things and gives up living in them. It makes its own limited models of things; operating upon these indices or variables to effect whatever transformations are permitted by their definition; it comes face to face with the real world only at rare intervals. Science is and always has been thar admirably active, ingenious, and bold way of thinking whose fundamental bias is to treat everything as though it were an object-in-general – as though it meant nothing ro us and yet was predestined for our own use.[28]

An ethnographic discourse that "comes face to face with the real world only at rare intervals" is usually so turgid that it is digestible by only a few dedicated specialists – a discourse that will soon be forgotten. A tasteful ethnographic discourse that takes the notion of melange as its foundation would encourage writers to blend the ingredients of a world so that bad sauces might be transformed into delicious prose.

## One Month of Bad Sauce among the Songhay

Stoller returned to Songhay and Adamu Jenitongo's compound in June of 1987. Jamilla had died in 1985, having drowned in a garbage pit filled with water after a torrential rain. Soon thereafter Djebo was pregnant with Hamadu, who in 1987 was about 18 months old.

In 1984 Moussa, Adamu Jenitongo's elder son, worked as a tailor; his atelier was in his father's compound. But business was slack because the Jenitongo compound was far from the center of Tillaberi. In 1985 he found a suitable atelier in Baghdad, a bustling section of Tillaberi. Moussa would leave his father's compound early in the morning, return for lunch, and leave again to complete the afternoon tasks at his tailor shop. Since he soon had more work than he could handle himself, he hired an apprentice.

By the time Stoller arrived in June of 1987, Moussa was spending much of his time at his tailor shop. He took his lunches there; for dinner, he sometimes ate small meals at the Baghdad bars: steak and french fries, green beans, omelettes. After Stoller's arrival, to fulfill the requirements of Songhay hospitality, Moussa made sure to return home to eat his meals.

The quality of the sauces hadn't changed. Djebo did not serve *fukko hoy*, but on occasion she refused to prepare meals, forcing Moussa, Stoller, and Adamu Jenitongo to eat meals of bread and sardines in soy oil.

"Things are better now that you have come," Moussa told Stoller. "Weeks go by and she doesn't prepare meals. Baba is old; he needs to eat better, but she doesn't care. My brother Moru doesn't care. And Baba, he chews kola and tobacco."

"Bad sauces are better than no sauces," Stoller said.

After one week of sauces the quality of which ranged from mediocre to bad, Stoller suffered a violent case of diarrhea. He quickly lost weight. Moussa suggested an alternative.

"Let's eat our lunches with Madame. She is an excellent cook." Madame was the daughter of Adamu Jenitongo's sister, Kedibo.

Moussa and Stoller began to eat lunch at Madame's house. The sauces were tasty: fine gombo, sesame, and squash sauces all of which were spiced delicately with permutations of garlic, ginger, locust bean, and hot pepper. Moussa and Stoller stuffed themselves, knowing that the evening fare would be much worse: tasteless rice paste drowned with watery tomato sauces all of which were spiced without imagination. There was millet in the compound, but Djebo refused to prepare it.

When it became apparent that Stoller was taking his meals at Madame's house, Djebo protested. Jemma, Djebo's mother-in-law, scowled.

"Why do you insist on your European sauces?" Jemma asked him. "Why don't you eat the sauces we prepare for you?"

Stoller did not respond directly; rather, he forced himself to eat two meals at lunch and one at dinner. Even with this increased consumption, Stoller lost more weight. His diarrhea continued.

Shamed by the bad sauces in his concession, Moussa confronted his younger brother Moru before an audience of visitors to the compound.

"How can anyone live in this compound with your lazy wife, who, when she lowers herself to prepare food for us, produces sauces that our chickens won't eat."

"Now hold on, older brother. How can you . . ."

"Shut up, you ignorant peasant. I feel like a stranger in my own home. Why return to a place where I'm not wanted?"

Moru wagged his forefinger at Moussa. "You donkey. Worthless person. Come closer and insult my wife. I'll tear your eyes out. A man who doesn't even have a wife deserves to eat shit."

"Better to be single than to be a slave to a bitch," Moussa retorted. "I'll eat my sauces elsewhere."

Moru's wife and mother restrained him.

Stoller restrained Moussa.

Adamu Jenitongo called for peace. "We shame ourselves in front of strangers."

Moru and Moussa are half-brothers. In the Songhay language they are *bab'izey*, which has two translations: "half-brothers" and "rivals." In Songhay *bab'izey* frequently have relationships the major ingredient of which is jealousy and bad feelings built up over a lifetime. This problem has poisoned the relationships between Moru and Moussa. As men, they have very different kinds of temperaments. Moru is hot-headed and prone to verbal and even physical confrontation. Moussa is even-tempered and keeps his emotions more to himself. Moru is a musician who sometimes works as a laborer. Moussa is a tailor who works steadily. To add more salt to an open wound, both

Moussa and Moru covet the powerful secrets of their father, one of the most powerful sorcerers (*sohanci*) in Niger.

In his old age Sohanci Adamu Jenitongo hinted that he would pass on his secrets to both Moussa and Moru. But only one of them would receive his chain of power and his sacred rings. To Moru, Jemma, and Djebo, the alliance of the younger son, it was painfully obvious that Adamu Jenitongo had chosen his eldest son Moussa to succeed him. Moussa had the relatively calm disposition required for receiving great power. Moussa also had powerful allies: Kedibo, Adamu Jenitongo's youngest sister, favored Moussa because Moussa's mother, Hadjo, was the sister of her late husband. Each time she visited her brother, Kedibo extolled the virtues of her "nephew."

Moussa's steady disposition and his strategic position in the family kinship network made Moru's situation hopeless. Being powerless to change the course of events, Moru, Djebo, and Jemma chose to make life miserable for Moussa, his mother Hadjo, and Adamu Jenitongo. As the recipient of power, Moussa would soon reap considerable social rewards. He would soon become the sohanci of Tillaberi; people would fear and respect him. Moru wanted the fear and respect that his older brother was soon to receive. Powerless, Moru, Jemma, and Djebo used sauce to express their frustrations. Moussa must eat bad sauces and suffer in exchange for his good fortune.

Sauce had again become the major ingredient in the stew of (Songhay) social relations, something which Montaigne had realized long before Djebo had produced her first (though certainly not her last) bowl of *fukko hoy*.

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# Dialogic Editing: Interpreting How Kaluli Read *Sound and Sentiment*

*Steven Feld*

The word in language is half someone else's.

– Mikhail Bakhtin

When the writer becomes the center of his attention, he becomes a *nudnik*. And a *nudnik* who believes he's profound is even worse than just a plain *nudnik*.

– Isaac Bashevis Singer

An engaging dimension of current interpretive ethnography and its critical rhetoric is the concern to situate knowledge, power, authority, and representation in terms of the social construction of literary realism. Ethnographers today are reading, writing, and thinking more about the politics of ethnographic writing.[1] That is why I read Bakhtin; in his literary world, a dialogic imagination helps reposition ethnographic writing beyond its overt trajectories, and toward reflexive, critical readings. Yet I've had a tendency to *kvetch* about the very literary genre and trend that I'm here to contribute to. I like the emphasis on a self-conscious, dialectical invention of culture, but I worry that the enterprise not devolve into an invention of the cult of the author. First-person narrative may be the fashionable way to write and critique ethnography these days, but that alone doesn't guarantee that the work is ethnographically insightful, self-conscious, or revelatory. That is why, in tandem, I read Singer; in his literary world, there is caution that first-person writing not pass as a ruse, that hermeneutic not pass for a mispronunciation of a *nom de plume:* Herman Nudnik.

## A Context

This article opens to a fixed-in-print text to look at how a new set of readers – its original subjects – opened up and unfixed some of its meanings and repositioned its author's authority. I am the author, and the text is *Sound and Sentiment: Birds, Weeping, Poetics, and Song in Kaluli Expression* (1982), an ethnography about the Kaluli people of Bosavi, Papua New Guinea. While I have been stimulated in this endeavor by previous "afterword" essays in Papua New Guinea ethnographies – Bateson's for *Naven* (1958), Rappaport's for *Pigs for the Ancestors* (1984) – what follows was more directly inspired by a series of significant field

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experiences that positioned Kaluli and me in a more blatant subject-to-subject relationship.

In 1982 I returned to Papua New Guinea for a short summer field trip after an absence of five years. While I was back in Bosavi, my book was published; its arrival in the field, and my momentary fixation on it stimulated the Kaluli to ask about it, and stimulated me to attempt translating sections of it for discussion with them. This article reports on the form of ethnographic discourse that developed in these encounters.

The "dialogic" dimension here implicates what Kaluli and I say to, about, with, and through each other; with developing a juxtaposition of Kaluli voices and my own.[2] My focus on "editing" invokes a concern with authoritative representation; the power to control which voices talk when, how much, in what order, in what language. "dialogic editing," then, is the impact of Kaluli voices on what I tell you about them in my voice; how their take on my take on them requires reframing and refocusing my account. This is the inevitable politics of writing culture, of producing selections and passing them off as authentic and genuine, and then confronting a recentered view of that selection process that both questions and comments upon the original frame and focus. In more direct terms, my aim here is to let some Kaluli voices get a few words in edgewise amongst my other readers and book reviewers.

My secondary title, "Interpreting How Kaluli Read *Sound and Sentiment*" is meant to implicate the work Kaluli helped me do in order to "write" them, and the work I had to do for them to "read" that writing. I want to suggest that this understanding is multiply textual, that Kaluli perceive the coherences and contradictions in my representational work as being about me in similar ways to how I perceive the book to be written about them. I also want to suggest Kaluli perceive it as a story about themselves that they also have occasion to tell, a line I'll use to play off of Geertz's phrase situating culture as "a story they tell themselves about themselves" (1973:448). But Kaluli tellings are different from mine in arrangement, focus, intention, and style. I'd also suggest that my Kaluli readers realize as clearly as I do that all of our tellings elide and/or condense certain scenarios while playing out others in detail; and that both kinds of tellings and tellers have a complicated cross-understanding of the way they speak and write with an acute awareness of different audiences.

## A Text

*Sound and Sentiment* is an ethnography of sound as a cultural system, a book about natural and human sounds – birds, weeping, poetics, and song – and how they are meaningfully situated in the ethos, or emotional tone, of Kaluli expression. The form of the book originates with Kaluli ideas as they are packed into a myth about the origin of weeping, poetics, and song in the plaintive sound of a fruitdove, the *muni* bird. I present and unpack that myth, following its structure, with chapters on birds, weeping, poetics, and song that alternate structural and cognitive summaries with symbolic/performance case studies. In this fashion, the book continually moves back and forth between Kaluli idealizations, prescriptions, intentions, and actualizations, and these are played off each other by my juxtapositions of linguistic (from metalanguage to texts), musical (from form to performance), and cultural (from ideation to action) analyses.

As to what is "in" the book: Kaluli myths and cosmology portray birds and humans as transformations of each other in death and life, living in different planes of visible and nonvisible reality that in part "show through" to each other. Birds can "show through" by their sounds; Kaluli apprehend and relate bird sound categories to spirit attributions according to which ones "whistle," "say their names," "talk Kaluli," "cry," "sing," or "make a lot of noise." The explicit link between bird sound and human emotional expression is first formed in the arena of weeping. The descending four tones of the *muni* bird call create a melodic framework through which women's funerary wailing turns into wept song.

While the performance of this sung-texted-weeping evokes the image that, like the deceased, the weeper too has "become a bird," the switch from spontaneous to elaborately planned ritual expression hinges on poetics. Transforming the "hard words" of assertive discourse to the "bird sound words" of poetic song involves evocative linguistic strategies to speak "inside" the words, and to "turn them over" so they reveal new "underneaths." Song texts are organized by these devices to follow a "path" along a set of place-names; these evoke the pathos of experiences Kaluli share together at the places that they travel to or visit each day.

These poetic "bird sound words" are then melded with the musical material of bird sound, a melodic song scale again based on the tones of the *muni* bird call. A polysemous lexicon of water motion names the contours of song melody and creates a theoretical vocabulary for compositional and aesthetic discourse on song. To be deeply affective and to move members of an audience to tears, these "flowing" songs are then performed in a plaintive bird voice by a dancer costumed as a bird at a waterfall.

While these are some of the features detailed in the book, it is probably more to the point to say that my "topic" was the aesthetics of Kaluli emotion, or, put differently, the invention of sound as aesthetically organized sentiment. My work in *Sound and Sentiment* was to demonstrate how sound is constructed and interpreted as the embodiment of feelings; that is, as aesthetically affecting evocation in the Kaluli ritual performance of weeping and poetic song.

## Readers and Readings

Let me now introduce some of my Kaluli readers and say something about how they read and about how they are positioned in relation to my ethnographic work. Virtually all Kaluli have seen books of some kind. (E. L.) Buck and Bambi Schieffelin (the researchers with whom I have worked in Bosavi over the last ten years) and I have always had a variety of books around the house, and Kaluli have had ample opportunities to watch us read. We also on occasion have read out loud to Kaluli and have shown them pictures and illustrations in books and magazines. Many Kaluli have gazed silently as we sat silently turning pages. A typical late afternoon interchange among Kaluli standing on our porches and looking in the windows might go like this: "What are they doing?" (interrupted by) "Nothing... they're looking at books." Indeed, the word "book," which has the same phonological shape in English and *tok pisin*, the pidgin/creole lingua franca of urban Papua New Guinea (where it is spelled *buk*) is one of the few *tok pisin* words universally placed in the Kaluli loan-word lexicon at this juncture.

Other varieties of familiarity with books have developed through contact Kaluli have had with missionaries who have been in residence since 1971. The mission people have done a small amount of literacy work, and a local school now run by the provincial government exists at the mission station. There are books in the local school, and all Kaluli know that the missionaries have a master book of their beliefs, the Bible, that they intend to translate into the Kaluli language. Books are also not entirely the domain of whites: Kaluli have also been read to by other Papua New Guineans, for example, in church by Kaluli and other pastors, at the school by teachers, or in other situations by government workers.

Kaluli have also watched the Schieffelins and me type and write by hand, and they have asked why we do it. As an explanation we have all probably told them at one point or another that we write so that we can remember what they tell us, and make books about it that our own people can read. Moreover, writing and the handling of books is an activity clearly central to work we and Kaluli do together. When a missionary linguist went to Bosavi in 1964 to supervise the building of a local airstrip and to take a first crack at the language, he introduced the word "school" as the name for the activity that local people would do with him to teach their language. The word was turned into a verb, *sugul-a:la:ma*, "do school," and "doing school" is generically what Kaluli do when they work with us.[3] Part of school work is watching us *dogo:fwanalo*, "yellow skins," as Kaluli call us, write. One day Ayasilo: was helping Bambi Schieffelin and me recruit a new transcription assistant. "Doing school is very hard and goes slow," Ayasilo: explained to young Igale; "when you speak they write it and rub it out and keep writing it again." (The Kaluli verb for "etch" was semantically extended long ago to cover the activity of "writing.")

Young men like Ayasilo: and Igale are not typical Kaluli. Both have a Papua New Guinea school education, are among the five or six Kaluli we know who speak English with moderate conversational skill, and can read and write to a very modest extent in Kaluli, *tok pisin*, and English. Men of this sort have worked often with Bambi Schieffelin and me as linguistic transcription assistants. They have sophisticated senses of their own linguistic and cultural identities as well as substantial contact with outsiders and other Papua New Guineans.

Ayasilo: and Ho:nowo: were two young men in this category who read *Sound and Sentiment* with me at informal and formal school sessions. On occasion these men actually did read out loud directly from the book – passages that I selected for them, knowing that there would be relatively few difficult words or places with lists or diagrams full of Kaluli words. With these men I speak in a continual mixture of Kaluli and English.

At the other end of the spectrum of my readers were older Kaluli men. They typically have had no prolonged experience of the world beyond their immediate neighbors. What they know of the outside is what of it has been brought or narrated by younger Kaluli. With them I speak only in Kaluli. These men included Jubi, a Kaluli man of wisdom and knowledge in various realms of things traditional. Jubi was a particularly astute natural historian, and he lived through the whole sweep of contact experiences from the mid-1930s until the present. Jubi was someone with whom Buck Schieffelin and I had done substantial schooling during each of our previous field trips.

Somewhere in between these two poles were two other people. Gigio was in the same age range as the young men mentioned earlier, but he has never been to school. He is a nonliterate Kaluli speaker with several experiences in the 1970s on labor contracts in the Papua New Guinea world outside Bosavi, and thus has had substantial contacts with Europeans and with other Papua New Guineans with whom he can communicate in *tok pisin*.

As a young boy, Gigio started working as a cook for Buck Schieffelin in 1996, and has held a continually expanding version of that position during each of our field trips over the last 20 years. Gigio is in many ways our closest confidant, friend, and barometer of everyday meanings and events in Bosavi. He is someone we talk to each day about everything from the weather and local garden crops to the poetics of ceremonial songs and linguistic nuances of Kaluli speech. He is enormously intelligent, curious, and perhaps the most knowledgeable interpreter to other Kaluli about what the "yellow skins" are up to.

When I returned in 1982, I found that Gigio had married Faile; she joined him each evening with the cooking and washing work, and then often with the conversations that we typically had before we all went off to sleep. Some of the reading sessions with Gigio also included Faile, a monolingual Kaluli speaker with no experience outside of Bosavi. Gigio also helped me stage my most experimental attempt to hear Kaluli responses to *Sound and Sentiment*, by taking a tape recorder to the communal longhouse one night and there recording discussions he prompted about the book while I was away at my own house.

In summary, the diverse social positioning of these five readers couples with the general understanding of the book-as-work-and-object surveyed earlier to clarify the fact that the appearance of my book and the claim to Kaluli that I wrote it about them was not incomprehensible or bizarre. Now to turn to the substance of these casual and "doing school" readings and to some of the dialogues they animated.

Before translating from the English to Kaluli, I started the first sessions by letting my Kaluli readers handle and thumb through the book. A substantial amount of time was thus spent discussing the book as an object, especially the amount of and kinds of black and white and color photographs, line drawings, and print. Doing this, Ho:nowo: and Ayasilo: noticed that there were two different kinds of print, standard and italics, and they questioned why this was so. That started the next dialogue in motion.

I explained that at many places in the book I told an idea first in the Kaluli language (italic print) as they had helped me write or transcribe it from conversation, texts, or songs. Then I said the same thing in English (standard print) so that my people would understand it. I did not know a Kaluli term for "translate," so I simply pointed to the italic writing and said (in Kaluli), "Here it is written in Kaluli," then pointing to the standard typeface below said, "and then the same thing is written in English." Ho:nowo: pointed and said (in Kaluli) "So Kaluli language is written there, then turned around in English language written there below."

I then questioned him in English and Kaluli about his use of this verb "turn around" for the English word "translate," with two things in mind. First, it is ironic that in *tok pisin* the term for "translate" or "translator" is *turnim tok*. (The term for the *tok pisin* verb "turn" is not related and comes from English "about," namely, *baut, bautim.*) My immediate thought was that Ho:nowo: had directly back-translated this Kaluli verb from the *tok pisin* idiom *turnim tok*, thereby coining a literalized Kaluli verb for "translate."

But another piece of lexical evidence was contradictory. The same Kaluli verb for "turn around" is used in poetic metalanguage to refer to a text copied in terms of major imagery but reformulated with new place-names and minor imagery. This kind of "turning around" is a compositional strategy for recycling poignant poetic phrases while dressing them up enough to have a fresh impact that bears the mark of their singer/composer. What Kaluli call a *gisalo nodolo:*, a "turned around *gisalo* song," is one that has had the text reworked in this way. One way to rework a *gisalo* text is to switch back and forth between the use of the Kaluli and Sonia languages. The latter is known by few Kaluli speakers, and this kind of textual "turning around" has the effect of obfuscating the message, building the poetic intensity until the idea is then again "turned around" back into Kaluli. I was taught this metalinguistic term and the strategy it labeled in 1977 when I learned to compose *gisalo*, and I noted it in the chapter on *gisalo* in my book (1982:166). My teacher on that subject was principally Jubi, an older Kaluli man; it is highly doubtful he could have been aware of the *tok pisin* term. Therefore I had been under the impression that this "turn around" notion was indeed an old Kaluli compositional term related to nuances of linguistic similarity and difference, or even code-switching per se in the song context.

When I mentioned this, Ho:nowo: and Ayasilo: both claimed that what Jubi had told me was correct, that "turn around" was also an old Kaluli way to talk about code-switching or translation. They said they had not "turned" this "turn around" term from *tok pisin* back to Kaluli; that it was *Kaluli to hedele*, a "truly Kaluli word," that is, not a loan-word or introduction from another language. Nevertheless, there are a variety of ways that "turn around" could have come from the *tok pisin* word *turnin tok* in the last thirty years, and it is doubtful whether we will ever really know the solution to that lexical puzzle.[4]

But the real importance of "turning around" Kaluli as the ethnographer's translation work comes by juxtaposition with the next part of the story. During this discussion, Ayasilo: and Ho:nowo: also noticed that there was far more standard print than italic print in the book. I explained that after I translate from Kaluli to English, the book then uses more English to tell the meaning of the translation. I should point out that having noticed this difference, Ho:nowo: and Ayasilo: did not question or contest why there was more English than Kaluli. In fact, it made perfect sense to them that a small bit of Kaluli would have to be followed by a long stretch of English, because even once the Kaluli is directly translated ("turned around") they assumed that it would take a long time to reveal ("turn over") all the relevant meanings ("underneaths").

During this discussion I used the Kaluli word *hego:*, which means "underneath," for the English word "meaning." The phrase *hego: wido:* (or *wilo:*) means "showed the underneath."[5] The implication is to lay bare the meaning, to indicate what might not be literally evident, to show another side of the coin, or, literally following the idiom, to get under the surface of things. The notion that meanings are "underneath" surfaces is a rather fundamental Kaluli idea. Things are not simply what they appear to be; what is intended is always potentially far more than what is said or how it is said. In this context, the Kaluli metalinguistic label *bale to*, "turned over words," is quite apt to designate metaphors, obfuscations, allusions, connotations, lexical substitutes, and poetic devices (Feld 1982:138–44).

The everyday speech contrast of the two verbs *nodoma*, "turn around," and *balema*, "turn over," is revealing. One "turns around" objects with observably symmetrical, oppositional, or discrete planes; one "turns over" continuous multi-surfaced objects without them. Replacing one language with another is a "turning around" of discrete items by substitution. Getting to the "underneath" of what is implied is "turning over" words to rotate or shift their multifaceted figure and ground possibilities.

Ayasilo: and Ho:nowo: then told other Kaluli that this book for "yellow skins" is "turned around" and "turned over" Kaluli.[6] They said we ethnographers are *Kaluli to nodolesen kalu*, "Kaluli language turn around people," and *hego: widesen kalu*, "underneath shower people," whose books "turn around" Kaluli into English, and then "show the underneath" of the words. This rather clever image of the intricacies of ethnographic work fits, even if the notions of "turn around," "turn over," and "show the underneath" strike you more in the way of a kinky cross-language pun. After all, we may claim to benignly or sympathetically "translate" and "interpret" languages and meanings, but many of our critics claim that this amounts to little more than "ripping off" or "fucking over" the languages and peoples concerned.

Progressing to other readings, the tendency turned to more about the book's words rather than the nature of the book's work. Before discussing the details of some of these readings, a word about the general style of these dialogues is in order, The form often went like this familiar scenario: You begin to recount a story, and without your realizing it, the story is in fact one your partner has already heard. Perhaps you previously told it to them and forgot you did so. Perhaps it was told to them by another person. Perhaps that other teller first heard it from you. In any case they are hearing a familiar story, but being either polite, disinterested, or unable or unwilling to interrupt, they let you continue the tale without letting on that it is redundant for them. But at some point when you, the teller, pause or stumble for a word, your hearer provides it, and capitalizes on the opening to finish or close the interchange, or even fully situate the hearing as a second one.

Interactions like this constituted the most common form of my readings with Kaluli. Essentially I was providing foregroundings or anchors or scaffoldings to things they knew well; they would dive in and remind me of the workings and outcomes of it all, as if I were unaware of them, had forgotten them, or as if to remind me that we'd been over this ground before. Other times my translations might be slow or halting; a second spent stumbling around for a word or mispronouncing it is all it took to animate a Kaluli hearer.

Part of this was simply a matter of excitement and of Kaluli interactional styles, many of which seem governed by the maxim: Always maintain intensity; don't hold back. Kaluli interactions, with outsiders and Kaluli companions, often come across as animated, sharp, bubbly. This is a key feature of Kaluli assertion (E. L. Schieffelin 1967:118–26; B. B. Schieffelin 1979:141–3). Borrowing some hip-hop argot from Grandmaster Blaster, many Kaluli don't hesitate to

> cap your rap,
> seal your deal,
> steal your meal
> while you spin your wheel

Indeed, it does not take long to figure out that Kaluli are quick to speak up and quicker to interrupt. You don't have to be like me, Jewish and from the urban Northeast, to enjoy and engage in Kaluli interactions, but I think it helps, particularly in terms of interpreting the subtleties of this lively interpersonal and verbal style as collaborative engagement rather than pushy abrasiveness (a not-so-uncommon attribution made both about Jews and Papua New Guinea Highlanders).

In any case, a good amount of interruption, side and splinter conversation, overlapped speech, and direct or challenging polyphonic discourse is common in Kaluli interaction, whether light or heated. And in this marked context I felt like I was often able to say no more than a few words before Kaluli would steal the moment, elaborate the tale, or provide the punchline without any of the buildup. Often I was left with the sense of "what you meant to say was . . ." before I knew what hit me.

Yet the form of these interchanges went beyond the use of my utterances as precadential formulae, *aide-mémoires*, or Rorschach stimuli for willing rap-cappers. The much more interesting outcome of this was that even when I was able to get a fair amount of my story told, my Kaluli readers essentially reconstituted portions or versions of source materials in my field notes upon hearing them summarized, capsuled, or stripped of their situated details. Kaluli took my stories and resituated them as their own as they had once before. To do that, they took every generality I offered and worked it back to an instance, an experience, a remembered activity or action. In effect, they "turned over" my story by providing recountings of the stories that more typically are left behind in my field notes, the stories I otherwise mined in order to report Kaluli "underneaths" to my own "yellow skin" constituency.

More pointedly, the abstracting, depersonalizing, summarizing, and generalizing moments that appear in my ethnography unanchored to specific instances, attributions, and intentions are the ones that Kaluli readers most often responded to with a concretizing and repersonalizing set of questions, side comments, or interpretations. On the tapes from the evening when Gigio took my tape recorder to the longhouse, the most common interjections are: "who said that?" and "who told him that?" It was that desire to situate knowledge and experience with specific actors, agendas, and instances that was most on my Kaluli readers' minds.

An example of this sort of contestation was evidenced both in the readings of the chapter on *gisalo* songs and the comments about that chapter that Gigio recorded. That chapter contains a case history of a song sung through a spirit medium during a seance. The medium was a man from a distant community, and the seance took place at Nageba:da:n, a community an hour or so away from Sululib, our home. Additionally, the initial transcriptions of the songs were done with the help of Kulu, a young man who was also from a distant community, and most of the exegetic commentary, responses, and ethnographic information that went into my characterization came from work I did with the medium and with people from Nageba:da:n who were at the event. Few people from Sululib experienced the event, although many had heard the tape recordings.

The discussions then constantly involved quizzing me about who told me what and, one way or another, mildly challenged the authority involved. Direct and veiled accusations that the medium was a fraud and that my transcription assistant was a Christian who didn't really understand the "turned over words" of the songs were mixed with queries as to whether Buck and various people from Nageba:da:n also agreed with what I said, and suggestions that I really should have discussed a song sung during a seance at Sululib by another medium everyone (including Buck and me) knew was a better performer.

Another example involved my characterization of male and female styles of weeping. The second chapter of the book contains a metalinguistic, structural, and behavioral statement of the dimensions of contrasts that differentiate these styles. When we read this material, my readers immediately complicated the generalizations I offered, largely related to events we experienced together, as if to question my memory. They recalled that most of my experiences of weeping derived from funerals that took place when I initially arrived in Bosavi. Hearing their comments was like reading my field notes. In other words, they set their explications in the context of specific events, actors, and actions, and constantly asked who had told me one thing or another. Field notes, of course, attend to such on-the-spot actions and situations in a way that pattern overviews typically do not.

The problem of compacting and compressing instances, structuralizing their form, and presenting them pulled away from the biographies and practices in which they were embedded is no minor problem in the critique of ethnographies. So how do we understand the obvious differences of the account and its readings? One way is to claim that they are fairly superficial. For example, there is always a difference in degrees of remove from situated experiences. And book work implicates style; the writer must select, edit, compress something. Investment and salience are also different; the ethnographer more typically accounts for self-investment and the reported investments of many Others rather than the single perspective of any one actor, any one Other. Different audiences also implicate different expectations.

An alternative is to suggest that Kaluli have given me a critique and lesson in poststructuralist method; that they have exposed a deep problem about (my) ethnography, and not a superficial one. A more cynical reaction would be to claim that all I have shown is that my Kaluli commentators are stuck in a world of the concrete (or stuck in the forest mud) and that this is precisely why an ethnographer is necessary – to tell "the point" of it all. Surely *Sound and Sentiment* is not intended as unmediated copy of "the native point of view." I think that few ethnographers these days would quibble with Geertz's (1976) assertion that ethnographies are supposed to be what we ethnographers think about things as much as they are supposed to be accounts of what we think the locals think they are doing.

Up against these possibilities, my own take on the interpretive moves of my readers is more local. I don't think that Kaluli readings of my ethnography are poststructuralist and praxis-centered, or that Kaluli are really "grounded," unable or unwilling to abstract what is going on in a way anything like mine. I also don't think that the issues here are easily resolved by just attributing them to differences of writing and audience. Part of my reasoning turns on the next layer of readings.

When I was reading with Gigio and Faile from the material in my chapter on women's weeping and recounting there the weeping by Hane *sulo:* over the death of Bibiali at Aso:ndo:, Gigio quickly interrupted, giggling slightly, asking if I had told the story of my trip to Aso:ndo: to record the weeping and of my unexpected overnight stay there. I told him that I had not, and he was truly puzzled by that.

He then quickly began to recount in great detail how I had only been at Sululib for one month, how it was my first trip away from the village without Buck or Bambi, how I barely spoke Kaluli, how they had left me in the hands of a guy named Kogowe, instructing him to return with me the same day, how many people thought Kogowe was a bit flaky and unreliable, how everyone started speculating on why I wasn't home when it turned dark, how upset Bambi was that Kogowe might have gotten lost leading me back, how there was one really bad river bridge to cross on the way, how it had rained heavily all afternoon so maybe the river flooded or I slipped on a log and fell in, how Buck managed to gather up Hasele and Seyaka for the miserable task of walking with him through the forest at night, two-and-a-half hours from Sululib to Aso:ndo:, and how when they got there, they just found me resting comfortably by the fire with a mild stomach ache.

And Gigio went on to tell how we stayed up that night and listened to the tapes with the mourners at Aso:ndo:, and then walked back home the next morning at the crack of dawn, and how everyone playfully teased Kogowe about getting lost, while Kogowe kept protesting that after starting out in the heavy rain the new "yellow skin" kept falling down, stopping him in order to fix the plastic bags protecting the tape recorder case, and at each instance of a fall or stop turning yellower, as if he would puke any minute.

Gigio told this very dramatically and had Faile and me in stitches. But it was more than an amusing story about embarrassing moments in the lives of the "yellow skins." It was Gigio saying, on the "underneath": "This is what good stories are made of; so why didn't you tell it?" Here we were beyond the more typical routine of Kaluli hearers trying to position my account in terms of what other Kaluli speakers said, thought, or told me. Now I was hearing Gigio criticize me for not putting enough of myself in the book. I found this at once a recognition that Gigio read the book as *my* story as much as anyone else's, but also that the concern with positioned Kaluli speakers and their biographical accountability was no different for "yellow skins."

Later on there were a number of similar instances of asking why I didn't tell my stories. I couldn't say to Kaluli that I simply didn't tell my stories more because I often felt them to be unimportant sources for illuminating the Kaluli stories I was trying to tell. So I defended myself by reading with them certain sections of the book where I am more clearly situated in the story. But what was always at issue in these dialogues was the need for a personally situated point of view. My sense here is that Kaluli readings of *Sound and Sentiment* key closely on their sense of biographies, because biographies frame what is memorable about experiences. They extend that concern to all stories, tellers and tellings, even if they don't imagine that other meanings might be assigned to them beyond the ones they momentarily have at hand. I think Kaluli assume that what they find memorable about me is also something that I should be able to recognize. And it is perplexing that I might ignore such an obvious fact in the context of writing about events and times we shared.

One other Kaluli cultural framework helps make sense of this issue; namely, a clear model for this kind of storytelling is neither myth nor historical narrative, but song. There are two reasons for that. One is that Kaluli assume that I know what songs are about and how they are constructed. Another is that song poetics are the height of an aesthetic evocation of the meaning of shared experiences. Kaluli song poetics simultaneously reference abstract qualities and values, and personal situations and experiences, particularly poignant ones. Since the book so deeply concerned such questions, I found that as time and readings progressed, Kaluli seemed to absorb and respond to it as a kind of meta-*gisalo* song, an evocation about evocation, a map of shared experiences about other maps of shared experiences.

In part, the book readings (particularly the chapters on poetics and song) were greeted like any Kaluli performance meant to move an audience, a performance intended to communicate to that audience the skill, care, and affective sensibilities of the composer/performer. And after the fact, I realized that during our reading sessions my companions frequently acted exactly the way Kaluli act at performances, rather than the way they act when we "do school." They often mixed side comments, wild interjections and exuberant chuckles, quiet clucks accompanied by downcast head turning (as if to say "this is heavy stuff"), feigned distraction and disinterest, intense concentration and engagement, and puns, put-ons and joking rounds playing off a passing verbal phrase or two.

As a matter of fact, there were instances in the readings of my chapter on *gisalo* songs when Gigio, Ho:nowo:, and Ayasilo: all went into silly mock weeping routines, as if both the recounting of a powerful song and my sung/spoken rendition of it had moved them to tears the way a real *gisalo* performance might well do. At one point I read Gigio the section recounting the time I composed a song about my loneliness over Buck and Bambi's departure, telling how it brought tears to his eyes. He playfully mocked weeping and limply fell over onto me, the way weepers at a ceremony throw their arms around the dancer they have just burned in retribution for a song that moved them to tears. Then he popped up and burst out laughing hysterically, exclaiming *Yagidi ni Sidif-o!*, as if to say "this is *too* much, Steve!"

What I think was going on there was the negotiation of a playful frame for a moment recalling shared experiences whose original experiential frames were emotionally highly charged. In a certain sense this was probably the most genuine and natural way for Gigio to take the book seriously, to communicate a positive and friendly aesthetic response to me, and to act perfectly Kaluli about the whole matter. In other words, Gigio knew that the way to greet my telling was not with a casual "that's good" or "yes, it was truly like that." Such responses would be distanced, impersonal, uncharacteristic of our relationship, (and something that I would read as "informant behavior"). Gigio's manner of response was a way to say "we can laugh about the heaviness we shared." This Kaluli way to reaffirm the power of shared feelings and experiences is not uncommon; instances of mock weeping are often invoked to convey expressions of camaraderie and affection among young Kaluli men.

Another way to help illuminate the dynamics of Kaluli interpretive style here is to focus on the parts of *Sound and Sentiment* that seemed to be read most easily and successfully, and the ones that seemed to be most troublesome. Most successfully read were my telling of the *muni* bird myth, material in the chapter on birds, and much of the material in the chapter on song poetics. Less successful were some of the things in the chapters on weeping and song. This puzzled me because, with the exception of the ornithological materials, it reverses what I said earlier about a preference for the concrete. Indeed, the weeping and song chapters had the longest and most specifically situated case histories in them, whereas the myth and poetics materials were more often structurally compacted.

In the case of the weeping and song materials, which in the book include a full transcription, translation, involved case history, and explication for individual performances, the problems involved the nature of providing a context for a microanalysis, the nature of which item was selected for such intense treatment, and the fact that neither of these major case studies come from events that took place in my home village.

For example, with the *gisalo* song, Gigio was quick to remind me that it was an early one in a larger seance that included 13 songs. For Gigio this was not in fact the most memorable of all the songs. He was also right that a later song that also moved the same man to tears came at a more climactic moment in the overall seance, and that the later song was also longer, more poetically complicated, and moved several other people to tears as well. My real dilemma here was that Gigio was not only right about all of these things, but that Aiba, the medium, Neono, a man who wept for both songs, and all the original consultants both from Nageba:da:n and Sululib told me exactly the same thing in 1977. I agreed then, and still do now, that the later song was more forceful (though no more typical of the genre) from musical-poetic-performative standpoints. But my choice of the song that appears in the book derived entirely from other considerations.

For example, the later song was almost one-and-a-half times as long as the earlier one, and involved linguistic, poetic, and pragmatic factors that would have required a much more extended discussion in the book. The multiplicity of agendas embodied in that song and the seance activity surrounding it meant that I could not have explained it clearly without discussing the larger event and its participants in greater detail. Also, I ran out of tape in the middle of that song, and the change of reels deletes about 30 seconds where I am not entirely sure of the text. Even though I worked with several people on that issue and have a pretty good idea of exactly what was included in the untaped verse, the situation was not ideal, because I wanted to publish the analyzed song on a record (as I have, Feld 1985) so that my readers could relate the performance to my description and analysis of it. Moreover, I was not really concerned with a discussion of the most powerful *gisalo* from this or any other single event, but simply with one that worked well to typify the style and the performance issues, and could accommodate my concern to integrate a case study into a larger sociomusical discussion of the genre.

Likewise, Gigio and Ho:nowo: were quick to point out that the *sa-ya:lab* weeping example that I picked for close scrutiny was done by a single woman. Indeed, *sa-ya:lab* are more typically wept by two to five women simultaneously. And there were other ways in which Hane *sulo:*'s long *sa-ya:lab* was not typical: it was more songlike than many, more poetically complicated, less ordinary, more profoundly moving. Here we have the inverse of the problem with the *gisalo* song selection. I picked what everyone agreed was the most forceful of the *sa-ya:lab* performances I had recorded for my case study only to be told (again, as I had been told before and knew well) that there were ways in which it was not entirely representative.

What I found interesting in these discussions was not that my readers were contesting my selection. Rather, it was that they were responding in real Kaluli style. Kaluli men seem to assume that whether or not they have anything substantial to say, or are explicitly asked their opinion, they are expected to have one, and expected to be ready with it and entirely up-front about asserting it. A premium is placed on having something to say, on saying it as a form of collaboration, and on engaging demonstratively. Talk is not only a primary measure of Kaluli social competence, but an arena for the display of intelligent interactive style, what Kaluli call *halaido*, "hardness"

(Feld and B. B. Schieffelin 1982). If my readers were giving me "a hard time," it was in their cultural idiom, and not mine.

Gigio and Ho:nowo: never told me that I was "wrong," never proposed explicit changes, nor indicated that I should have said things differently. That would be too trivial a response and out of character. Their discussions opened issues rather than resolved them, and their comments were filled with sentences that opened or closed with classic Kaluli hedges, *hede ko:sega*, "true, but . . ." and *a:la:fo: ko:sega*, "like that, however . . ." The pragmatics of these very typical Kaluli phrases are complex, not just in terms of whether they open or close an utterance, but also in terms of how they work to always keep the conversation moving.

What Gigio and Ho:nowo: did say about my editorial policy was also very Kaluli; they occasionally responded to my assertions with a terse but semantically complicated Kaluli term, *ko:le*, "different." Sometimes this term can and should be taken at face value, a neutral and direct "oh, *that's* different." The term also can be distancing, carrying the sense of "well, OK, but that's *your* thing." It can also carry a very positive sense of different, a sort of "far out, I never it saw it *that* way before." It can also imply a rather bland "different," carrying the sense of "I suppose you *could* see it that way." Or a more evaluatively suspect "that's *different*." Even when attending carefully to syntax, conversational context, intonation, and paralinguistics, it is not easy to get a single semantic reading when Kaluli use this term. My intuition is that the term more often frames multiple or ambiguous attitudes rather than singular ones in any case. Here it seemed that Gigio and Ho:nowo: used it in virtually every way with me, creating a continually mixed feeling of acceptance and challenge.

As for the easier read sections, I went over all of the bird taxonomy, symbolism, and stories in real detail with Jubi. He was perhaps the best ornithologist in Bosavi, and had worked longest and hardest, at one point almost everyday for five straight months, with me on the bird materials originally. I was interested in his reading, and interested to see how similar or different his interpretations would be five years later. In about a week's time we went through the whole chapter; he corrected me on about four or five identifications that I had botched, elaborated others, insisted that I had "forgotten" certain things, but basically gave me the sense that my bird portrayal was fairly complete and accurate in terms of his knowledge and understanding.

It was clear, however, from Jubi and others, that I had not gone far enough in stating that classification of birds by sound was more typical of Kaluli everyday use and knowledge, and more salient than the detailed classification by beaks and feet to which I had devoted so much formal lexical attention. And if I had it to do over, I think that a restructuring of the way those two classifications are presented would be in order. Also, more attention to other bird myths would be in order, as I found that Jubi invoked them often, as he had done in the past, in order to explain the "underneaths" of bird colors, sounds, and behaviors. In any case, what was most successful here was the organization of the material in terms of metalinguistically and culturally focused Kaluli domains.

Similarly, what was successful to all my readers in the materials on poetics and song structure was the orderly presentation of things following the Kaluli metalinguistic demarcations. Like the ornithological materials, sections of the book framed by Kaluli domains led my readers to act as if my role in the presentation were more secretarial than "turn around"/"turn over." Set in that light, the isolated phrases from songs as examples were questioned less for being taken out of context. There were plenty of instances here that led me to feel justified in believing and stating that Kaluli can and do think quite abstractly and theoretically about song form, composition, and poetic construction as a kind of symbolic persuasion.

## Dialogic Editing of Another Kind

I thought my most radical move in *Sound and Sentiment* consisted in simultaneously stressing the theoretical importance of sound (as distinct from music or language per se) and its situated importance in understanding how

Kaluli constructed and interpreted their expressive modes. It turned out that this was read as rather less adventurous by both Gigio and Jubi. They were taken by how much time I had spent discussing a single song and a single weeping episode, but then how much less I had spent talking about the more mundane daily sounds – the ones that tell the weather, season of year, time of day. They asked why I told so much about birds but so little about frogs, about insects, different animals. They asked why I had told the *muni* bird myth and not told many others. They asked why I had not told about how all sounds in the forest are *mama*, "reflections" of what is unseen. I responded that I thought birds were most important; they had more stories, there were more of them, Kaluli *ane mama*, "gone reflections" (spirits of dead) more often show through as birds, and so on. They did not dispute this; they simply made it clear that every sound was a "voice in the forest" and that I should tell about them all.

The responses of Jubi and Gigio to the emphasis on weeping, poetics, and song, and to the ethnoliterary device of the case example made it clear to me that there was a gap between my emphasis on the meaning of Kaluli-performed human sounds and the kind of practical and affective everyday interaction with environmental sound that more deeply grounds the specific aesthetic and performative arenas that I focused upon.

My main response to this was to record more everyday sounds, usually on early morning and late afternoon treks each day in the surrounding bush with Jubi, and then to have playback sessions where I would let the tape recorder run and simply invite people to sit around and listen. I also stayed up all night on several occasions to record nighttime forest sounds, and tried to get Kaluli to identify and discuss all of them. What I was tying to do here was to create a pool of sensate material from which Kaluli and I could have different kinds of discussions from the ones we more typically had about linguistic, poetic, and musical material. My hope was that this kind of refocused activity could lead to better realizations about the nature of sound, particularly at the level of everyday Kaluli meanings and interpretations.

The dialogues that followed made it clear that the sociomusical metaphors I had earlier identified in discourse about human sound are thoroughly grounded in natural sounds. For example, *dulugu ganalan*, "lift-up-over-sounding," is an important concept in Kaluli song form and performance. It turns out to also be the most general term for natural sonic form. Unison or discretely bounded sounds nowhere appear in nature; all sounds are dense, multilayered, overlapping, alternating, and interlocking. The constantly changing figure and ground of this spatio-acoustic mosaic is a "lift-up-over-sounding" texture without gaps, pauses, or breaks.

This key image clarifies how the soundscape evokes "insides" and "underneaths (*sa* and *hego:*) and "reflections" (*mama*). These notions involve perceptions, changes of focus and frame, motions of interpretive access to meanings packed into layers of sensation as they continually 'lift-up-over" one another. It is not just that the forest is the abode of invisible spirits; it is that all sounds invite contemplation because their juxtaposition and constant refiguring make it possible to mildly or intensely interpret presences.

"Lift-up-over-sounding" sounds and textures disperse, pulse, rearrange. This constant motion is also an energy, a "hardness" that comes together and that "flows," remaining in one's thought and feelings. In song poetics, "making hard" (*halaido doma:ki*) is the image of competent formation; it is force, persuasion, the attainment of an energized evocative state. The holding power of that "hardened" state is its "flowing" (*a:ba:lan*), the sensation that sounds and feelings stay with you after they have been heard or performed.

What to do with these new understandings and new sounds? I had already produced two sampler phonograph discs illustrating all Kaluli song and instrumental styles (Feld 1981, 1985). The first of these contained an 8-minute-long unbroken stretch of Kaluli talking, singing, and whooping recorded during garden clearing work. But this brief attempt to place Kaluli soundmaking in the environmental context does not contain examples of the interplay of human and natural sounds (like singing and whistling with birds and insects) that I recorded in 1982. I decided that an extended version of this kind of recording would make it possible to illustrate the interaction of environmental sounds and Kaluli aesthetic sensibilities at the everyday, nonritual/ceremonial level. That led to the conception of *Voices in the Forest*, a tape recording depicting a day in the life of the Kaluli and their tropical rain forest home.[7] This tape attempts an editing dialogue with sounds in order to work more reflexively with Kaluli in a sensate idiom so naturally their own.

When fieldworkers make tape recordings and select a representative set of materials for publication or presentation, they generally follow certain realist conventions of sound as a mode of ethnographic representation. In these practices I think it fair to claim that the typical mode of tape editing and use is literal and descriptive; more important, bounded and discrete. The on/off switch or the fade up/fade down potentiometer of the tape machine marks a control over the finiteness of a recorded item. What comes in between the on and the off or the fade up and the fade down is itself expected to be whole, unmanipulated, unviolated. In other words, we expect a tape excerpt or a record band to be a true sonic index of the temporal stretch it occupies. Assurances that what we hear in that temporal stretch has not been spliced, cut, rearranged, altered, filtered, mixed, or otherwise edited are part of the guarantees of a recording's authenticity.

The kind of selection and editing necessary to construct *Voices in the Forest* is of a different sort. While it is a sound construction that is both narrative and realist in convention, it is closer in concept and execution to *musique concrète* than ethnomusicological tape display. Its form is accomplished by editing sounds. While it displays a concern with both ethnographic representativeness and audio accuracy, this concern is realized compositionally rather than literally. In this sense it owes much to R. Murray Schafer (1977) and the World Soundscape Project's concern that soundscape research be presented as musical composition.

To make *Voices in the Forest* I selected three hours of source material from about 60 hours of recordings made in 1976–7 and 1982.[8] The selected materials were arranged according to various graphs (again, modeled in part on Schafer's work) made in the field. These plot the pattern and interaction of daily human and natural sound cycles and have the names of the sound sources as well as Kaluli commentaries about them, noted at the time of recording or during playback sessions. These selections represent the typical cycle of sounds during a 24-hour period, patterned from a human point of view. In other words, the progression of sounds follows the progression of general Kaluli activities in the village and surrounding forest settings. The recording then attempts to present a participant's spatiotemporal ear-perspective.

Once the materials were arranged, no scissor cuts were made. Editing was accomplished by rerecording slices of the source material directly onto an eight-track recorder, using three sets of stereo tracks and two monaural ones. The eight tracks were then mixed down to two, continuously cross-fading to create the illusion of seamless narrative. In this way the sounds sampled from a 24-hour period are condensed to 30 minutes, beginning with the early morning hours, progressing through dawn in the village, morning and midday work in the forest, an afternoon rain storm back in the village, dusk and settling in for the night, and returning to the night and early morning hours.

As a sound object, *Voices in the Forest* is a mixed genre: experimental ethnography and musical composition. Its sources of inspiration include a variety of non-Kaluli notions that condition my perception of sound as an environmental sensorium. Nature recordings (like Jean-Claude Roché's extraordinary series *L'oiseau musicien*), environmental compositions (like R. Murray Schafer's *Music for Wilderness Lake*), and experiments in interspecies communication (like Jim Nollman's underwater guitar duos with dolphins) have all been interesting to me in this regard; they tell cultural stories about nature.

*Voices in the Forest* also tells this kind of story, using Kaluli directorial participation and my technical skills to complement each other. Here multitrack recording becomes the ethnoaesthetic means to achieve a Kaluli "lift-up-over-sounding" and "flowing" sound object full of "insides" and "underneaths" that speak to the "hardness" of Kaluli stylistic coherence, and to its "reflection" in my appreciation.

The main thing I have learned from the experience of recording the sounds, discussing them in the field, and editing *Voices in the Forest* is that for Kaluli, the nature of sounds is far more deeply grounded in the sounds of nature than I had previously realized. In other words, Kaluli culture rationalizes nature's sound as its own, then "turns it over" to project it in the form of what is 'natural" and what is "human nature." This is the link between a perception of a sensate, lived-in world and the invention of an expressive sensibility. "Lift-up-over-sounding" sounds that "harden" and "flow," producing a sense of "insides," "underneaths," and "reflections" reproduce in Kaluli cultural form the sense that nature is natural, and that being Kaluli means being aesthetically "in it" and "of it." This is both the background and stage for Kaluli expressive styles, the natural condition and world-sense that makes it possible for bird sound, weeping, poetics, and song to be so inextricably linked, not just in mythic imagination and ritual performance, but throughout the forest and in the treetops at the same time.

While dialogic editing of the first kind mostly taught me how Kaluli felt the "underneath" of *Sound and Sentiment* needs more sentiment, the second kind made it possible for us to work together to "harden" the sound.

# 31

# Senses

## *Michael Herzfeld*

### Common Sense, Body Sense

Anthropology, like all academic disciplines, is primarily a verbal activity. Even the study of visual media must always be expressed in words. Recent attempts to introduce pictorial representations of human movement (notably Williams 1991; Williams, ed., 1997; Farnell 1995) suggest the inadequacy of this Cartesian commitment. We have seen already that modern representational practices are heavily dependent on visual formats, but even this restriction seems to appear most commonly as an extension of verbal texts. A diagram without a caption would not be easily understood. In consequence of this bias built into the preferred modes of representation, the role of smell and hearing, not to speak of touch, has been grossly under-represented. Can suffering, the theme of the previous chapter, be understood without reference to sensation? Especially in this chapter and in the chapter on aesthetics, I shall try to suggest paths along which anthropologists have been trying for some years now to rectify this pervasive absence.

The technical difficulties of recording smell and taste are formidable, and have certainly inhibited progress. Much of the work even on the significance of gesture must proceed through verbal responses to visual cues (e.g., Cowan 1990). The possibility of knowing "what something smells like" to a member of one's own culture is remote; when we add the further problem of cross-cultural translation, the difficulties may seem to be insurmountable, especially given the intractability to analysis of psychological inner states. It is nevertheless encouraging that a few scholars have begun to broach an "anthropology of the senses" – although I would caution that this label risks marginalizing, as yet one more specialist concern, what really ought to be a central concern for the comparative study of cultures and societies.

The fundamental premise underlying the concept of an "anthropology of the senses" is that sensory perception is a cultural as well as a physical act: sight, hearing, touch, taste, and smell are not only means of apprehending physical phenomena but are also avenues for the transmission of cultural values. While the most obvious domains for this process may be the performing arts, it is also an integral part of social relations: smell, for example, creates social boundaries, not because some smells are naturally bad, but because they are culturally constituted as such. (If one considers the radical difference between most Southeast Asians' enthusiastic response to the smell of the durian fruit in contrast to the disgust it

Michael Herzfeld, "Senses," pp. 240–53 from Michael Herzfeld, *Anthropology: Theoretical Practice in Culture and Society* (Malden, MA: Blackwell, 2001). © 2001 by UNESCO.

evokes from most Europeans, for example, it becomes immediately apparent that smell is as culturally relative as aesthetic judgment.) This realization is an extension, rarely recognized as such, of Mary Douglas's famous insight that "dirt" is a matter of cultural categories rather than of biological fact (Douglas 1966), as is indeed suggested by the association of the less recordable senses with concepts of pollution and cleanliness – a bad smell, a disgusting sound, a slimy touch.

Like the notion of order that defines dirt, the experience of the senses is calibrated to the "common sense" – to the accepted range of what is "self-evident" (Douglas 1975) – in any given society. In this regard, the study of the senses is remarkably like that of, say, economies: it is resistant to such anthropological relativization because this relativization threatens the security of our own unconsciously cherished perceptions and thus – especially – of the idea of a transcendent "common sense." Indeed, the etymological splitting of terms like "sense" and "taste" into two streams – the cerebral and the sensual – is diagnostic of the extent to which Cartesian and even earlier western assumptions about the separation of body from mind have taken hold of our consciousness.

This chapter should be read in close association with the chapter on aesthetics. The very term "aesthetics" is derived from a Greek root connoting the subjective perception of feeling. In the West this category is freely extended to the auditory domain: music is especially prominent here. Yet this term, too, is restricted in its applications by particular cultural definitions of the sensorium. The addition of taste and smell is rare, half-humorous (calling a chef an "artist" has all the metaphorical ring of artifice), and confined to relatively few domains. Perhaps the commodification of visual art offers a clue here: given the economic significance of collecting as well as the heavy investment in monumentality made by nation-states, the difficulty of recording smell and taste in some reproducible and reasonably durable medium has marginalized these senses more than any other except touch – which, because it is primarily dyadic and thus relatively private, generally escapes social analysis altogether.

## An Anthropology of the Senses?

In recent years, anthropologists concerned with the restrictive understanding of the phenomenal world that is possible using the conventional descriptive instruments of an academic discipline, have begun to explore new approaches. Some of these approaches are inspired by phenomenology (e.g., Jackson 1989), some by growing awareness of dominant medical paradigms of embodiment (Desjarlais 1992; Kleinman and Kleinman 1994), some by the realization that historical knowledge may as easily be embodied as objectified (Connerton 1989; Seremetakis 1991, 1993), and some by a focus on the inculcation of social knowledge by nonverbal means (Coy 1989; Kondo 1990; Jenkins 1994). The contribution from medical anthropology is potentially perhaps the most radical, because this field tackles the Cartesian paradigm at source – in the body itself.

A few anthropologists, moreover, have bravely tackled the whole gamut of "the senses" as a key topic for the discipline. Among these, C. Nadia Seremetakis (ed., 1994) and Paul Stoller (1989) have offered us exercises in reflexive exploration. Others, notably a group of Canadian scholars including Constance Classen (whose work is central to this chapter), have attempted to synthesize a comparative "anthropology of the senses." This is an important departure. It places the sensual in systematic analytic focus for virtually the first time, and it substitutes methodological challenges for vague assertions.

It also, however, inevitably raises the usual difficulty associated with the invention of any new "anthropology-of" formulation. The small number of scholars so far engaged in this enterprise suggests, above all, the risk that the senses – other than those already dominant – will remain marginal to ethnographic description unless, in some practical fashion, all of anthropology can be recognized as necessarily shot through with alertness to the entire gamut of sensory semiosis. I use the term "semiosis" rather than "perception" here advisedly, for I wish to signal the importance of recognizing that what this new development offers is a specifically social, as opposed to psychological, assessment of how the various senses are used.

Finally, I suggest that another risk is that of simply developing a catalogue of cases. As a new awareness of the centrality of the sensory emerges, some of this is undoubtedly inevitable, and indeed may constitute a necessary precondition for "resensitizing" the discipline. That said, however, a slight intimation of strain may appear as this chapter progresses, for my own intention – which is surely not ultimately at odds with Classen's – is not so much to list all the exciting new areas in which we can explore sensory semiosis, but to think about how this might affect other domains of anthropological investigation. We can ask, for example, how considerations of smell might affect economic relations (a gift that smells wrong may indeed be poisoned, metaphorically or otherwise); how the nose often provides a means of ethnic, class, and even professional classification in ways that subvert the explicit social ideology of a culture; or how the physical discipline of the body through intensely boring and uncomfortable posture may not only inculcate artisanal conformity and obedience but provide space for silent mutiny and a reallocation of loyalties. Here again, I would emphasize the probable future contribution of what has already been done in medical anthropology, where the issue is no longer simply one of recognizing that culture mediates experience but has become a focus on how such mediation is negotiated and modulated through actual changes in the social sphere. An excessive focus on static "cultures" resists this insight; in this chapter, I shall attempt to reverse that flow, and so to suggest some linkages that would not necessarily accrue to an anthropology of the senses narrowly conceived.

The senses are arenas of agency. Thus, the view that perception is conditioned by culture, while unexceptionable in itself, does not suffice. Not only do the ways in which people perceive the world vary as cultures vary, but indeed they also vary within cultures; they are negotiated. Yet it is certainly useful to begin with the local understanding of what actually can be sensed and how. As Classen has shown, perhaps the most surprising realization is the fact that even the enumeration of the senses may vary. Within western history we find, aside from the customary grouping into five senses, enumerations of four, six or seven senses described at different periods by different persons. Thus, for example, taste and touch are sometimes grouped together as one sense, and touch is sometimes divided into several senses (Classen 1993a: 2–3). Similar variations in the enumeration of the senses can be found in non-western cultures. Ian Ritchie writes that the Hausa of Nigeria, for example, recognize two general senses: visual perception and nonvisual perception (Ritchie 1991: 195). Such basic differences in the divisions of the sensorium recognized in different cultures suggest the extent to which sensation is cultural as much as it is physiological.

Scholars who are interested in the cultural patterning of sensation report a wide variety of kinds of meaning attributed to various kinds of sensory experience. The senses themselves may be linked with different trains of associations, and certain senses rank higher in value than others. The elaborate attention paid to both sight and taste in the Chinese kitchen, for example, may contrast with the Balkan preference for uncomplicated tastes and frequent indifference to the visual appearance of the food. The predominance of particular senses in symbolism also varies a great deal: smell, for example, is nowadays either neutral or slightly negative in North American culture unless the reverse is specified – as it may have to be in any attempt to generalize about "the Western sensorium," because in an older western tradition the "odor of sanctity," often marked by the attractive smell of the corpse of a saintly person, was held in high esteem. Christian mystical tradition, for example, is characterized by a strict asceticism of the body coupled with a rich sensuality of the spirit, whereby the divine is conceptualized and mystically experienced through a wealth of sensory symbols. Nonetheless, today we are more likely to say of an idea that it stinks, or that a scheme smells fishy, than to praise it as "smelling of roses" (although this, too, may happen). And a North American might regard as eccentric the behavior of a tough Greek man of military service age who plucks a flower in order to savor its smell, and then rolls a single word around in his mouth with obvious sensual relish, not because its referential meaning is remarkable, but because he wishes to share his pleasure in the pure sound of it – a revealing reversal of what we so often consider to be the "real" value of speech. (It may also be significant in this connection that the Greek term *noïma*, literally a "meaningfulness," is usually understood to mean a somewhat covert gesture that would actually be betrayed by the act of speaking, especially in the kind of pretentious way that such a gesture can be used to mock: real, social meaning inheres in action, not grandiose verbiage.)

All these examples suggest that bodily sensation and cultural value are mutually engaged at all times. Our task is to explore, not only the variety of such associations, but also their consequences for the whole range of social relations and acts. Social codes determine what constitutes acceptable sensory behavior and indicate what different sensory experiences mean. To stare at someone may signify rudeness, curiosity, flattery, or domination, depending on the circumstances and the culture. Downcast eyes, in turn, may suggest modesty, fear, contemplation, or inattention. And these are simply the possibilities for cultural coding, within which personal idiosyncrasies may produce further variation in the meanings intended – and attributed to a particular posture. Yet relatively little of this makes its way into ethnographic writing. When it does, this is often because the explicit exegesis of knowledgeable informants has legitimated it – made it "real."

Classen points out that the association between sensory faculties and kinds of meaning is surprisingly varied. Sight may be linked to reason or to witchcraft, taste may be used as a metaphor for aesthetic discrimination or for sexual experience, an odor may signify sanctity or sin, political power or social exclusion. Together, these sensory meanings and values form the sensory model espoused – more or less consistently – by the members of a society. This is what it means to say that people "make sense" of the world. Classen acknowledges the likelihood of challenges to this model from within the society – from "persons and groups who differ on certain sensory values" – but, in accordance with the respect for indigenous theories enjoined in this book, we would agree on insisting that there is usually a central paradigm to be debated, negotiated, or simply – if differentially – experienced.

## Three Assumptions Challenged

Classen notes three prevalent assumptions that have impeded our understanding of the cultural construction of sense. (She herself describes that understanding as "an alternative approach to the study of culture," which suggests that one might still be able to conceive that an anthropology not fully responsive to the role of the senses could still offer a persuasive interpretation of some aspects of social life.) These assumptions are: that the senses are "windows on the world," or in other words transparent in nature, and therefore are precultural; that the most important sense is the visual; and that a more acceptable alternative to this "visualism" is the recasting of knowledge – especially about nonliterate societies – as verbal, and specifically as oral/aural.

We experience our bodies – and the world – through our senses, which we apprehend on the basis of the codes we have learned. The normative sensory model of a society thus reveals the expectations placed on individual understanding, and points up important aspects of its internal organization. For example, the gradual European abandonment of smell and increasing emphasis on vision is directly linked to the technologies of literacy and to the expansion of social relations beyond the face-to-face that these made possible.

To address the first assumption briefly, then, the view of the senses as "windows on the world" is a misleading metaphor. Unlike windows, the senses are not transparent. They are, rather, heavily encoded instruments that translate bodily experience into culturally recognizable forms. They thus frame and mediate perceptual experience in accordance with a balance of personal idiosyncrasy and socially prescribed norms. And even the idiosyncratic dimensions are variations on cultural themes. Two individuals in a given culture may not enjoy the same foods, yet they will express their respective preferences in terms of a set of preconceived categories. For examples, contrary to western stereotypes, not all Thais adore spicy food (although some may "justify" their blander tastes, which are also a claim on higher social status, by wryly attributing it to their Chinese ancestry!), yet the "quality" of the food will be debated in terms of agreed-upon notions of balance, freshness, and so forth. A Greek restaurateur I know excoriated a distinguished habitué of his restaurant for demanding that the spaghetti be cooked soft; the gentleman in question, a sophisticated traveler, complained about the "hard" pasta he had encountered in Italy, where locals usually insist that it should not be cooked beyond the toughness they call *al dente* (literally, "for the tooth"). Where mutually hostile cultures abut each other, we may even find systemic discordance within a larger common code. In the Balkans, for example, Greeks view with deep ambivalence certain Turkish dishes that combine the sweet fruitiness of raisins with meat or yoghurt or add more spice than Greeks usually enjoy; the ambivalence expresses both the historical awareness of a cultural debt and the ideology of their own cultural superiority. And individual Greeks will position themselves very differently between the two extremes, sometimes varying their stance according to the social context (preferring, for example, to affect a "European" preference for blandness in a restaurant but demanding more spice at home). Note here how sensory experience intersects with performance and context.

The second assumption that has impeded the development of an anthropology of the senses is the idea that, in terms of cultural significance, sight is the only sense of major importance. This assumption reflects a western bias that associates vision with reason. Aristotle, for example, considered sight to be the most highly developed of the senses. However, while vision was usually considered the first and most important of the senses, it was still the "first among equals" (Classen 1993a: 3–4; Synnott 1991). More than that, Classen has emphasized, sight has become "something of a sensory despot," leaving little play to the other senses in the imagination. As we have also seen in other chapters, this visualist bias has dramatically influenced the way in which anthropology itself has evolved. Thus, one emergent and potentially very important aid to the refocusing of the discipline lies in attending to kinds of knowledge that have proved resistant to being coded in graphic or visual ways.

Ironically, the third problem arises precisely from the work of certain scholars who have challenged the hegemony of sight in cultural studies. These academics have suggested replacing or supplementing visual models of interpretation with models based on speech and aurality. Marshall McLuhan (1962) and Walter Ong (1967), notably, argued that the sensory model of a society is determined by its technologies of communication. According to this theory, literate – particularly print-based – societies emphasize sight because of the visual nature of writing, while nonliterate societies emphasize hearing because of the auditory nature of speech. For the latter, consequently, the notion of a "world harmony" is more appropriate than that of a "world view" (Ong 1969).

Classen objects, rightly, that "while such approaches have helped prepare the ground for an anthropology of the senses by proposing alternate sensory paradigms for the study of culture, they have one major drawback from the perspective of sensory anthropology. This drawback is that they do not allow for sufficient variation in sensory models across cultures." Just as we should not typecast a society by one particular kinship mode (Salzman 1978: 66) or one particular mode of subsistence (Netting 1982: 286), so "the sensory combinatories of culture are much too complex to be stereotyped as either auditory or visual according to the dominant mode of communication." The oral culture of the Hopi of Arizona, for example, places an emphasis on sensations of vibration, while that of the Desana of Colombia highlights the symbolic importance of color (Classen 1993a: 11, 131–4). Moreover, the oral–literate model assumes that societies that give priority to sight (preeminently the West) will be analytic, while those that emphasize hearing will be synthetic. Classen rightly opposes this view – "The vision which is deemed rational and analytical in the West . . . may be associated with irrationality in another society, or with the dynamic fluidity of colour" – and calls instead for "culturally-specific investigations of particular sensory orders." In fact, as Derrida (1976) mischievously noted in his famous critique of Lévi-Strauss, the absence of what westerners mean by writing does not necessarily mean, and empirically does not mean, the absence of "graphological" representation – and hence of a visual orientation. Indeed, we might add that the whole distinction between oral and literate cultures is highly prejudicial, for it all too easily creates an aesthetic hierarchy in which "oral literature" and "oral poetry" are absorbed into the canon of western written forms as lesser or "archaic" versions. This, indeed, is what happened in the history of European folklore studies.

Such biases reflect the extraordinary persistence of evolutionism in both popular and scholarly thinking in the West. The reluctance of present-day anthropologists to examine or recognize the cultural importance of smell, taste, and touch is due not only to the relative marginalization of these senses in the modern West, but also to the racist tendencies of an earlier anthropology to associate the "lower" senses with the "lower" races. As sight, and to a lesser extent, hearing, were deemed the predominant senses of "civilized" westerners, smell, taste and touch were assumed to predominate among "primitive" non-westerners.

Many early scholars were interested in depicting the "animalistic" importance of smell, taste, and touch in non-western cultures. This trend is already evident and widespread in eighteenth-century aesthetics: "as long as man is still a savage he enjoys by means of [the] tactile senses [i.e., touch, taste and smell]," rather than through the "higher" senses of sight and hearing (Schiller 1982: 195). Where Linnaeus in the seventeenth century had associated different human populations with different forms of dress, thereby yoking together supposed levels of governability with equivalent levels of bodily restraint that were sartorially expressed (Hodgen 1964), an eighteenth-century "authority" on African slaves stated that their "faculties of smell are truly bestial, nor less their commerce with the other sexes; in these acts they are as libidinous and shameless as monkeys" (Edward Long, cited by Pieterse 1992: 41). In the early nineteenth century the natural historian Lorenz Oken postulated a sensory hierarchy of human races, with the European "eye-man" at the top, followed by the Asian "ear-man", the Native American "nose-man", the Australian "tongue-man", and the African "skin-man" (Gould 1985: 204–5). In this setting, the anthropologist Charles Myers was surprised to find when he set out to explore the importance of smell among the inhabitants of the Torres Straits at the turn of the twentieth century that "the people of the Torres Straits have much the same liking and disliking for various odours as obtains among ourselves" (Myers 1903: 185). Nonetheless, Myers suggested that the strong power of evocation which odours held for the Islanders provided "yet another expression of the high degree to which the sensory side of mental life [as opposed to the rational side] is elaborated among primitive peoples" (Myers 1903: 184).

## Senses and Systematic Knowledge

In its pursuit of a middle ground, as we have seen, anthropology has steered a course between several pairs of extremes. Among these is the opposition between generalization and particularism – Radcliffe-Brown's (1952) "nomothetic" and "idiographic" methods, respectively. The centrist position here is that of a heuristic approach – a probing that takes nothing for granted, but that remains firmly committed to ethnographic analysis. In the study of the senses, however, the discipline has perhaps been relatively slow to move away from gross generalizations, both because of the weight of its own Cartesian philosophical heritage and because the technological limits to investigation have seemed too daunting. While few would voice sentiments like those of Edward Long or even Charles Myers today, there has been little systematic field investigation of the ways in which meanings are invested in and conveyed through each of the senses. But a few pioneers have opened the way. Once free of the intellectualist prejudice against smell, taste, and touch as "animal" senses, the fact that the Sereer Ndut of Senegal have a complex olfactory vocabulary (Dupire 1987) or that the Tzotzil of Mexico describe the cosmos in thermal terms (Gossen 1974) no longer appears a telltale mark of "savagery" but bespeaks a sophisticated cultural elaboration of a specific sensory domain. Conversely, it also draws attention to the role of these neglected senses in societies, such as those of the anthropologists themselves, in which they have receded into the background – for they have not disappeared altogether. Indeed, the prominent recognition of the evocative powers of a "Proustian" moment of taste or smell awaits only the full emergence of what is already a nascent acknowledgment of the importance of evocation itself. A "smellscape" may encapsulate collective local histories as well as personal pasts. But there is a practical reluctance on the part of scholars to engage with what their recording equipment cannot fix in time and space. An anthropology that refuses to admit the significance of what it lacks the technical means to measure or describe would nevertheless be a poor empirical discipline indeed.

Parenthetically, it is worth making the point that both sight and writing are directly associated with power – and often with dangerous, alien, and intrusive power – in many societies, including those just mentioned. The anthropologist's role as a recorder of facts, and the frequent assumption that the anthropologist is engaged in espionage or police surveillance, largely spring from this perception. The ever-increasing domination of the world by a few industrial nations usually intensifies that association. Thus, it is a matter of political as well as epistemological urgency for the discipline to become much more sensitive to the messages couched in alternative sensory codes.

## Groundwork in the Field

A number of different people have been influential in the development of the anthropology of the senses (for a fuller account see Classen 1993a). Those who see themselves as developing an "anthropology of the senses" acknowledge a debt to the media specialist Marshall McLuhan (1962, 1964) and his student Walter J. Ong (1969, 1982), who as we have seen were important prototheorists of the anthropology of the senses. Ong's largely undocumented view that, "given sufficient knowledge of the sensorium exploited within a culture, one could probably define the culture as a whole in virtually all its aspects" (1967: 6), encouraged other scholars (such as Edmund Carpenter [1972, 1973]) to explore the whole of the cultural sensorium, despite Ong's own restrictive preoccupation with the oral–literate distinction.

Within anthropology, Claude Lévi-Strauss was inspired by the synaesthetic ideals of the nineteenth-century Symbolists to pioneer exploration of the sensory codes of myths. In the first volume of *Mythologiques*, in a section entitled "Fugue of the Five Senses" (Lévi-Strauss 1969), he traces how oppositions between sensations in one modality, such as hearing, may be transposed into those of another modality, such as taste, and in turn related to various conceptual oppositions – life/death or nature/culture – and to their attempted resolution in mythical thought.

Lévi-Strauss did not, however, make the transition from analyzing the sensory codes of myths to analyzing the sensory codes of culture as a whole. His interest, as Classen perceptively remarks, "lay more in tracing the operations of the mind than with analyzing the social life of the senses." Moreover, in the absence of adequate analytic and recording technology, the structuralist penchant for breaking taste down into "gustemes" – culturally significant units of taste – has not led to any new insights, although Goody's (1982) class-based analysis of the rise of elite cuisines has at least proved suggestive and constitutes prima facie evidence for the need for further research.

Influenced by both McLuhan and Lévi-Strauss, Anthony Seeger (1975, 1981) examined how the Suyá of the Mato Grosso region of Brazil classify humans, animals, and plants according to their presumed sensory traits. He found, for example, that the Suyá characterized men as pleasantly bland-smelling, while women and children were deemed to be unpleasantly strong-smelling. This characterization is due to the association of men with the valued domain of culture, and the association of women and children with the suspect domain of nature. Seeger further found the Suyá to emphasize the social importance of speaking and hearing, while linking sight with antisocial behavior such as witchcraft – an association also made in some European societies, notably those of the eastern and northern Mediterranean (the "evil eye"), and a good illustration of why the visualism of anthropology can be methodologically self-defeating. Seeger argued, by contrast, that the importance of aurality was evident in the lip and ear discs worn by Suyá men, an instance of body decoration serving to remind individuals of the proper sensory hierarchy (see further Turner 1995).

The influence of Lévi-Strauss and McLuhan can also be discerned as well in the work of the ethnomusicologist Steven Feld (1982, 1986, 1991; Keil and Feld 1994), who examined the role of sound in the classificatory thought and performance art of the Kaluli of Papua New Guinea. As with Seeger on the Suyá, Feld determined that hearing, rather than sight, is the sense of greatest cultural importance for the Kaluli, providing a model for aesthetic expression, social relations, and the orchestration of the emotions. Neither Seeger nor Feld, however, follows the Ong–McLuhan ascription of the importance of aurality to the fact that the peoples they have studied belong to nonliterate cultures. In each case, the explanation for the primacy of hearing is found within the society in question in the form of indigenous theories of meaning. It does not spring from a generalized paradigm of "oral cultures" (see also Laderman 1991; Roseman 1991; Peek 1994).

The phrase "the cultural anthropology of the senses" was coined by the historian Roy Porter in his preface to *The Foul and the Fragrant: Odor and the French Social Imagination*, by Alain Corbin (Porter 1986). The anthropology of the senses did not, however, arise as a distinct field until the late 1980s. In 1989 Paul Stoller, arguing that "anthropologists should open their senses to the worlds of their others," called for the production of "tasteful" ethnographies with vivid literary descriptions of "the smells, tastes and textures of the land, the people, and the food" (1989: 29). In order for anthropologists to achieve this, he cautioned that they must reorient their senses away from the visualism of the West and toward the sensory landscapes of other cultures (see further Fabian 1983; Tyler 1987). In his own work among the Songhay of Niger, Paul Stoller explored the importance of such aspects of Songhay culture as perfume, sauces, and music (Stoller and Olkes 1987; Stoller 1989, 1995). As regards perfume, for example, Stoller describes in rich detail a ceremony by which a Songhay woman offers up fragrance to the spirits (1989: 128–9). Such description gives the reader a taste of Songhay sensory life.

A similar descriptive or evocative approach to the anthropology of the senses has been taken by C. Nadia Seremetakis (1991; 1994) in her work on Greece. Seremetakis has employed multisensory imaging – the taste and feel of a peach, the smell and texture of grandma's dress – to bring to sensory life her memories of childhood in Greece: "The grandma sits on a wooden stool.... Her face dark, her hair tied in a bun, her hands freckled and rough. The child slips into her lap. It is time for fairy tales. Slipping into her lap is slipping into a surround of different smells and textures, sediments of her work in the fields, the kitchen, with the animals" (Seremetakis 1994: 30). Seremetakis states that her aim in undertaking an anthropology of the senses is to recover the "often hidden sensory-perceptual dispositions" of traditional societies and thereby recover the memory of culture embedded in personal recollections and material artifacts (1994: x, 9–12).

In my own work in Greece, I have suggested that the evocation of smell has the capacity to reproduce historical sequences of much longer duration. Describing the "smellscape" (Herzfeld 1991: 3–4) of a day in the life of a Cretan seaside town, I attempted to show how the phases of this sensory succession alluded to quite different moments in the town's cultural history. While such parallels should not be overdrawn, they also offer the possibility of making explicit the often intangible-seeming sources of evocation. They do not speak to, or raise, unanswerable questions of intention and motive – we may never know whether such olfactory associations of times of the day with different segments of the larger history are ever locally conceptualized as such – but they begin to suggest how and why present-day uses of particular substances may acquire the "smell of the past" and may consequently generate affective associations between images of the past and experiences in the present. In this sense, the daily sequence of smells may recapitulate, although not necessarily in their original order, a succession of smells associated today with different periods, from an agricultural past (warming olive oil), through industrialization (the smoke from motorcycles and cars), and on to tourism and the life luxurious (colognes and sun tan creams). Such smells can also become a domain for competing agencies, as when, in that same Cretan town, a housewife tries to calculate her neighbors' economic condition from the cooking smells emanating from their houses.

At the same time as Stoller, Seremetakis, and others were developing an evocative anthropology of the senses in the United States, in Canada a group of scholars was exploring how an anthropology of the senses might help to uncover the symbolic codes by which societies order and integrate the world. The members of this group, based at Concordia University in Montreal, include David Howes (1988; Howes, ed., 1991), Anthony Synnott (a sociologist) (1991, 1993), Ian Ritchie (1991) and Constance Classen (1993a, 1993b). David Howes describes the approach of this group: "The anthropology of the senses is primarily concerned with how the patterning of sense experience varies from one culture to the next in accordance with the meaning and emphasis attached to each of the senses. It is also concerned with tracing the influence such variations have on forms of social organization, conceptions of self and cosmos, the regulation of the emotions, and other domains of cultural expression . . . [It] is only by developing a rigorous awareness of the visual and textual biases of the Western episteme that we can hope to make sense of how life is lived in other cultural settings" (in Howes, ed., 1991: 4). Howes has employed this approach to examine and compare the sensory models of Dobu and Kwoma society in Papua New Guinea (Howes 1992) and to explore the elaboration of olfactory symbols and rites across cultures (Howes, ed., 1991: 128–47; Classen, Howes and Synnott 1994). In the former work Howes analyzes the social significance of diverse Melanesian sensory practices, such as the use of oil to give the body a brilliant shine, the employment of scents of mint and ginger in love magic, the bobbing motions of the dance, and the aural power of names. Throughout his writings, the emphasis is on tracing the cultural interplay of the senses, as opposed to treating a given sense in isolation.

Classen similarly examines sensory models across cultures and in western history. In *Inca Cosmology and the Human Body* (1993b), she explores the way in which the Incas ordered the cosmos and society through sensory symbols, and how this order was disrupted and reconfigured at the time of the Spanish Conquest. In *Worlds of Sense* (1993a), a key work of reference in this field, she outlined the potential breadth of a sensory approach to culture by applying it to a range of subjects, from the shifts in sensory values which have taken place at different periods of western history to the diverse sensory priorities of various non-western societies. More recently, she has examined the historical embodiment of gender ideologies through sensory codes such as the masculine gaze and the feminine touch (Classen 1998).

Those who situate their work explicitly within a self-proclaimed anthropology of the senses have cleared a great deal of ground. The real test of their contribution, however, will not be a simple proliferation of similar studies, but systematic ethnographic engagement with sensory issues as a matter of course. Some hints of things to come have already appeared. In his studies of the politics of violence in Northern Ireland, Yugoslavia and the United States, Feldman (1991, 1994) has powerfully illustrated how the senses may be employed as media for political terrorism and for "cultural anesthesia" – the use of sensory techniques and technologies to distort and efface instances of political violence. Desjarlais (1992) has explored the sensory aesthetics of pain and healing among the Tibetan Yolmo Sherpa in order to present an "embodied" analysis of emotional and physical suffering and the ritual cures used to treat them. And Taussig focuses "understanding mimesis as both the faculty of imitation and the deployment of that faculty in sensuous knowing, sensuous Othering" within European history and Latin American colonial and postcolonial culture (1993: 68). These three avenues of research illustrate the range of subject matter amenable to a sense-based investigation. It may also be that the increased attention of anthropologists to the politics of domination has begun to require that heightened sensibility to nonverbal and nonvisual encodings of experience. The very assumption that these other sensory domains are somehow closer to nature suggests that indeed their use as avenues of indoctrination, repression, and incitement in western cultures, as well as of alienation, may have been "naturalized" to the point where their ideological charge has become all but "invisible" – a telling metaphor in this context!

## Sensing the Future

The anthropology of the senses has parallels in many fields of the social sciences and humanities.[1] Historical data may prove especially important: realizing that "our own" sensorium has changed is an important first step toward decentering what we take to be, as it were, the common sensorium. In Europe, for example, the decline in the importance of the nonvisual senses from the Middle Ages to modernity (Classen 1993a) was especially accelerated by the development of photographic technology – a technology in which "anything can be separated, can be made discontinuous from anything else," as Susan Sontag (1978: 22) remarks. We can thus see the increasing predominance of the visual in the European and European-dominated world as enhancing what Don Handelman has argued is the basis of the bureaucratic state: the power to present, over and over again in the form of uniform spectacular performances, the classification of the world that best suits the interests of those in power. Reciprocally, the intended effect of visual surveillance – the (literal) supervision of the spectators – is to drown out awareness of messages encoded in the less controllable media and senses. While this development represents the increasing sophistication of visual technology, it has occurred at great conceptual as well as political cost, so that we are now obliged to make extraordinary efforts simply to apprehend all the information that flows around us but is not encoded in the print media or the ever more exigent assault of telecast images.

The broad range of applications for a sensory analysis of culture indicates that the anthropology of the senses need not be only a "subfield" within anthropology, but may provide a fruitful perspective from which to examine many different anthropological concerns. Just as the anthropology of the senses is not ahistorical, neither is it apolitical. Indeed, the study of sensory symbolism forcefully reveals the hierarchies and stereotypes through which certain social groups are invested with moral and political authority and other groups disempowered and condemned. The use of skin color as a mark of discrimination is well known in many societies. Within the West olfactory codes have served to support the "fragrant" or "inodorate" elite and stigmatize such marginal groups as Jews and blacks. Among the Dassanetch of Ethiopia similar codes serve to distinguish "superior" cattle herders from "lowly" fishermen (Classen 1993a: 79–105). And exotic cooking smells can prompt strong reactions in neighborhoods trying to remain ethnically exclusive.

Sensory codes are likewise employed across cultures to express and enforce gender divisions and hierarchies. Anthony Seeger, as noted above, for example, has shown how the Suyá negatively characterize women as "strong-smelling" in relation to "bland-smelling" men. Women are furthermore associated with disruptive touch by the Suyá while men are deemed to possess superior powers of hearing (Seeger 1981). In the West, women have traditionally been associated with the "lower" "sensual" realms of touch, taste and smell, the realms of the bedroom, the nursery and the kitchen. Men, on the other hand, have been linked with the "higher" "intellectual" realms of sight and hearing, the sensory domains of scholarship, exploration, and government (Classen 1997).

Issues of politics and gender are penetrated with sensory values, as are all issues of importance to a culture, from religious beliefs and practices to the production and exchange of goods. With regard to the latter, examples include the precautions taken by certain New

Guinea peoples to avoid offending "the sense of smell" of their garden-grown yams (Howes 1992: 289–90), the ritual exchange of differently flavored ants (representing different moieties) by the Tukano of Colombia (Reichel-Dolmatoff 1985), and the concern of western marketers to imbue their products with exactly the right look, feel and taste to appeal to (and manipulate) the consumer's sensory imagination (Howes, ed., 1996).

Classen calls for "an increase in the number of scholars pursuing a sensory approach to culture" and she deduces from the widening influence of sensory anthropology that this increase is likely to occur. While I share her enthusiasm for the topic, I would be more reassured by a pervasion of ethnographic writing by such interests. In a later chapter dealing with Aesthetics, we shall discover that this has in fact been happening. But the study of smell and taste in particular is still very undeveloped. For this, the careful scholarship of the Concordia group and others will be essential, all the more so as it increasingly intersects with a medical anthropology no longer tied to Cartesian models of causation but sensitive to the needs of an anthropology that is attuned at once, as Michael Jackson (1989) and Timothy Jenkins (1994) have especially shown, to both empirical and phenomenological concerns. The older mode of sense-less description indeed now begins to smell rather fishy.

# Part IX
# Reflexive Ethnography

*Antonius C. G. M. Robben*

The postmodern turn of the 1970s heightened anthropology's awareness of the epistemology of fieldwork relations, the constitutive position of the researcher, and the prevalent styles of ethnographic writing. Reflexivity, i.e., the conscious self-examination of the ethnographer's interpretive presuppositions, enriched fieldwork by making anthropologists pay much closer attention to the interactional processes through which they acquired, shared, and transmitted knowledge. It implied a conscious reflection on the interpretive nature of fieldwork, the construction of ethnographic authority, the interdependence of ethnographer and informant, and the involvement of the ethnographer's self in fieldwork. Reflexivity also prompted an interest in narrative styles, because if ethnography was all about intercultural and intersubjective translation and construction, then form, style, and rhetoric were of central importance (see Clifford and Marcus 1986, Geertz 1988, Ruby 1982, Trencher 2000).

In the excerpt entitled "Fieldwork and Friendship in Morocco," from his book *Reflections on Fieldwork in Morocco* (1977), Paul Rabinow emphasizes that fieldwork is an intersubjective construction which relies heavily on the encounter between ethnographer and research participant. Anthropology is therefore an interpretive science which necessarily involves an unending search for meaning. Under the influence of his mentor Clifford Geertz, Rabinow regards culture as a heterogeneous web of meanings spun by people themselves, thus requiring interpretation and translation. Such translation is especially crucial during fieldwork in foreign cultures. Local people must first make sense of their own culture and then find the right discourse to explain it to a foreign ethnographer who lacks any lived experience in the community under study. Informants situated at the margins of society, as was the case for all but one of Rabinow's research participants, are ideal cultural interpreters because they have the ability to view several worlds from across the social fences that set them apart.

Rabinow describes Sefrou, an oasis market town in the Middle Atlas Mountains of Morocco, through a patchwork of fieldwork relations with a French expatriate, five Moroccans located at different places in Sefrou society, and his host ben Mohammed, who was positioned firmly in the local community. The book describes Rabinow's progressive involvement with these seven key persons from the foreign outsider Richard to the native insider ben Mohammed, from the social periphery to the social heart of Sefrou, without privileging the cultural understanding of any one of them. Each informant revealed unique aspects of Sefrou society because of his social position, opinions, and beliefs. Nonetheless, despite this diversity among his informants, Rabinow senses one fundamental divide between him and his Moroccan informants: the different pasts, historical traditions, and religious backgrounds. This awareness opened the way for a deeper intercultural reflection based on the acceptance of a fundamental cultural difference between the American ethnographer and the Moroccan research participants.

Vincent Crapanzano takes the importance of the intersubjective construction of ethnography one step further in his book *Tuhami: Portrait of a Moroccan* (1980). Tuhami was an illiterate tilemaker who lived in a windowless shed near his kiln. He was married to a camel-footed demoness, a capricious spirit who controlled his love life. Crapanzano met Tuhami through his Berber field assistant Lhacen. The three talked every Saturday morning for three to four hours at Lhacen's house for nearly eight months. They spoke about women, saints, sexuality, illness and death, tense family relations, the Hamadsha religious brotherhood, and Tuhami's desire to free himself from the female jinn (Crapanzano 1980:4–12).

*Tuhami* is an experimental ethnography, not an ordinary life history (for a contrast, see Du Bois 1944, Lewis 1963, Radin 1963). Crapanzano does not describe Tuhami's life in a straightforward manner but is conscious of the dialogic quality of the encounter. Strongly influenced by psychoanalysis, Crapanzano has an attentive ear to the self-revelatory meanings behind Tuhami's and his own words, silences, dreams, and anxieties, as well as the transferential projection of feelings by Tuhami on Crapanzano and the countertransferential projections by Crapanzano on Tuhami provoked by the latter's transferences. The swaying between feelings of similarity and dissimilarity among fieldworker and informant demonstrates the shifting nature of the ethnographic encounter: there are no fixed social positions or distances.

The fieldwork relation between Tuhami and Crapanzano began with their radical dissimilarity. They were raised in different cultural traditions, had different outlooks on life, and held a different sense of agency. These differences were reinforced by a universal need for social classification that transforms each unique individual into a representative of a particular culture, ethnicity, religion, profession, gender, age group, morality, or personality type. These typifications intervene in any meeting between two persons, and so also between anthropologist and research participant. The awareness of this dissimilarity is unsettling and can lead to culture shock. The researcher is jolted between times of holding on tenaciously to his or her cultural identity and times when the many human similarities with the informants are emphasized. Crapanzano became worried about his personal emotional involvement with Tuhami because it hindered the dialectic of empathy and detachment advocated by conventional anthropology. His field assistant Lhacen came to the rescue. His presence gave a dynamic instability to their triadic relations, allowing for continued redefinition and renegotiation. This emotional rollercoaster led to mutual understanding and self-knowledge by detour of the Other. The ethnography about Tuhami, the bedeviled Moroccan tilemaker, thus became a narrative of the continuously changing and entwining intersubjective relations among anthropologist, field assistant, and informant in which each held back when the similarities became too uncomfortable.

In the selection "The Way Things Are Said," from her book *Deadly Words: Witchcraft in the Bocage* (1980), Jeanne Favret-Saada suggests that only a complete surrender to the discourse of her interlocutors allowed her to understand witchcraft in western France. People in the Bocage suspect witchcraft after suffering a series of unusual misfortunes. The victim approaches doctors, therapists, priests, and eventually unwitchers who, each from their own branch of knowledge, interpret the tragic events and recommend a cure. The unwitcher acknowledges witchcraft as the principal cause, traces the victim's soft spots, and identifies the witch through harmful words uttered in the past. The unwitcher turns those deadly words around, directs them against the witch, positions himself or herself between witch and victim, and seals the victim's weak spots.

Favret-Saada argues that the only way to understand witchcraft in the Bocage was to accept the position of an unwitcher. A detached objectivist stance (as taken by French folklorists), an apprenticeship (recommended by anthropologists), a verbal translation (employed by Rabinow), or even a dialogic exchange (practiced by Crapanzano), would all fall short. Favret-Saada was faced with a body of literature that dismissed Bocage witchcraft as a superstitious belief, an exotic anachronism in modern France, an example of the peasants' pre-logical backwardness, and their inability to distinguish cause and effect. The classic ethnography *Witchcraft, Oracles, and Magic among the Azande* comes to mind but, unlike Evans-Pritchard (1968), Favret-Saada was not out to prove that the Bocage peasants were in full possession of their rational faculties, despite their belief in witchcraft. Instead, she argues, witchcraft did not exist in some esoteric knowledge or secret spells but in the harmful words spoken by a witch or an unwitcher. There was no template beyond the words themselves. Words were power. Therefore, knowledge about witchcraft was powerful and could not be shared with a neutral, disinterested fieldworker because such knowledge implicated the researcher in the conflictive world itself (see also Ashforth 2000, Stoller and Olkes 1987). So, Favret-Saada had to demonstrate that she had been "caught" as either a bewitched victim or as an unwitcher to elicit the discourse of witchcraft victims and unwitchers.

In his article *On Ethnographic Authority*, published originally in 1983, James Clifford (1988:21–54) analyzes the development of ethnography from experiential to interpretive, dialogical, and polyphonic modes of authority. He shows how reflexive fieldwork and ethnography arose from a critique of earlier fieldwork practices and forms of textual representation. These older practices did not pay attention to the power relation between fieldworker and informant, the intersubjective construction of field notes, and the translation of experience and dialog into authoritative texts. Clifford credits Bronislaw Malinowski (see Chapter 3) with establishing this mode of fieldwork authority founded on the ethnographer's year-long experience in the culture under study. Anthropologists became trained scientists who resided among their research participants, learned their language, participated in and observed their everyday and not so everyday lives, wrote ethnographies based on objective data, and were informed by theoretical questions. Participant observation became the queen of anthropological field methods because of its combination of empathy and detachment which allowed an ethnographer to alternate the native's point of view with an objective stance. However, behind this seemingly controlled insider–outsider dialectic, there was a continuous negotiation between fieldworker and informants about cultural representation which was shielded by the ethnographer's rhetorical authority.

These objectified ethnographies, argues Clifford, came under increasing attack in the 1960s because they ignored the voices of the informants and the subjective experiences of the fieldworkers. Influenced by a range of theories, notably hermeneutics, critical theory, feminist theory, ethnomethodology, and symbolic interactionism (see Alvesson and Sköldberg 2000), anthropologists began to emphasize the importance of meaning, interpretation, and intersubjectivity. An analogy was drawn between cultures and texts because grasping the multiple meanings of actions and practices, just as understanding the many meanings of words and sentences, required interpretation. The most direct influence of this approach on ethnographic fieldwork and writing was a commitment to so-called thick description. Thick description involved an interpretation of the intertwined cultural constructions and social discourses of actors. This exegesis promised to result in a richly textured, multi-layered and multi-voiced ethnography (Geertz 1973). Such interpretation became achieved methodologically by way of a hermeneutic circle in which understanding was advanced in circular, rather than linear, ways between part and whole, and back again. Clifford Geertz (1973) and David Schneider (1968) were at the forefront of this movement, which became known as interpretive anthropology, while the French philosopher Paul Ricoeur had an important theoretical impact (Ricoeur 1974, see also Rabinow and Sullivan 1979). The emphasis on textuality and meaning soon provoked a critique from within interpretive circles. Kevin Dwyer (1987) argued that Geertz and his supporters, including Rabinow in his *Reflections on Fieldwork in Morocco*, failed to bridge the gap between Self and Other because the anthropologist remained in the superior position as the observer and interpreter of a cultural reality beyond the fieldwork encounter. Dwyer advocated a dialogical approach that acknowledged the processual construction of field notes and the interdependence of ethnographer and research participant (see also Dumont 1978, Handelman 1993). Dwyer accomplished this goal in his own ethnographic work on Morocco by presenting verbatim the unfolding dialogs with his key informant. This shift from meaning and interpretation to discourse and dialog evolved into a growing interest in power and polyphony. Under the principal influence of the Russian literary critic Mikhail Bakhtin (1981), there arose the awareness that culture was constructed by multiple voices of different power and influence, and that this heteroglossia should be expressed in the ethnography.

Concern for meaning, interpretation, subjectivity, intersubjectivity, thick description, dialogics, and polyphony found its way into standard fieldwork practice and social science methodology (Alvesson and Sköldberg 2000; for a critique, see Salzman 2002). This attention might not be as explicit or prominent in most ethnographies today as it was in the 1970s and 1980s, but anthropologists continue to realize that they do not simply observe and describe cultures but that they interpret and inscribe them, and that fieldwork implicates the selves of ethnographers and local research participants in constitutive ways.

# 32

# Fieldwork and Friendship in Morocco

*Paul Rabinow*

## 8 Friendship

Driss ben Mohammed, a jovial, portly, and even-tempered young man, had consistently refused to work as an informant. Over the course of my stay we had come to know each other casually, as time permitted, almost accidentally. Gradually, a certain trust had flowered between us. At its root, I think, was an awareness of our differences and a mutual respect.

Ben Mohammed was not afraid of me (as many other villagers were), nor did he have hesitations about associating with Europeans (although he had had almost no personal contact with them), nor did he seek to profit from my presence (he refused most gifts). Simply, he was my host and treated me with the respect which is supposed to be reserved for a guest, even one who stayed as long as I did.

To be friends, according to Aristotle, two people "must be mutually recognized as bearing goodwill and wishing well to each other . . . either because of utility, pleasure, or good. . . . That type of friendship stemming from the good is best because . . . that which is good without qualification is also pleasant, but such friendships require time and familiarity . . . a wish for friendship may arise quickly but friendship does not."[1]

As time wore on and my friendship with ben Mohammed deepened, I was learning more and more from him. During the last months of fieldwork, when he was home from school and we could spend many of the hot hours together, the field experience, now nearing its completion, reached a new emotional and intellectual depth. Casually, without plan or schedule, just walking around the fields, ripe with grain or muddy from the irrigation water in the truck gardens, we had a meandering series of conversations. Ben Mohammed's initial refusal of informant status set up the possibility of another type of communication. But clearly our communication would not have been possible without those more regularized and disciplined relationships I had had with others. Partly in reaction to the professional situation, we had slipped into a more unguarded and relaxed course over the months.

Although we talked of many things, perhaps the most significant set of discussions turned on our relations to our separate traditions. It would have been almost impossible to have had such conversations with either Ali or Malik, enmeshed as they were in the web of their own local world. Nor, for that matter, would it have been possible with many of the Frenchified Moroccan intellectuals; half torn out of their own ill-understood traditions, and

Paul Rabinow, "Friendship," pp. 142–9, and "Conclusion," pp. 150–62 from Paul Rabinow, *Reflections on Fieldwork in Morocco* (Berkeley: University of California Press, 1997). Copyright © 1997 by The Regents of the University of California.

afflicted with a heightened and unhappy self-consciousness, they would be unable to bridge the gap either way. Ben Mohammed, in his own modest way, was also an intellectual, but he was one of those who still looked to Fez rather than Paris for his inspiration. This provided a crucial space between us.

The fundamental tenet of Islam, for ben Mohammed, was that all believers are equal before Allah, even though pride, egoism, and ignorance obscure this fact. Very, very, few people, in his view, actually believe in Islam. Most take only a "narrow" view: they think that if they merely follow the basic prescriptions then they are Muslims. Ben Mohammed emphatically disagreed. If belief in the equality among believers and in submission to Allah is not in your heart, and does not inform your actions, then prayer or even the pilgrimage to Mecca counts for nothing. *Niya*, or intention, is the key. You might be able to fool your neighbors by shallow adherence to externals, but you would not fool Allah. Today, for ben Mohammed, the true Muslim is mistrusted in the Islamic world. People interpret generosity and submission as weakness or foolishness. Boasting, hypocrisy, quarreling, and fighting prevail because people do not truly understand and accept the wisdom of Islam.

He brought up the example of Sidi Lahcen. Most of the saint's descendants knew little if anything about his teachings or his "path." They are ignorant. Yet they feel superior to other Muslims because they are descended from a famous saint and can lay claim to his *baraka*, to his holiness. But if they would read the books which their patron saint wrote, they would see that Sidi Lahcen himself fought against such vanity. He had preached submission to Allah alone. The only true nobles in Islam were those who lived exemplary lives and followed Allah. Sidi Lahcen's descendants, however, by relying on his spiritual strength, had lost their own. They think that their genealogical connections alone should command respect; Sidi Lahcen would have disagreed.

Ben Mohammed was striving, he said, to follow the path of Sidi Lahcen. But it posed specific problems for him. His father, whom he respected, vehemently opposed his "reformist" interpretations. This would not change ben Mohammed's personal beliefs, but it was his duty to respect those of his father. Ben Mohammed knew that his father, an old man set in his ways, was not about to alter his views. Actually, Sidi Lahcen himself had taken a parallel stance in his own age: popular religion was to be combatted in its excesses but tolerated for its piety.

For ben Mohammed the tensions of his world view turned on these two Moroccan alternatives. Morocco's future was far from bright. He would have great difficulty in finding the kind of work and life he desired. His expectations were geared to those of his country. But he also knew that the symbols and guides for the future would have to be drawn from Morocco's tradition. Moroccans could not ignore the West. This attitude required borrowing, integrating, and eliminating certain archaic and oppressive practices, but it did not mean merely imitating the West; and most important of all, it did not require the abandonment of Islam.

With most informants, I would have stopped at this point of generality. But with ben Mohammed I felt I could proceed further. Throughout my stay in Morocco I had noticed that black was negatively valued in a variety of ways. In the broadest terms, white was generally equated with good and black with evil. Malik in particular seemed consistently concerned about color distinctions and their symbolism. Black was bad, according to his view, a color worthy of a dog. The lighter you are the better you are, the more you shone in the eyes of Allah. Malik was joking one day about a very poor villager. He said the man was so poor he would have to marry a black. Malik's new-born daughter, he pointed out innumerable times, was very white. When I showed him pictures of America he always made a point of saying that he could not tell if the blacks were men or women. He had been very upset when he found out that one of his favorite songs on the radio was by a black group. After that, he was careful to find out the color of a singer before offering any opinions on the music. Malik was not at all timid about discussing this symbolism. He was quite sure of himself; his source of ultimate authority was the Koran.

Throughout my stay I had been the dutiful anthropologist and noted down his comments, refraining from publicly reacting. But toward the end I let myself be affected by them more, and they really began to rankle. I am light in complexion with blue eyes and light brown hair. I was tempted many times to ask Malik, who has a dark skin tone, kinky hair, and large lips, if he thought this made me superior to him, but I never did. There was no point in confronting him.

Ben Mohammed was a different story. When I finally approached him about my feelings on the matter, he was quite lucid. We were sitting on a hillside, under some fig trees, overlooking the Brueghelesque field below, amiably passing a hot, cloudless summer afternoon. I cautiously began to unburden myself about Malik. Again ben Mohammed straddled the cultural divide rather artfully. He fully agreed that the downgrading of blacks was a bad thing. It was incumbent on Muslims to fight racism in all its forms. There was no ambiguity on that point. But, such symbolism was indeed in the Koran. Most people rely on custom and not on their own intelligence. Malik was a peasant and could not be expected to know any better. He had been raised with these aphorisms, and lived with them, and he was not going to easily rid himself of such a bias.

He cautioned me, however, not to confuse Malik's views with the kind of racism he knew existed in America or Europe. Although Malik expressed anti-black sentiments, no Moroccan would ever keep someone out of a hotel or a job because of his skin color. Cultures were different, ben Mohammed was saying. Even when they say the same thing, an expression can mean something entirely different when it is played out in society. Be careful about your judgments. I agreed.

Yet, there was one further question to ask: Are we all equal, ben Mohammed? Or are Muslims superior? He became flustered. Here there was no possibility of reformist interpretation or compromise. The answer was no, we are not equal. All Muslims, even the most unworthy and reprehensible, and we named a few we both knew, are superior to all non-Muslims. That was Allah's will. The division of the world into Muslim and non-Muslim was *the* fundamental cultural distinction, the Archimedean point from which all else turned. This was ultimately what separated us. But, as Aristotle points out, "in a friendship based on virtue, complaints do not arise, but the purpose of the doer is a sort of measure; for in purpose lies the essential element of virtue and character . . . friendship asks a man to do what he can, not what is proportional to the merits of the case, since that can not always be done. . . ."[2]

The lessons of tolerance and self-acceptance which ben Mohammed had been teaching me during the past months held sway. I had a strong sense of being American. I knew it was time to leave Morocco.

The "revolution" had occurred during my absence (1968–9). My friends from Chicago, many of them now living in New York, were fervently and unabashedly "political" when I returned. New York, where I had grown up, looked the same as when I had left it. But the city and my friends were now more impenetrable to me than ben Mohammed. The whole revery of future *communitas* which had sustained me through months of loneliness refused to actualize itself upon my return. I adopted a stance of passivity waiting for it to appear. Perhaps the most bizarre dimension of my return was the fact that my friends were now seemingly preoccupied with the Third World; at least the phrase had an obligatory place in their discourse. I had just been in the Third World with a vengeance. Yet this Third World which they so avidly portrayed bore no obvious relation to my experiences. Initially when I pointed this out, I was politely ignored. When I persisted it was suggested that I was perhaps a bit reactionary. The maze of slightly blurred nuance, that feeling of barely grasped meanings which had been my constant companion in Morocco overtook me once again. But now I was home.

Over the next several years other activities absorbed me, writing and teaching among them. Writing this book seems to have enabled me to go on to another type of fieldwork, to begin again on a different terrain.

Trinh Van Du entered the room carrying a dozen roses for our hostess. He was perhaps five feet tall and drew attention to this immediately by announcing that although he was thirty-three, Americans often mistook him for fifteen. The first hour or so of introductory chat was a bit stilted, but Du managed to include six or seven references to Ho Chi Minh along with the fact that he had been in the United States almost twelve years, doing odd jobs and teaching, for a time, at the Monterey Army language school. Things warmed up enormously when we switched from politics and credentials to language and culture. Yes, he would love to teach us Vietnamese and introduce us to Vietnamese literature, particularly poetry. The Hue dialect, his own, is the most poetic (as are its women), the Saigon dialect the most sing-song like Chinese, and the Hanoi the most precise and clear. But all Vietnamese read the same language and all love the *Tale of Kieu*. He would recite it for us in all three dialects and we would choose the one we liked best. Leaping up, filled with sparkle, yet almost solemn, he recited the first verses of the famous nineteenth-century poem, three times.

## Conclusion

Culture is interpretation. The 'facts' of anthropology, the material which the anthropologist has gone to the field to find, are already themselves interpretations. The baseline data is already culturally mediated by the people whose culture we, as anthropologists, have come to explore. Facts are made – the word comes from the Latin *factum*, "made" – and the facts we interpret are made and remade. Therefore they cannot be collected as if they were rocks, picked up and put into cartons and shipped home to be analyzed in the laboratory.

Culture in all of its manifestations is overdetermined. It does not present itself neutrally or with one voice. Every cultural fact can be interpreted in many ways, both by the anthropologist and by his subjects. The scientific revolutions which established these parameters at the turn of the current century have been largely ignored in anthropology. Frederic Jameson's reference to the paradigm shift in linguistics applies to anthropology as well. He notes "a movement from a substantive way of thinking to a relational one.... Difficulties arose from terms which tried to name substances or objects... while linguistics was a science characterized by the absence of such substances.... There are first of all points of view... with whose help you then subsequently create your objects."[3]

The fact that all cultural facts are interpretations, and multivocal ones at that, is true both for the anthropologist and for his informant, the Other with whom he works. His informant – and the word is accurate – must interpret his own culture and that of the anthropologist. The same holds for the anthropologist. Both live in rich, partially integrated, ongoing life worlds. They are, however, not the same. Nor is there any mechanical and easy means of translation from one set of experiences to the other. That problem and the process of translation, therefore, become one of the central arts and crucial tasks of fieldwork. It should be clear that the view of the "primitive" as a creature living by rigid rules, in total harmony with his environment, and essentially not cursed with a glimmer of self-consciousness, is a set of complex cultural projections. There is no "primitive." There are other men, living other lives.

Anthropology is an interpretive science. Its object of study, humanity encountered as Other, is on the same epistemological level as it is. Both the anthropologist and his informants live in a culturally mediated world, caught up in "webs of signification" they themselves have spun. This is the ground of anthropology; there is no privileged position, no absolute perspective, and no valid way to eliminate consciousness from our activities or those of others. This central fact can be avoided by pretending it does not exist. Both sides can be frozen. We can pretend that we are neutral scientists collecting unambiguous data and that the people we are studying are living amid various unconscious systems of determining forces of which they have no clue and to which only we have the key. But it is only pretense.

Anthropological facts are cross-cultural, because they are made across cultural boundaries. They exist as lived experience, but they are made into facts during the process of questioning, observing, and experiencing – which both the anthropologist and the people with whom he lives engage in. This means that the informant must first learn to explicate his own culture, to become self-conscious about it and begin to objectify his own life-world. He must then learn to "present" it to the anthropologist, to an outsider who by definition does not understand even the most obvious things. This presentation by the informant is defined, therefore, by being in a mode of externality. The informant is asked in innumerable ways to think about particular aspects of his own world, and he must then learn to construct ways to present this newly focused-on object to someone who is outside his culture, who shares few of his assumptions, and whose purpose and procedures are opaque. Thus when a Moroccan describes his lineage structure to an anthropologist, he must do several things. He must first become self-reflective and self-conscious about certain aspects of his life which he had previously taken largely for granted. Once he arrives at some understanding of what the anthropologist is driving at, thinks about that subject matter, and comes to a conclusion (all of which can occur in a matter of seconds, of course, and is not in itself a theoretical process), the informant must then figure out how to present this information to the anthropologist, an outsider who is by definition external to his usual life-world.

This creates the beginnings of a hybrid, cross-cultural object or product. During the period of fieldwork a system of shared symbols must be developed if this process of object formation – through self-reflection, self-objectification, presentation, and further explication – is to continue. Particularly in its early stages when there is little common experience, understanding, or language to fall back on, this is a very difficult and trying process; the ground is just not there. Things become more secure as this liminal world is mutually constructed but, by definition, it never really loses its quality of externality. This externality, however, is a moving ratio. It is external both for the anthropologist (it is not his own lifeworld) and for the informants, who gradually learn to inform. The present somewhat nasty connotations of the word do apply at times, but so does its older root sense "to give form to, to be the formative principle of, to animate." What is given form is this communication. The informant gives external form to his own experiences, by presenting them to meet the anthropologist's questions, to the extent that he can interpret them.

This informing, however, goes on not in a laboratory but in interpersonal interaction. It is intersubjective, between subjects. At best, it is partial and thin. The depth and scope of the culture that has been constructed is often woefully inadequate when measured against people interacting and carrying on their daily rounds in the everyday world. Anthropology is not a set of questionnaires which are handed over, filled out, and handed back. Most of the anthropologist's time is spent sitting around waiting for informants, doing errands, drinking tea, taking genealogies, mediating fights, being pestered for rides, and vainly attempting small talk – all in someone else's culture. The inadequacy of one's comprehension is incessantly brought to the surface and publicly displayed.

Interruptions and eruptions mock the fieldworker and his inquiry; more accurately, they may be said to inform his inquiry, to be an essential part of it. The constant breakdown, it seems to me, is not just an annoying accident but a core aspect of this type of inquiry. Later I became increasingly aware that these ruptures of communication were highly revealing, and often proved to be turning points. At the time, however, they seemed only to represent our frustration. Etymology comes to the rescue again: *e-ruption*, a breaking out, and *inter-ruption*, a breaking in, of this liminal culture through which we were trying to communicate.

Whenever these breaks occurred – and I have described several of the most important ones earlier – the cycle began again. This cross-cultural communication and interaction all took on a new content, often a new depth. The groundwork we had laid often seemed to fall away from under us and we scrambled somewhere else. More had been incorporated, more could be taken for granted, more could be shared. This is a moving ratio and one which never reaches identity, far from it. But there is movement, there is change, there is informing.

Fieldwork, then, is a process of intersubjective construction of liminal modes of communication. Intersubjective means literally more than one subject, but being situated neither quite here nor quite there, the subjects involved do not share a common set of assumptions, experiences, or traditions. Their construction is a public process. Most of this book has focused on these objects which my Moroccan friends and I constructed between us, over time, in order to communicate. That the communication was often painstaking and partial is a central theme. That it was not totally opaque is an equally important theme. It is the dialectic between these poles, ever repeated, never quite the same, which constitutes fieldwork.

Summing up, then, we can say the following.

The first person with whom I had any sustained contact was the Frenchman Maurice Richard. Staying at his hotel was an obligatory first step for Europeans entering into Sefrou (although recently the Moroccan government has opened a luxury hotel). Knowing that his clientele will not be with him long, Richard has developed a persona of cheerful good will, which becomes less and less convincing as he becomes more isolated. The contact with Richard was immediate. There was no language barrier. He was eager to talk. Being an outsider to all of the other Sefrou groups, he had interesting stereotypes of each, which he was more than willing to exchange for a receptive smile. His very accessibility, however, was also revealing of his limitations. He provided entry only to the past, to the last days of colonialism. He was located on the very edge of Sefrou society, its most external point. His corner was easily accessible, but it revealed only the fringes of Moroccan society. Although this subject provided ample material for an inquiry, and was in fact in the process of disappearing forever, my project led me in other directions.

Ibrahim was on the other side of the buffer zone between the French and the Moroccan societies. He had matured during the waning days of the Protectorate and made his career by artfully straddling the line between communities without any confusion as to which side of the line he was on. His speciality was presenting goods and services for external consumption. They were carefully packaged. He was a guide along the main thoroughfares of Sefrou society. His tour was quite helpful for understanding the Ville Nouvelle, but his aid stopped at the walls of the medina. Despite his caution, the first breakthroughs of Otherness occurred with Ibrahim. This professional of the external was, nonetheless, a Moroccan.

My guide through the medina of Sefrou and the transitional zones of Moroccan culture was Ali. My contact with him was the first major step toward a more intimate relationship with Sefrou. He was a floating figure within his own society, living a hand-to-mouth existence in the city. He was a patient, curious, highly imaginative, adventurous, sensuous, and relentlessly perceptive person. My orientation to Moroccan culture as immediacy, as lived experience, came from my friendship with Ali. He had rejected a certain way of life, but not other Moroccan alternatives. He was acerbic and direct in his criticisms of village ways, but they were insider's jibes.

Ali was also limited by his strengths. Because of his demeanor and antagonism he had almost become an outcast in the village. The insights and orientations which he continued to provide for me throughout the field experience were invaluable. He knowingly and adroitly used the villagers' inhibitions and vulnerabilities against them. Ali was an insider's outsider. His unique vantage point and provocative attitude periodically rescued me from impasses and collective resistance. Ali was, however, now outside village affairs, basically out of touch. He provided little help on the day-to-day level, but could be relied on for vital aid.

So, just as Richard was situated between the two French communities, and Ibrahim between the French and local Moroccan Ville Nouvelle groups, so Ali was situated between the floating population of the medina and his natal village of saint's descendants. All were marginal, all provided help in making transitions from group to group, site to site.

Within Sidi Lahcen itself, the situation became more tightly controlled. The community tacitly (and in some cases explicitly) attempted to situate the anthropologist and thereby control him. The first two young men with whom I worked exemplify this. Mekki, my first informant, literally pushed on me by the villagers, was from Ali's sub-lineage. Not being burdened with family or work obligations, he eagerly sought what to others was a mixed blessing. Unfortunately, he lacked both intelligence and the imaginative ability to objectify his own life-world and then present it to a foreigner. This was an insurmountable handicap. Rashid, my second informant, was everything that Mekki was not; that was his problem. He was imaginative, energetic, curious, intelligent, and was floating, like Ali, except that Rashid's experience was essentially limited to village life. He could have been and was (from time to time) an extremely important informant. But, again like Ali, he aroused strong community disapproval. Rashid's tongue was feared. Everyone, including his father, sought to silence him. Unsure about my presence in the village, they wanted some control over the information I was receiving. Rashid knew a great deal and was eager to convey it. As the Moroccan proverb goes, Those who have no shame, do as they please. And so those with no internal sense of appropriate behavior must be controlled by force. Rashid, unlike Ali, had no power base, no alternative cards to play. In general, he was forced to accede to the community's injunctions, yet he enjoyed violating them whenever the opportunity presented itself.

Malik offered an excellent compromise, both for me and for the community. I had forced my way into Sidi Lahcen, after all, and the villagers feared that ultimately I had come to subvert their religion. Therefore, it was appropriate that the man who became my central informant was situated on the edge of the most respected of the saintly sub-lineages. This group had a very high rate of endogamous marriage. Malik's father, however, had married a woman not only from outside the sub-lineage but from outside the village. Consequently, as closely attached to this core group as he was emotionally, he was structurally somewhat on its edge, and he overcompensated for it.

He was the perfect representative of orthodoxy. He was proud of his tradition but he had failed to find a traditional role for himself. Impatient with the position of *fqi*, he was stymied in pursuing his own grandiose self-image. A conservative, he lacked institutions to defend. He proved to be the perfect community choice. The elders of his sub-lineage sanctioned his involvement and so did Sergeant Larawi, the most powerful man in the village. They knew they could trust Malik.

Malik, like Ibrahim, was self-controlled, orderly, and reserved. But unlike Ibrahim, he had not made a career of external relations. Malik had remained within the rural world. Malik would have liked to be the internal counterpart of Ibrahim. But no such role existed. He had to improvise as he went along. His "impression management," however, was in constant tension with the inputs of Ali, Rashid, and others. Malik attempted to steer cautiously around sensitive areas. Once challenged, he would yield, but after the early going, he would rarely initiate. As we proceeded, Malik became more dependent on me than I was on him. This helps explain his lack of sustained resistance on sensitive areas; Ibrahim, no doubt, would not have backed down so readily.

Many of the political dimensions of the informant relationship were obviated by Driss ben Mohammed's steadfast adherence to the role of host. This eventually established the grounds for a dialogue. Ben Mohammed was internal to the Moroccan tradition. He looked back to his forefather, the seventeenth-century saint, for guidance in the modern world. He maintained a belief in the ultimate and unconditional superiority of Islam.

This absolute difference which separated us was openly acknowledged only at the end of my stay. We had become friends, we had shown each other mutual respect and trust. The limits of the situation were not obscured for either of us. I was for him a rich member of a dominant civilization about which he had the profoundest reservations. To me, he was struggling to revive a cultural universe which I no longer inhabited and could not ultimately support. But our friendship tempered our differences. Here we had come full circle. There were now two subjects facing each other. Each was the product of an historical tradition which situated and conditioned him. Each was aware of a profound crisis within that tradition but still looked back to it for renewal and solace. We were profoundly Other to each other.

That I would journey to Morocco to confront Otherness and myself was typical of my culture (or the parts of it I could accept). That ben Mohammed would enter into this sort of dialogue without self-denigration was impressive. My restless and scientifically cloaked wanderings brought me to this mountain village in Morocco. Ben Mohammed sought the wisdom of the reformist saint, yet was willing, even eager, to tell me about him. Through mutual confrontation of our own situations we did establish contact. But this also highlighted our fundamental Otherness. What separated us was fundamentally our past. I could understand ben Mohammed only to the extent that he could understand me – that is to say, partially. He did not live in a crystalline world of immutable Otherness any more than I did. He grew up in an historical situation which provided him with meaningful but only partially satisfactory interpretations of his world, as did I. Our Otherness was not an ineffable essence, but rather the sum of different historical experiences. Different webs of signification separated us, but these webs were now at least partially intertwined. But a dialogue was only possible when we recognized our differences, when we remained critically loyal to the symbols which our traditions had given us. By so doing, we began a process of change.

# 33

# Tuhami: Portrait of a Moroccan

## *Vincent Crapanzano*

There had been something desperate in Tuhami's last words: "Even my neighbors ask the same thing: why I don't get married. It irritates me. It is up to Allah. There is nothing I can do about it." I knew that I could no longer maintain ethnographic distance. Tuhami's appeal was too great, and I myself too much of an activist, to accept what I understood then to be his passivity before forces externalized in 'A'isha Qandisha, the saints, and ultimately Allah. I was a doer, and I came from a culture of doers – a culture that could accept as reasonable the maxim "Nothing is impossible." I had learned this maxim as a child, and others like it ("Try, try, and try again; and if at first you don't succeed, try, try again"; "Don't give up the ship"; etc.). I had read modern versions of Horatio Alger and understood self-help and Nietzsche's notion of self-overcoming. I was angered by Tuhami's passivity before the demonic, his fatalism, his submission to Allah's will, to what was "written." His beliefs, I was convinced at the moment, held him back; they hindered his self-expression and impeded his self-reliance; they precluded the possibility of self-overcoming. They were a sanctioned ground for rationalization. There was, I realized, a limit to my relativism. I became a curer. I was leaving in two weeks. I was anxious for Tuhami. My wife and I even talked about staying longer, but that was impossible.

I write: "I became a curer." In fact, I had already become a curer. This is evident in my notes, and the reader is certainly aware of the way in which Tuhami and I both negotiated our exchange into a therapeutic one. Tuhami provided me (and I feel the consequence of this even now, as I write) with the possibility for maneuver, manipulation, and cure, with the occasion for a vicarious participation that was perhaps not so vicarious after all. (My own father died when I was young, though not so young as Tuhami had been.) And he provided me with the occasion for that romance of adventure and exploration that was, I imagine, what drew me, and continues to draw me to anthropology and field research. The moment I have cited as marking my change of status is for me one of those fatal instants that mark a moment in what has been recollected and will be repeated. Repetition and recollection, Kierkegaard (1964, p. 33) tells us, "are the same movement, only in opposite directions; for what is recollected has been, is repeated backwards, whereas repetition properly so called is recollected forwards."

"Ethnographic distance" was a rhetorical device that enabled me both to mask my position and to rationalize it. Certainly, as I look back over my meetings with Tuhami, it is clear that the "distance" between us – and the intensity of our involvement – varied considerably, despite even the presence of Lhacen, my field

Vincent Crapanzano, pp. 133–52 from Vincent Crapanzano, *Tuhami: Portrait of a Moroccan* (Chicago: University of Chicago Press, 1980). © 1980 by The University of Chicago.

assistant. The methodological strategies of the fieldworker are, as George Devereux (1967) has observed, frequently the result of his anxieties. Indeed, so, frequently, are his epistemological concerns.

I had arrived in Morocco as a stranger with a determination to understand at least some small facet of the lives of the people that, for whatever reason, I found intriguing and somehow significant. I met many Moroccans whom I found uninteresting and unlikable, and I met some whom I found interesting and unlikable. I was also fortunate enough to meet a few who were both interesting and likable, and Tuhami was among these. He was, within his terms, giving; and I, with an avariciousness supported by my science, was willing to receive. I wanted to possess everything that Tuhami knew and could tell me – and even more. I wanted to know him completely. I have always been fascinated by d'Annunzio's portrayal, in *The Triumph of Death* (1900), of hero and heroine's obsessive desire to know each other fully. The presumption that such knowledge can be achieved rests either on the belief in total sexual possession – a possession that ends up, as d'Annunzio understood, in total extinction – or on the reduction of the Other to that which is completely graspable: the specimen. The one, the goal of passion, and the other, the product of science, are not in fact so easily separable. Both are of course illusory.

"The picture of another man that a man gains through personal contact with him," Georg Simmel observed in 1908, "is based on certain distortions."

> These are not simple mistakes resulting from incomplete experience, defective vision, or sympathetic or anti-pathetic prejudices. They are fundamental changes in the quality of the actual object perceived. (Simmel, 1965 ed., p. 342)

These qualitative distortions result, if I understand Simmel correctly, from generalizations in some measure of the Other qua individual.

> In order to know a man, we see him not in terms of his pure individuality, but carried, lifted up or lowered, by the general type under which we classify him. Even when this transformation from the singular to the typical is so imperceptible that we cannot recognize it immediately; even when all the characterological concepts such as "moral" or "immoral," "free" or "unfree," "lordly" or "slavish" and so on, clearly appear inadequate, we privately persist in labeling a man according to an unverbalized type, a type which does not coincide with his pure, individual being. (Ibid., p. 343)

These generalizations, which result from our fragmentary knowledge of the Other, both detract and supplement his individuality. The individual is, for Simmel, always in tension with the a priori, operative categories through which he is pictured.[1] These categories, or typifications, as we would call them today, include, ironically, the "singular individual." Although it might well appear that the apprehension of this singular individual should form the basis for correct relations with the Other, such an apprehension would itself be a distortion of the individual as a social being. The very alterations and new formations that preclude the ideal knowledge of the Other – the qualitative distortions – "are, actually, the conditions which make possible the sort of relations we call social" (ibid., p. 345). "The individual is contained in sociation and, at the same time, finds himself confronted by it" (ibid., p. 350).

I quote Simmel at some length here because, in his roughshod Kantian way, he struggled in his Introduction to *Soziologie: Untersuchungen über die Formen der Vergesellschaftung* with the problem of the knowledge of the Other with a passion and a freshness that have been lost in later theories of labeling, typification, and alienation. His argument, reformulated in hermeneutical terms, is that we come to know another individual with a certain foreknowledge[2] – a foreknowledge that is sanctioned by social convention (i.e., tradition), that fills in the "incompleteness" of that individual's presentation, and that qualitatively modifies the individual as a subject of perception. Such knowledge, for Simmel, demands similarity, "because we cannot fully represent to ourselves an individuality which deviates from our own" (1965, p. 343). It also demands dissimilarity, "in order to gain distance and objectivity." The knowledge of the Other requires, however paradoxically, both similarity and dissimilarity:

> Nevertheless, *perfect* cognition presupposes perfect identity. It seems, however, that every individual has in himself a core of individuality which cannot be re-created by anybody else whose core differs from his own. And the challenge to re-create is logically incompatible with psychological distance and objective judgment, which are also bases for representing another. (Ibid., p. 343)

Simmel is led to conclude: "We cannot know completely the individuality of another." And this fact, however described, is, I believe, essential to the understanding of all social encounters. It is perhaps no accident that C. G. Jung (1961) concluded his autobiographical meditation with a personal anagram that he left unexplained. It is also no accident that Simmel himself wrote about the lie and the secret.

In most of our ordinary encounters, the assumptions of similarity and dissimilarity are accepted without question. It is only in the exceptional encounter that they come into question. And there are certain moments, but only certain moments, in the ethnographic encounter when they are indeed questioned. As I have written earlier, in most social encounters we assume that what a person says he experienced he did in fact experience; we assume that we can know another person, at least up to a certain point; and we assume the transparency of language. Such assumptions are, I believe, mystifications – at least from a strict epistemological point of view – that are necessary for social existence. We must assume knowledge not only of the Other as an external actor – a soulless marionette – but as an experiencing individual with whom we are in as-sociation. We are rarely pushed, except in the height of romantic passion or in the biographical enterprise, to assume, or even to desire, as Simmel would put it, "perfect cognition" of the Other. (To be sure, such perfect cognition occasionally becomes an ideal within those utopian movements that Victor Turner [1974] has characterized as *communitas*, but such movements are for the most part characterized by an extreme psychological naïveté.) The push toward such cognition – fusion, really – is motivated, I suggest, by the fear of the very opposite – solipsistic miscognition or de-fusion – which the psychoanalyst would associate with separation.

The ethnographer's entry into the field is always a separation from his world of primary reference – the world through which he obtains, and maintains, his sense of self and his sense of reality. He is suddenly confronted with the possibility of Otherness, and his immediate response to this Otherness is to seek both the security of the similar and the distance and objectivity of the dissimilar. No longer bound to the conventions of similarity and dissimilarity that obtain within this own world of reference, he vacillates between an overemphasis on the similar or on the dissimilar; at times, especially under stress, he freezes his relationship with – his understanding of – this Otherness. He may become overly rigid, and his rigidity may determine the "texts" he elicits and the form he gives them. He may, in his anxiety, attempt to arrest time. Fortunately, the field experience is a lived experience that perdures, permitting a certain learning and requiring a flexibility that militates against this tendency to freeze both the relation with, and the understanding of, Otherness. Fortunately, too, most ethnographic encounters are, despite even the ethnographer, very human experiences. The savage is, so to speak, less cowed by the ethnographer than the ethnographer is by the savage.

I came to Morocco with an awareness of Otherness. I had been there briefly as a tourist several years before I began my field study, and I had lived in several other societies that were, in their own ways, more alien to me than Morocco. I did not meet Tuhami until I had been in Meknes for several weeks. He did not immediately threaten my taken-for-granted world. I had already been shocked on more than one occasion into questioning that world. (The most dramatic but hardly the most significant example was the first time I saw a Hamdushi slash his head until he was drenched with blood; the more significant examples were far less dramatic and in their own right hardly memorable: e.g., eating in restaurants in which there were never any women, waiting for hours for a new and casual acquaintance to feed me an enormous meal). I had reached a moment of flexible accommodation with my new Moroccan reality when I first met Tuhami. I could get along.

Such moments of flexible accommodation punctuate the field experience. If there are no conventions for describing encounters between two or more persons, they are quickly negotiated, but in a very specific and superficial manner. They are determined by the most essential matters on hand. With time, however, one's relations deepen and become more complex; they demand new accommodations – and new conventions. As Paul Rabinow observed with respect to his Moroccan informant Ali:

> there began to emerge a mutually constructed ground of experience and understanding, a realm of tenuous common sense which was constantly breaking down, being patched up, and re-examined, first here, then there. (Rabinow 1977, p. 39)

fieldwork must be understood within its temporal dimension as a process of continual discovery and self-discovery. There is considerable truth in Paul Ricoeur's involuted definition – quoted by Rabinow (1977) – of the hermeneutic as "the comprehension of self by the detours of the comprehension of the other." There is also a value, coordinate with tact and respect for the other, in pushing the swing of comprehension back to the other.

There is no doubt that I learned much about myself and my world through the detour of my comprehension of Tuhami. Some of this I have thought relevant to my study, and I have attempted to convey it explicitly or implicitly. Some of it I find irrelevant – and even irreverent to Tuhami – and I have tried to omit it. I am not, after all, engaged in autobiography, except in the most tenuous sense. I am certainly not interested in confession and expiation, though both confession and expiation enter inevitably into my enterprise.

As I look back over my notes, and as I attempt to recall my meetings with Tuhami some ten years ago, I am immediately struck by the impoverished quality of my emotional response. My questions seem frequently cold, unemotional, and detached. Was I frozen before Tuhami? In part, the question must be answered in the affirmative. There were times when my relations with Tuhami specifically or with Morocco and the Hamadsha more generally – the two cannot easily be distinguished – were such that I could not permit myself any response but the most distant. It was at such times that I took refuge in my difficulties with Arabic and exploited, I suppose, the presence of Lhacen. It was at such times, too, that I made use of "ethnographic distance" and various theoretical positions, most notably the psychoanalytic but others as well, to distance myself and to defend myself from an onslaught of presumably intolerable emotions. (I should add here that Tuhami took refuge at times in Lhacen's presence, in "ethnographic distance" as he understood it, and, undoubtedly, in his own theoretical understanding of what was transpiring.)

Even today, as I write, such defensive maneuvers, in more attenuated form, I believe, come into play. Indeed, at some level, my literary enterprise must be conceived in such terms. I have difficulty, both stylistically and psychologically, in distinguishing the time of encounter from the time of writing. For Tuhami, I have my notes; for myself, I have only my memory. I do not know when my theoretical confabulations, my observations and explications, result immediately from the encounter and when they result from the literary reencounter. I note, for example, that I have tended to overinterpret Tuhami's words in the first pages of his portrait. Is this a result simply of the need to introduce the reader to Tuhami and his culture and to explain my own theoretical orientation? Or am I repeating the over-interpretation that comes in the first months of fieldwork? Am I, in other words, recreating a past response or responding anew to an encounter?

The ethnographic encounter, like any encounter, however distorted in its immediacy or through time, never ends. It continually demands interpretation and accommodation. The ethnography, as I have written elsewhere (Crapanzano 1977b), is an attempt to put a full stop to an encounter that is necessarily disorienting. The same may be said of the portrait, the case history, the life history, the biography, and even the autobiography. In their own ways they all demand a cessation of time – a complete departure from the encounter. The sadness, the guilt, the feelings of solitude, and the love that come with departure and death will not, cannot, end. Tuhami has come to embody these feelings for me, much, I think, as the demons that haunted him embodied similar feelings for him.

There was always something captivating about Tuhami's discourse. It was as though he wanted to entrap me, to enslave me *through the power of the word* in an intricate web of fantasy and reality – to reverse, if you will, the colonial relationship that I as a foreigner, a *nasrani*, must have suggested to him. There was something seductive in his discourse, too. He did not in fact want *me* or anyone else. That would have been too immediate, too burdensome, too demanding for him. What he wanted, I have come to believe, was rather the imaginary fulfillment of an emptiness, a lack, a *manque-à-être*, to use Jacques Lacan's (1966) phrase, that he suffered. I became, I imagine, an articulatory pivot about which he could spin out his fantasies in order to create himself as he desired. I was created to create him, to fill metaphorically the emptiness that his desire, in its perversity, desired. Tuhami wanted fulfillment through the metaphor without denying the essentially irreal quality of the metaphor. Anything more concrete would have been too dangerously real.

As for me, I was soon captivated and seduced by Tuhami's evocations. I now see signs of captivation and seduction in my very first meeting with him. I guarded myself with the devices offered by my science and with a certain forced naïveté. I would return from my sessions with Tuhami filled with the joy of discovery and eagerly describe them to my wife.[3] We were fascinated and pleased with the constant deepening of our awareness of Morocco that came through Tuhami and many of my other informants. I pushed ever deeper, sometimes without the restraint that is required in such encounters. (Lhacen frequently corrected my haste with his sure sense of tact and his indomitable patience; he too was excited by our discoveries.) Tuhami was, at the beginning and occasionally at other times during our meetings, potentially graspable. All that was required was time. His distance from me, his dissimilarity, made him into a specimen. But I soon felt very uneasy in this attitude. My conversations with my wife soon shifted from fascination and joy to concern and worry. We were coming to know Tuhami as a person and beginning not only to sympathize with his condition but to empathize with him. Care had entered our relationship.

A confident empathy is not readily forthcoming in fieldwork. The fieldworker is often overwhelmed by dissimilarity; he is too distant and too objective. To experience the Other as a subject through the full range of his emotions, Sartre (1956) observes, is not an act of passive cognition. It is an active granting of importance – importance for oneself – to the Other's subjectivity. The Other must matter in one's own self-constitution; he must not simply be an object of scientific or quasi-scientific scrutiny. To understand the Other, the ethnographer must come to participate as best he can in the Other's reality. Aside from the usual difficulties that come with entering into an alien cultural tradition, the ethnographer is caught within a dilemma of intentionality that has come to be described by the oxymoron "participant observation" (Rabinow 1977). On the one hand, the ethnographer must engage in the life of the people he studies; he must enter into their intentionally determined world – the world of their praxis; and he must permit himself somehow only the self-reflection necessitated by their (and his) particular praxis. (I assume here that all intentional activities both demand and delimit self-reflection.) On the other hand, the fieldworker must remain faithful to his own primary intention: to do research. He must be able to remove himself from the life of the people he studies; he must remain outside their intentionally determined world; and he must permit himself a self-reflection that is demanded and delimited by his own particular praxis, his research.

The instant that marked the change in my relationship with Tuhami reflects, I believe, my own inability to maintain, or to pretend to maintain, these two distinct intentionalities. Tuhami had come to matter to me. My science and even the presence of Lhacen were no longer sufficient to distance me from him. His dissimilarity had fallen away to reveal his similarity. (Or perhaps I had discovered in his dissimilarity a similarity, though I do not think this was the case at the time.) And I believe I had come to matter for him. I see evidence of this not only in his response to me and to Lhacen – and Lhacen's to me, for that matter

– but in several of Tuhami's dreams that I report in the next part of this book. Unfortunately, I have no exact record of my own dreams. I do remember, rather vaguely, a dream fragment I had, either toward the end of my stay in Morocco or several seeks later in Paris. I was watching Tuhami, from the outside, sitting cross-legged in the courtyard of a saint's tomb. The tomb itself was brilliantly white. He was wearing very bright blue pants and was swaying back and forth in meditation – oblivious of me. Then (a secondary elaboration, I believe) he looked up and smiled at me.[4]

This change precluded, at least at the time, the maintenance of a double intentionality. I had to respond to Tuhami in the immediacy of our relationship. I was relieved, and so was Lhacen, who, despite his sophistication, must have been puzzled by the very strange transactions I had initiated. He began to reiterate the advice I was giving to Tuhami. I was nervous and at times stiff in my new role, less because of its newness than because of my imminent departure.

Tuhami was relieved, too. He yielded to me. He came to speak my language – the language of the "real" rather than the "imaginary," however sanctioned it was by his traditional idiom. I was unable at the time to recognize the putative quality of the real; I did not understand that the "real" as well as the "imaginary" can serve a metaphorical function. The colonial relationship was restored. I was secure and could rationalize my position as protector-therapist. Tuhami accepted this reversal with ease, not simply because it is always easier to return to old ways, especially when dependency is involved, but because he could at last understand our relationship. Although my ways were mysterious to him, their mystery itself was familiar. The ways of the Moroccan curer, like the ways of all curers, are always mysterious.[5]

I have been writing largely as though Tuhami and I were conversing alone. I have ignored Lhacen. In Part One I wrote:

> As a Moroccan and yet a stranger to Meknes, and as a Berber, Lhacen provided, I believe, a "familiar distance" that was necessary for the frankness of our discourse. Had he not been there, our relationship would have been awkward. Present, he could be ignored and was ignored.

Masked here by a rhetorical flourish is a significant contradiction. How could Lhacen both free the relationship between Tuhami and me of its awkwardness and yet be ignored? To consider this contradiction – and it must be considered – is to consider the role of the Third in any relationship. Lhacen's presence raises the question of the use of a field assistant or an interpreter in anthropological research.

In my fieldwork I have worked both alone and with a field assistant. I have found that there is a qualitative difference in the material obtained in the two situations. Contrary to what I would have expected on theoretical grounds, I have found that the material I collected with a field assistant, at least in the initial phases of research, had an intimacy of tone and detail that I did not obtain when I worked alone.

Lhacen – and the field assistant more generally – served to mediate the relationship between Tuhami and me in a very complex manner. He and I were both strangers – to Meknes, to Tuhami, and to each other. I was a stranger to Morocco as well. We shared, in our relationship with Tuhami, that "unity of nearness and remoteness" that Simmel (1964a) finds in the "phenomenon of the stranger." In relationship with the stranger, "distance means that he, who is close by, is far, and strangeness means that he, who is also far, is actually near" (Simmel 1964a, p. 402). As strangers we shared a kind of objectivity (see Schutz 1944; Nash 1963), a detachment even, that was rationalized in my case by my science and in Lhacen's case perhaps by the "job" he was performing for me. We shared a common intention: to learn as much as we could about the Hamadsha and about the people, like Tuhami, around them. In different ways, we were both entrapped in the formulations of that intention. The most significant of these, and for me, at least, the most personally distasteful, was the formulation – the creation – of the informant, whom I frequently confused, and still confuse in my reveries, with the "informer."[6] Lhacen told me after meeting Tuhami for the first time that he had discovered "un informateur formidable." I was excited by the prospect, but now, in retrospect, I am troubled by it. Tuhami, Moroccans more generally, and the Hamadsha had become for Lhacen, as well as for me, informants-to-be-discovered! Lhacen and I had talked for many hours about Morocco, the Hamadsha, ourselves, and about my research aims and strategies. We had visited many sanctuaries and brotherhoods throughout Morocco before finally deciding on the Hamadsha in and around Meknes. We had rehearsed, so to speak, "our" research. We accommodated ourselves to it, to each other, and to the people, now potential informants, with whom we were to work. We became friends; but in differing ways and for different reasons we cast the eye of a stranger on our relationship to each other. Lhacen was for me always a Moroccan, an informant – privileged to be sure – in his own right. I was his employer, who offered him not only a livelihood but the opportunity to acquire knowledge that could be of potential benefit to him.

Lhacen and I were strangers in very different ways. I was an American, a *nasrani*, a speaker of French – the language of the *colon* – a man of letters. Lhacen was a Berber from Marrakech, originally from an isolated village high in the Atlas Mountains, a *sherif*, a member of a holy and venerable lineage, a Moroccan. My roots, the milieu from which I drew my personal sustenance, as the Moroccans might say (Rosen 1972), were distant, in Europe and America. There, in the space that was "magically" construed for Tuhami and others in his milieu, was the locus of my identity, the ground of my meaningful world, my most significant social horizon, the place where my friends and family dwelt, the center of my ambition, and the privileged arena of my concern. Lhacen's roots, his identity, his meaningful world, his social horizon, his friends and family, his ambition, and his concern, were in Morocco. He was bound in that "chain of consociation" that characterizes Moroccan social life (Rosen 1972a, b and above, p. 77). He was a participant in a society that, as Clifford Geertz argues (somewhat too rhetorically), "does not cope with its diversity by sealing it into castes, isolating it into tribes, dividing it into ethnic groups, or covering it over with some common denominator concept of nationality . . . (Geertz 1975, p. 52)." Lhacen could be incorporated into Tuhami's world, could be classified and understood, in a way that I could never be. Lhacen did not possess the privilege of departure that I did. Most Moroccans I met – but, significantly, not Tuhami – were, I suspect, far more envious of my car and my passport than of my other material possessions.

Lhacen's role in the exchanges between Tuhami and me varied over time. At first it was active. It was Lhacen who discovered Tuhami and introduced him to me. Tuhami was for Lhacen a "find" that could help seal his relationship with me (by attesting to his seriousness, his unique capacities, and his understanding of my needs) and affirm my dependency on him. (Lhacen and I often talked, after those first meetings with Tuhami, about Tuhami's potential as an informant.) Lhacen was also able to demonstrate his importance to Tuhami. Not only was he familiar with the ways and language of the *nasrani* – and this familiarity was so important to him that he assumed its importance for others – but he was also able to provide Tuhami with an additional income. I paid Tuhami, as I paid all the Moroccans who took time from their own work to help me. This was expected, and they never bickered or argued or vied with one another for my favor; certainly, Tuhami did not. Payment gave some of them an "understanding" of my demands. They had a job. Tuhami refused this "understanding." He never spoke directly about the money I gave him except when he lost a portion of it and told me he was saving it for his first *jallaba* (see below). Usually, and despite its evident importance for him, he accepted the money with seeming indifference.

In those first meetings, Lhacen mediated my relationship with Tuhami. I was still new to Morocco. I was caught in the whirl of the unfamiliar. I was without anchor and did not have the confidence that comes with knowing the rules of social comportment and cultural evaluation. I was determined not to succumb to the easy aloofness of the total stranger. I felt awkward, confused, lonely even in the presence of my wife, and occasionally afraid. I was terrified of failure and of everything that failure symbolized for me, and I gave expression to this terror, most notably in terms of a loss of *rapport* with the best of my informants. Tuhami was among them, and the sway of my terror and its idiosyncratic expression are evident in my *entretiens* with him. I clung, in those first encounters with Tuhami, not only to a rigid and rather banal conception of my task but to Lhacen as well. He gave me distance and protected me from direct and immediate contact and from the fears and pleasures of such contact. Lhacen was less of a stranger than Tuhami. I imagine that Lhacen served a similar role for Tuhami. He did not, at any rate, get in the way.

Lhacen, and other assistants in Morocco and elsewhere, also gave me access to the more immediate and, as I have said, more intimate world of my informants and friends. It was not *they* who had to give me access. Through Lhacen I had already had my "introduction" to Morocco. They did not have to feed me those representations of culture and society, those clichés, by which the members of any group present themselves to, and defend themselves against, the stranger. (These representations frequently become the stuff of superficial ethnographic description and bolster the stranger's stereotypic view of an alien people.) Rather, Lhacen's presence, and his and therefore my putative savvy, permitted a more direct entrée into the lived world of the Moroccans with whom we worked. Their reflections were not determined by the presence of a "total stranger."[7] My informants were deprived to some extent of a protective shield, or, perhaps more accurately, they were given, through Lhacen, a new shield. The defenses they did have had to be respected or else overcome through time and warmth and concern.

I am pointing here to certain structural factors in the use of an assistant that may facilitate certain types of research. (Among others would be the slowed rhythm of the meetings, the possibility of observing often illuminating distortions within the translations, the ability to deflect responsibility for questions and misunderstandings to a Third, and the opportunity to discuss the meetings afterwards – an opportunity with obvious implications for the meetings themselves.) Certainly, my relationship with Lhacen and Tuhami was unique. As I remarked earlier, Lhacen had an almost uncanny ability to efface himself in our encounters – an effacing that, paradoxically, did not preclude his active participation as an interpreter and, later as an interpreter-observer in these encounters.[8] (Self-effacement does not necessarily or even usually come with silence and feigned invisibility. It requires, I believe, the quality that Heidegger [1971] finds characteristic of "equipment" [*das Zeug*] in his famous analysis of Van Gogh's paintings of peasant shoes: *Verläßlichkeit*, reliability. In the picture, one's attention is called to the shoes and all they evoke; in fact, the peasant "simply wears them." This reliability "first gives to the simple world its security and assures to the earth the freedom of its steady thrust.") We were both strangers and, as such, encouraged openness. The stranger, Simmel (1964a) noted, "often receives the most surprising openness – confidences which sometimes have the character of a confessional and which would be carefully withheld from a more closely related person." However this may be, a certain warmth and sympathy, an approachability, is requisite for such openness even among strangers. Lhacen possessed this, and I believe that I did, at least when I was not frozen in myself. Both of us remarked on the innumerable confidences we had received, over the years, from total strangers.

With time, Lhacen's role in the meetings changed. Tuhami and I recognized our importance for each other.[9] It was *we* who were meeting. We nevertheless needed Lhacen. We could not go on without him, but in our diverse ways we bracketed him off (never completely, however, for his presence was necessary to our relationship). He was, for Tuhami and me, the Third, who rendered us, in Sartre's (1964) words, an us-object.

> Thus what I experience is a being-outside in which I am organized with the Other in an indissoluble, objective whole, a whole in which I am fundamentally *no longer distinct* from the Other but which I agree in solidarity with the Other to constitute. (Sartre 1964, p. 418)

The essentially conflictual nature of the dual relationship, in Sartre's understanding and in Hegel's and Simmel's too, is, at least for the moment, arrested. Through the Third, embodied here in Lhacen, Tuhami's and my possibilities become "dead possibilities": "But as soon as the Third appears, the Other's possibilities and my own are leveled into dead possibilities, and hence the relationship becomes reciprocal" (Sartre 1964, p. 418).

There is, as Sartre brilliantly portrays in *No Exit* (1945), a fundamental instability in any triadic relationship. There is a constant shifting of alliances and objectifying gazes. These, of course, were all at play in the meetings between Tuhami, Lhacen, and me; but after the first meetings, where Lhacen played an active role, they tended to be subsumed under an intentionally validated, an ad hoc conventional, frame that we had negotiated and now accepted. The meetings were between Tuhami and me. Lhacen was a kind of spokesman for one and then the other of us; that is, he was identified seriatim with each of us as we addressed the other.

It is precisely the conventionally validated frames, ignored by Sartre in *Being and Nothingness* and *No Exit*, that permit a certain (symbolic) constancy in our triadic relations. Within the *established* frame, Lhacen was able to mediate those minor conflicts, those "insignificant differences of opinion, the allusions to an antagonism of personalities, the emergence of quite momentary contrasts of interest and feeling," that continually color, as Simmel (1964b) notes, even the least significant conversations and that certainly colored my conversations with Tuhami.

> Such mediations do not even have to be performed by means of words. A gesture, a way of listening, the mood that radiates from a particular person, are enough to change the difference between two individuals so that they can seek understanding, are enough to make them feel their essential commonness which is concealed under their acutely differing opinions, and to bring this divergence into the shape in which it can be ironed out the most easily. (Simmel 1964b, p. 149)

It is the difference between the frame and the action within the frame that permitted me to assert, and permits me to reassert, the contradictory observation with which I began this discussion of Lhacen's role in my encounter with Tuhami. "Had he not been there, our relationship would have been awkward. Present, he could be ignored and was ignored."

The complicity to bracket off Lhacen – a complicity that the three of us entered into from our different standpoints – not only permitted the conventional framing of our encounter and the maneuvers that occurred within it; it also enabled us to invest Lhacen with symbolic significance. He came to represent the constancy of the frame. Indeed, he became for me, and for Tuhami too, I believe, a symbol of constancy and continuity in a discourse that was threatened with interruption through both fantasied departures (Tuhami's announced intention to make a trip) and my inevitable departure. The fact of departure, which has been largely ignored in the anthropological discussions of fieldwork, plays a determining role in all field research.[10] The anthropologist as stranger – the wanderer who comes and leaves – is not the stranger of whom Simmel (1964a) writes. His stranger comes to stay. The inevitability of my departure, understood at the outset and ignored thereafter, was reflected not only in the rhythm of my *entretiens* with Tuhami and Lhacen's role in them but also in the recurrent themes of separation, death, castration, and abandonment that punctuated these interviews.[11]

Lhacen himself played out his symbolic role in mediating conflict. He thus freed Tuhami and me of the burden of constancy and continuity and the fear of death and departure and allowed us the indulgence of symbolic attribution, of transference and countertransference. As controller of the word (and he struggled hard with our words and resented, I thought, my increasing knowledge of Arabic), he came to embody, quite literally, the transcendental Other, that "seat of the Word and guarantor of Truth," which, if I understand Lacan (1966) correctly, is necessary to intersubjective communication (Crapanzano 1978). (Lacan, *pace* Lacan, can be read as a gloss not only on Freud but on Sartre as well.) Lhacen occupied, to speak figuratively, the place of God – in Sartre's terms, the place of the unrealized Third – within the little universe we three created.[12]

I exaggerate of course – to point out an irony. We assume that Lhacen played a noteworthy role – a role that governed the exchange between Tuhami and me. He did. Of this there can be no question. Yet I must recognize here a certain "empirical" bias. Possessed of body, Lhacen could embody for me, and perhaps for my readers, highly abstract symbolic meanings. Are Tuhami's demons – who are bodiless, at least as I understand "body" – thus incapable of such "embodiments"? Is it not possible that 'A'isha Qandisha, and the other *jnun* that haunted Tuhami's life, represented the Third, the transcendental locus of meaning, the constancy and continuity of our relationship, of any relationship he had? (I have argued elsewhere [1977a] that possessing spirits provide the possessed with a frozen identity by arresting the dialectics of identity formation.) Or perhaps these spirits render the relationship considerably more complex than I have described it here. I remember working with other Moroccans who, for whatever reasons, suddenly became aware of (we might say, "were spooked by") a demonic presence. My interviews then came to a dead halt. I am not proposing here a mysticism or a philosophy of excessive idealism. I am simply calling attention to a possibility of Otherness that I do not even claim to be Tuhami's. It must be entertained though.

There is, at least for me, an elegiac quality in my re-creation of Tuhami. I recognize, as I write now, the contingency of our existence. How did it come to pass that I, an American anthropologist, should have met Tuhami, a Moroccan tilemaker, and entered so deeply into his life and allowed him to enter so deeply into my own? I write of him now with the hope that something of what I learned from him will serve to correct our own mechanistic presumptions about the nature of man and his relations to his fellow beings. I have placed my personal encounter within an abstract theoretical edifice – a consequence of my encounter – that is neither fully consistent nor as illuminating as I should have liked it to be in order to call attention to these presumptions. With the same end in mind I have played with style and form, and I am satisfied with neither. I have forced myself into the theoretical position that we can know the experience of another only by what he says (as though a text can be understood without the assumption of intersubjectivity), and, at the same time, I have made a plea for a more immediate intersubjective understanding, which I take to be necessary in any social encounter. This paradox, masked in most ordinary, conventional encounters, is brought to light in the ethnographic encounter and may well be its hallmark. "It probably requires cultural insiders," James Clifford (1978) speculates, "to recognize adequately the subtle ruses of individuality where outsiders see only typical behavior." I do not know. Our individuality, Simmel (1965, p. 344) noted, in the same essay I have been quoting, "is supplemented by the Other's view of us, which results in something that we never are purely and wholly." The same must be said for the Other, for Tuhami, in his subjectivity. We should respect in the Other the same mystery we expect others to respect in ourselves. This too is a social fact.

# 34

# The Way Things Are Said

*Jeanne Favret-Saada*

*It seems that even the pure light of science requires, in order to shine, the darkness of ignorance.*

Karl Marx (1856)

Take an enthnographer: she has chosen to investigate contemporary witchcraft in the Bocage[1] of Western France. She has already done some fieldwork; she has a basic academic training; she has published some papers on the logic of murder, violence and insurrection in an altogether different, tribal society. She is now working in France, to avoid having to learn yet another difficult language. Especially since in her view the symbolic shaping out of murder or aggression – the way things are said in the native culture – is as important as the functioning of political machinery.

## I The Mirror-Image of an Academic

Getting ready to leave for the field, she looks through the scientific (and not so scientific) literature on contemporary witchcraft: the writings of folklorists and psychiatrists, of occultists and journalists. This is what she finds: that peasants, who are 'credulous', 'backward' and impervious to 'cause of effect', blame their misfortune on the jealousy of a neighbour who has cast a spell on them; they go to an unwitcher[2] (usually described as a 'charlatan', now and again as 'naïve') who protects them from their imaginary aggressor by performing 'secret' rituals which 'have no meaning', and 'come from another age'. The geographical and cultural 'isolation' of the Bocage is partly responsible for the 'survival' of these 'beliefs' in our time.

If that is all there is to be said about witchcraft (and however much you try to find out from the books of folklorists or the reports of trials in the French press over the last ten years, you will learn no more), you may wonder why it seems to be such an obsession. To judge by the public's immense curiosity, the fascination produced by the very word 'witchcraft', the guaranteed success of anything written about it, one wonders what journalistic scoop could ever find a greater public.

Take an ethnographer. She has spent more than thirty months in the Bocage in Mayenne, studying witchcraft. 'How exciting, how thrilling, how extraordinary . . .!' 'Tell us all about the witches,' she is asked again and

Jeanne Favret-Saada, "The Way Things Are Said," pp. 3–12, and "Between 'Caught' and 'Catching'," pp. 13–24 from Jeanne Favret-Saada, *Deadly Words: Witchcraft in the Bocage*, trans. Catherine Cullen (Cambridge: Cambridge University Press, 1980). English translation © Maison des Sciences de l'Homme and Cambridge University Press 1980. Originally published in French as *Les mots, la mort, les sorts*. Paris: Gallimard, 1977. © Éditions Gallimard, Paris, 1977.

again when she gets back to the city. Just as one might say: tell us tales about ogres or wolves, about Little Red Riding Hood. Frighten us, but make it clear that it's only a story; or that they are just peasants: credulous, backward and marginal. Or alternatively: confirm that *out there* there are some people who can bend the laws of causality and morality, who can kill by magic and not be punished; but remember to end by saying that they do not really have that power: they only believe it because they are credulous, backward peasants . . . (see above).

No wonder that country people in the West are not in any hurry to step forward and be taken for idiots in the way that public opinion would have them be – whether in the scholarly version developed by folklorists, or in the equally hard faced popular version spread by the media.

To say that one is studying beliefs about witchcraft is automatically to deny them any truth: it is just a belief, it is not true. So folklorists never ask of country people: 'what are they trying to express by means of a witchcraft crisis?', but only 'what are they hiding from us?' They are led on by the idea of some healer's 'secret', some local trick, and describing it is enough to gratify academic curiosity. So witchcraft is no more than a body of empty recipes (boil an ox heart, prick it with a thousand pins, etc.)? Grant that sort of thing supernatural power? How gullible can you be?

Similarly, when the reporter, that hero of positivist discourse, goes along on behalf of a public assumed to be incredulous, and asks country people whether they 'still believe' in spells, the case is decided in advance: yes, people do still believe in spells, especially if you go to the Lower Berry or the Normandy Bocage. How convenient that there should be a district full of idiots, where the whole realm of the imaginary can be held in. But country people are not fools: they meet these advances with obstinate silence.

But even their silence about things to do with witchcraft; and more generally about anything to do with illness and death, is said to tell us about their status: 'their language is too simple', 'they are incapable of symbolizing', you won't get anything out of them because 'they don't talk': that is what I was told by the local scholarly élite. Why not say they are wild men of the woods, since they live in a 'bocage'; animals, even? 'Medicine is a veterinary art round here' a local psychiatrist once told me.

So all that was known about witchcraft is that it was unknowable: when I left for the field, knowledge of the subject boiled down to this. The first question I asked myself when I met the peasants, who were neither credulous nor backward, was: is witchcraft unknowable, or is it just that those who say this need to block out all knowledge about it in order to maintain their own intellectual coherence? Does the 'scholar' or the 'man of our own age' need to comfort himself with the myth of a credulous and backward peasant?

The social sciences aim to account for cultural differences. But can this be achieved by postulating the existence of a peasant who is denied all reality save that he is the mirror-image of an academic?

Whenever folklorists or reporters talk of witchcraft in the country, they always do so as if one were facing two incompatible physical theories: the pre-logical or medieval attitude of peasants, who wrongly attribute their misfortunes to imaginary witches; and ours, the attitude of educated people who know how to handle causal relations correctly. It is said or implied that peasants are incapable of this either because of ignorance or of backwardness. In this respect, the description given of the peasant and the '*pays*', the canton, that determines him is governed by a peculiar set of terms which necessarily imply that he is incapable of grasping causal relations. Witchcraft is put forward as a nonsense theory which peasants can afford to adopt because it is the local theory. The folklorist's job is then to underline the difference between his own theory (which also happens to be a 'true' one) and the peasant's, which is only a belief.

But who can ignore the difficulties involved in postulating the coexistence of two incompatible physical theories which correspond to two ages of humankind? Do you really have to do thirty months of fieldwork to be in a position to say that country people are just as well able to cope with causal relations as anyone else, and to make the suggestion that witchcraft cannot be reduced to a physical theory, although it does indeed imply a certain kind of causality?

## II Words Spoken with Insistence

I began by studying the words used to express biological misfortunes, and used in ordinary conversation: about death, sterility, and illness in animals and humans. The first thing one notices is that they distinguish between ordinary misfortunes and their extraordinary repetition.

In the Bocage, as anywhere else in France, ordinary misfortunes are accepted as 'one-off'; so, a single illness, the loss of one animal, one bankruptcy, even one death, do not call for more than a single comment: '*the trouble with him is that he drinks too much*'; '*she had cancer of the kidneys*'; '*my cow was very old*'.

An onslaught by witchcraft, on the other hand, gives a pattern to misfortunes which are repeated and range over the persons and belongings of a bewitched couple: in succession, a heifer dies, the wife has a miscarriage, the child is covered in spots, the car runs into a ditch, the butter won't churn, the bread won't rise, the geese bolt, or the daughter they want to marry off goes into a decline ... Every morning, the couple ask anxiously: '*What on earth will happen next?*' And every time some misfortune occurs: always unexpected, always inexplicable.

When misfortunes occur like this in series, the countryman approaches qualified people with a double request: on the one hand for an interpretation, and on the other for a cure.

The doctors and vets answer him by denying the existence of any series: illnesses, deaths and mechanical breakdowns do not occur for the same reasons and are not treated in the same way. These people are the curators of objective knowledge about the body, and they can claim to pick off one by one the causes of the misfortunes: go and disinfect your stables, vaccinate your cows, send your wife to the gynaecologist, give your child milk with less fat in it, drink less alcohol... But however effective each separate treatment may be, in the eyes of some peasants it is still incomplete, for it only affects the cause and not the origin of their troubles. The origin is always the evil nature of one or more witches who hunger after other people's misfortunes, and whose words, look and touch have supernatural power.

Faced with a bewitched, one can imagine that the priest is in a more awkward situation than the doctor, for evil, misfortune and the supernatural mean something to him. But what they mean has become singularly blurred by many centuries of theological brooding. The dividing line between the ranges of the natural and the supernatural has been fixed by Catholic orthodoxy; but the reasons given have scarcely been assimilated, especially since each late pronouncement does not categorically cancel former ones. So theological knowledge is no more unified in the mind of a country priest than it is in the body of doctrine.

Hearing the various stories told in his parish, the priest can choose between three different and mutually exclusive types of interpretation:

1 He can dismiss these misfortunes as part of the natural order, and so deny them any religious significance: by doing so he sides with medical ideology, and in effect says the bewitched are raving or superstitious people.

2 He can acknowledge that these misfortunes do pertain to the supernatural order, but are an effect of divine love: so the bishop of Séez preaches 'good suffering' to a congregation of '*luckless*' peasants. A universally aimed (Catholic) discourse can turn him who is '*luckless*' into the most lucky. The man whom God loves best and so chastises, is only a victim in the eyes of the world. This reversal of appearances sometimes has its effect.

3 The priest can meet the peasant on his own ground and interpret his misfortunes as the work of the devil. He is permitted to do this by at least one branch or stratum of theology. He then has two alternatives.

He may consult, as he is supposed to, the diocesan exorcist, the official expert in diabolical matters appointed by the hierarchy. But in Western France, the priest knows very well that he is not likely to convince the expert, who has held this position for thirty years precisely because he is skeptical about the devil's interest in so-called 'simple' peasants: you have to be clever to interest the devil. So the diocesan exorcist, in the elitist style of any country priest who has risen in the Church or any peasant who has risen in society, offers the positivist interpretation. He refuses to give any religious meaning to the peasant's misfortune except by mentioning 'good suffering' or saying he will pray for him. Like the doctor, he refuses the peasant's request for a meaning by advising the man to consult a psychiatrist, to live a more balanced life, and to apply better the rules of the experimental method. The village priest knows in advance that to send a bewitched to the diocesan exorcist is to ask him to take his troubles elsewhere, and in effect to direct him to a doctor by way of the ecclesiastical hierarchy.

Alternatively, the priest comes and exorcises the farm and its inhabitants without consulting the hierarchy. As a more or less willing distributor of blessings and medals, holy water and salt, he plays the role in his parish of a small-scale unwitcher who protects people from evil spells without sending them back to the witch.

'*If it's a small spell, it works*': the series of misfortunes stops and everything returns to normal. It works, but the origin of the misfortune and its repetition are still not satisfactorily symbolized. For when the peasant talks about being bewitched to anyone who is willing to listen, what he wants acknowledged is this: *if such repetitions occur, one must assume that somewhere someone wants them to*. I shall show later that witchcraft consists in creating a misunderstanding about who it is that desires the misfortunes of the bewitched. Note here that the Church's rite merely clouds the issue by attributing the evil to some immaterial spirit included by half-hearted theology in a list of '*preternatural facts*'. For the victim, the witch is some familiar person (a neighbour, for example) whose aims he can at least hope to discover.

If '*it doesn't work*': if the priest '*isn't strong enough*' because his parishioner is '*caught tight*' in the spells, the bewitched is left with his question: why this series of events, and why in my home? What is at stake here, my sanity or my life? Am I mad, as the doctor says, or does someone have it in for me to the point of wanting me to die?

It is only at this point that the sufferer can choose to interpret his ills in the language of witchcraft. Some friend, or someone else who has noticed him moving deeper into misfortune and seen the ineffectiveness of approved learning makes the crucial diagnosis: '*Do you think there may be someone who wishes you ill?*' This amounts to saying: 'you're not mad, I can see in you the signs of a similar crisis I once experienced, and which came to an end thanks to this unwitcher.'

The priest and the doctor have faded out long ago when the unwitcher is called. The unwitcher's task is first to authenticate his patient's sufferings and his feeling of being threatened in the flesh; second, it is to locate, by close examination, the patient's vulnerable spots. It is as if his own body and those of his family, his land and all his possessions make up a single surface full of holes, through which the witch's violence might break in at any moment. The unwitcher then clearly tells his client how long he still has to live if he stubbornly remains defenceless. He is a master of death; he can tell its date and how to postpone it. A professional in supernatural evil, he is prepared to return blow for blow against '*the person we suspect*', the alleged witch, whose final identity is established only after an investigation, sometimes a long one. This is the inception of what can only be called a cure. The séances later are devoted to finding the gaps which still need sealing, as they are revealed day by day in the course of life.

## III When Words Wage War

In the project for my research I wrote that I wanted to study witchcraft practices in the Bocage. For more than a century, folklorists had been gorging themselves on them, and the time had come to understand them. In the field, however, all I came across was language. For many months, the only empirical facts I was able to record were words.

Today I would say that an attack of witchcraft can be summed up as follows: a set of words spoken in a crisis situation by someone who will later be designated as a witch are afterwards interpreted as having taken effect on the body and belongings of the persons spoken to, who will on that ground say he is bewitched. The unwitcher takes on himself these words originally spoken to his client, and turns them back on to their initial sender, the witch. Always the '*abnormal*' is said to have settled in after certain words have been uttered, and the situation persists without change until the unwitcher places himself like a screen between the sender and the receiver. Unwitching rituals – the actual 'practices' – are remarkably poor and contingent: this ritual or that, it makes no difference, any one will do. For if the ritual is upheld it is only through words and through the person who says them.

So perhaps, I was not entirely mistaken when I said I wanted to study practices: the act, in witchcraft, is the word.

That may seem an elementary statement, but it is full of implications. The first is this: until now, the work of ethnographers has relied on a convention (one too obvious to be stated) about the use of spoken words. For ethnography to be possible, it was necessary that the investigator and the 'native' should at least agree that speech has the function of conveying information. To be an ethnographer is first to record the utterances of appropriately chosen native informants. How to establish this information-situation, the main source of the investigator's knowledge, how to choose one's informants, how to involve them in a regular working relationship . . . the handbooks always insist on this truly fundamental point in fieldwork.

Now, witchcraft is spoken words; but these spoken words are power, and not knowledge or information.

To talk, in witchcraft, is never to inform. Or if information is given, it is so that the person who is to kill (the unwitcher) will know where to aim his blows. 'Informing' an ethnographer, that is, someone who claims to have no intention of using the information, but naïvely wants to know for the sake of knowing, is literally unthinkable. For a single word (and only a word) can tie or untie a fate, and whoever puts himself in a position to utter it is formidable. Knowing about spells brings money, brings more power and triggers terror: realities much more fascinating to an interlocutor than the innocent accumulation of scientific knowledge, writing a well-documented book, or getting an academic degree.

Similarly, it is unthinkable that people can talk for the sake of talking. Exchanging words just to show that one is with other people, to show one's wish to communicate, or what Malinowski called 'phatic communication' exists in the Bocage as it does anywhere else. But here it implies strictly political intentions: phatic communication is the expression of zero-aggressiveness; it conveys to one's interlocutor that one might launch a magic rocket at him, but that one chooses not to do so for the time being. It is conveying to him that this is not the time for a fight, but for a cease-fire. When interlocutors for whom witchcraft is involved talk about nothing (that is about anything except what really matters) it is to emphasize the violence of what is not being talked about. More fundamentally, it is to check that the circuit is functioning, and that a state of war does indeed hold between the opponents.

In short, there is no neutral position with spoken words: in witchcraft, words wage war. Anyone talking about it is a belligerent, the ethnographer like everyone else. There is no room for uninvolved observers.

When Evans-Pritchard, founder of the ethnography of witchcraft, studied the Zande, he made it his practice to interpret the events of his life by means of schemes about persecution, consulting oracles and submitting to their decisions: 'I was aided in my understanding of the feelings of the bewitched Azande', he says, 'by sharing their hopes and joys, apathy and sorrows [. . .]. In no department of their life was I more successful in "thinking black" or as it should more correctly be said "feeling black" than in the sphere of witchcraft. I, too, used to react to misfortunes in the idiom of witchcraft, and it was often an effort to check this lapse into unreason' (1937). But we learn from his book that actually the Zande had given him the position of 'Prince without portfolio', which is no slight consolation if one remembers that in Zande society, a prince can only be bewitched by another prince (a rather reassuring thought for an ethnographer established many miles from the court) and that by not giving him a portfolio, the Zande were exempting Evans-Pritchard from having to play the role, so important for the effectiveness of the cure, of symbolic guarantee of the return to order.

In other words, the ethnographer could not himself possibly be involved in a case of witchcraft.[3] In the Bocage, the situation happens to be less comfortable: nobody ever talks about witchcraft to gain knowledge, but to gain power. The same is true about asking questions. Before the ethnographer has uttered a single word, he is involved in the same power relationship as anyone else talking about it. Let him open his mouth, and his interlocutor immediately tries to identify his strategy, estimate his force, guess if he is a friend or foe, or if he is to be bought or destroyed. As with any other interlocutor, speaking to the ethnographer one is addressing either *a subject supposed to be able* (a witch, an unwitcher) *or unable* (a victim, a bewitched person).

It follows that wanting to know could only be – for me as for anyone else – in the name of a force which I claim to have or which my interlocutor credits me with. If I were not equipped to confront it, no one would believe I could survive unharmed, or even survive it at all.

'*Are you strong enough?*' I was often asked when I tried to establish an information-relationship, that is to get people who had experience of witch stories to tell me about them. A mere desire for information is the sign of a naïve or hypocritical person who must at once be frightened off. The effect that the person telling the story is trying to achieve is either to fascinate or to frighten: nobody would talk about it who did not hope to fascinate. If my interlocutor is successful, he says I have '*weak blood*' and advises me to change my course of research towards folk song or the ancient papegai festival. If he fears that he has not brought it off, he anxiously asks me how I can bear to hear such stories every day, and offers various assumptions: '*You've got strong blood*', or else '*you've got something*' (to protect yourself with). He then tries to identify my fetishes, to find out whether or not they are '*stronger*' than his own. Otherwise, he may identify me with a certain unwitcher who has just died, a double-edged compliment which I am bound to appreciate: to say that my '*hands tremble like Madame Marie's*' means that, like her, I'm '*quite strong*' – but also that in the end she met her master in witchcraft, and he did away with her quite recently.

As you can see, this is not exactly a standard situation, in which information is exchanged and where the ethnographer may hope to have neutral knowledge about the beliefs and practices of witchcraft conveyed to him. For he who succeeds in a acquiring such knowledge gains power and must accept the effects of this power; the more one knows, the more one is a threat and the more one is magically threatened. So long as I claimed the usual status of an ethnographer, saying I wanted to know for the sake of knowing, my interlocutors were less eager to communicate their own knowledge than to test mine, to try to guess the necessarily magic use I intended to put it to, and to develop their force to the detriment of my own. I had to accept the logic of this totally combative situation and admit that it was absurd to continue to posit a neutral position which was neither admissible or even credible to anyone else. When total war is being waged with words, one must make up one's mind to engage in another kind of ethnography.[4]

[...]

## II A Name Added to a Position

In pursuing the ethnography of spells, the first point to grasp is being clear about whom each 'informant' thinks he is speaking to, since he utters such radically different discourses depending on the position he thinks his interlocutor holds. To someone who '*isn't caught*', he will say: '*spells don't exist*'; '*they no longer exist*'; '*that was in the old days*'; '*they were true for our back people*'; '*they exist, but not here: go and look in Saint-Mars* (or Montjean, or Lassay: somewhere else...) '*over there, they're really backward*'; '*oh, spells! I don't hold with all that rot!*'. To someone who is '*caught*', one speaks in a different way, depending on whether the person is given the position of bewitched or unwitcher. (No one talks to the alleged witch, but this very silence is in itself a whole discourse, the silent assertion of a fight to the death, which always has some effect.)

When an ethnographer works in an exotic field, he too has to take up some sort of stance. But common sense and the handbooks point out the virtues of distance and the advantages to be derived from the status of rich cannibal. To claim, on the other hand, that one wants to hear about peasant witchcraft yet remain alien to it is to condemn oneself to hearing only objectivist statements and to collecting fantastic anecdotes and for unwitching recipes – i.e. to accumulate statements which the stating subject formally disavows. So for the last hundred and fifty years, the native and the folklorist have been looking at themselves in a mirror each has held up to the other, without the folklorist apparently ever noticing the ironic complicity that this implies on the part of the native.

When I left for the Bocage, I was certainly in no better position than any of my predecessors, except that I thought their findings trivial compared with the reality at stake in a witchcraft attack. Within a few months I had myself done much the same collecting as they; it left me unsatisfied, and gave me no guidance about how to pursue my investigation. It would have been just as futile for me to try and win over the peasants with large-minded statements of good intention, since anyway, in matters of witchcraft, it is always the other person who decides how to interpret what you say. Just as a peasant must hear the words of the annunciator, if he is to confess that he is indeed bewitched, so it was my interlocutors who decided what my position was ('*caught*' or not, bewitched or unwitcher) by interpreting unguarded clues in my speech.[5]

I must point out that I knew nothing about this system of positions, and that the main part of my work has been to make it out little by little by going back over puzzling episodes. For several months my notes describe a number of situations in which my interlocutors placed me in this stance or that ('*not caught*', '*caught*'-bewitched, '*caught*'-unwitcher) although at the time I did not see anything but a classic situation of ethnographic investigation, even if a somewhat difficult one because I was after something particularly secret.

I was probably not yet ready to maintain this speech process in the only way conceivable to my interlocutors: by accepting that being given such a stance committed me to utter my part in this discourse in the same way as they did. Of course this position existed before me and was acceptably occupied and maintained by others. But now I was the one being placed there, and my name was being attached to this position as well as to my particular personal existence.

Although I went through the whole experience in a state of some confusion, I can say today that it is actually patterned around a small number of characteristic situations in which my interlocutors required me to occupy a position that they indicated. They were conveying that they had no need of my ability to listen, for what mattered to them was not merely to be understood, or, in the language of communication-theory – they had no need for a decoder. In witchcraft, to receive messages obliges one to send out other, signed messages: it was time for me to speak.

For example, here are some instances of the manner in which I was put to the test: (1) the first time that the bewitched told me their own story (and not that of some hypothetical '*backward people*'), it was because they had identified me as the unwitcher who could get them out of their troubles. (2) A few months later, a peasant interpreted my '*weakness*', took on the role of annunciator of my state as a bewitched, and took me to his unwitcher to get me '*uncaught*'. (3) For more than two years, I subjected the events of my personal life to the interpretations of this unwitcher. (4) Several bewitched asked me to '*uncatch*' them. Although at this point I had become quite competent at handling magical discourse, I felt quite incapable of taking the speech-position upholding it, and I sent them on to my therapist. (5) Lastly, this unwitcher, with whom I had a complicated relationship (I was her client, agent, and guarantor of the truth of her words during the cures in which I was invited to participate) instructed me to bring her a healer who would relieve her of her bodily pains and to assist him in his task.

You could say, given the ideal assumption that I might have made my choice in full consciousness of the situation, that every time these were the alternatives: either I refused this assignment of my identity to a position and withdrew from the speech process pointing out that I was being mistaken for someone I was not (*I am not who you think I am*); or I agreed to occupy the position assigned to me, unless I could propose some other which I felt more able to occupy (*I am not where you think I am*). In the first case, I would have had to leave the Bocage, where I no longer had any place; in the second, the speech process would go on but I had to place myself in the position of subject of the enunciation.

It emerges from the asides of investigators that I was not the first person to be offered this alternative. Some folklorists, for example, tell of their amusement at having been invited, at one point or another, to act as unwitchers. This type of occurrence is worth looking into. Note in the first place, that it is out of the question for the investigator to be assigned the position of bewitched. He would have had to give some sign that he knows he is mortal, vulnerable or at least subject to desires – all things one can freely admit, but only to close relations and in confidence – certainly not to uneducated farmers and while practising one's profession. In the field, the investigator therefore routinely presents himself to his interlocutor as someone who does not lack anything: or to take up the expression I used above, he displays a continuous surface without holes in it. Everything in his behaviour suggests he is '*strong enough*'. This especially since he does not omit arguments likely to loosen their tongues: he may say he belongs to a local line of magic healers (he might claim, for example, that his maternal grandmother, who is still remembered in the area, '*passed the secret*' on to him); and in his conversations with 'informers', he shows he knows many unwitchment recipes, magic formulae and fantastic anecdotes. Without being conscious of it, the investigator has done everything necessary for his interlocutors to assign him the position of unwitcher. But if he is actually told this, and asked to perform, he is amused. He recounts this episode as if it were just an entertaining anecdote, and a particularly conclusive evidence of peasant gullibility, to a listener who is confidently assumed to feel equally superior. Indeed, there is cause to smile: there has been an error, a mistaken identity, the investigator was not the person he was thought to be.

But one may wonder who is more naïve, the peasant or the folklorist. The former cannot understand that one might collect formulae without putting them to any use, just for the sake of information; the latter judges that he has satisfied the demands of science by collecting information, without realizing he cannot do anything with it, neither science nor magic.

Not science: the folklorists failed to recognize the existence and role of the power of therapists in unwitching cures. They strained to find out what these therapists knew, and this in the particular form of secrets to be collected. In other words, whatever in their discourse most resembles an utterance, a statement which can stand on its own independent of the stating subject.

The content of the secret (the utterance) is for the most part neither here nor there: it does not matter whether one is told to pierce an ox heart, twist steel nails, or recite misappropriated Church prayers. Magicians know this, when they quietly say: '*to each one his secret*', and show themselves in no hurry to increase their knowledge. For what makes an unwitcher is his '*force*' and its links with a world of language (the very one which produced the content of the secret). The power of the magician, thus referred to a symbolic set, places him in the position of recognized avenger (and not, for example, of a criminal settling private scores), but on condition that he openly declares his readiness to assume this position.

Not magic: unwitching does not consist in uttering formulae or practising magic rituals. If they are to have any chance of being effective, a set of positions must first be established, by which someone who is not the magician places him in the position of subject supposed to be able; and the magician himself must acknowledge he is in it, and accept what this implies in terms of personal commitment to a discourse, and of assuming the effects of magic speech on his own body and so on.

So when the folklorist reacts to a request for unwitchment by laughing as if this were an inappropriate proposal, and excuses himself by saying he cannot do anything, or by sending the patient to his doctor, the peasant gathers that this academic does not want to commit his '*force*', if he has any; or more likely, that he has no idea what '*force*' is or just how much is involved in speaking. The folklorist's mirth simply shows that he does not think he can cure anyone with magic formulae, and that for him such knowledge is pointless. And so it is, unless a subject agrees to become the support of these magic utterances and to proffer them in the name of his own '*force*' taken as part of a symbolic universe – i.e. to convert this knowledge into a power.

## III Taking One's Distances from Whom (or What)?

So one cannot study witchcraft without agreeing to take part in the situations where it manifests itself, and in the discourse expressing it. This entails certain limitations which will seem most unwelcome to those who favour an objectivizing ethnography.

1 You cannot verify any assertion: first because there is no position of impartial witness in this discourse. Second, because it is pointless to question outsiders: to be bewitched is to have stopped communicating with one's presumed witch as well as with anyone not involved in the crisis; so other villagers know almost nothing of the matter. Finally, it is inconceivable that an ethnographer to whom someone had spoken as to the legitimate occupier of one of the positions in the discourse might step outside it to investigate, and ask what is the truth behind this or that story.

2 You cannot hear both parties – the bewitched and their alleged witches – since they no longer communicate. Not only do they not talk to each other, they do not speak the same kind of language. If, exceptionally, it were ever possible to obtain both versions of the same story, they could not be set face to face, since witches always claim that they do not believe in spells, object to the discourse of witchcraft, and appeal to the language of positivism.[6] In any case, the bewitched prevent any such confrontation by warning the ethnographer to avoid meeting their aggressor, for fear of becoming his victim. To take no notice of this advice would be a sign either of disturbing masochism, or of a rash faith in the powers protecting you, or indeed of an intention to work some betrayal. Note that such daring would be just as disturbing to the 'witch', however imaginary he may be: knowing that the ethnographer sees people who call themselves his victims, he would, on receiving a visit from this stranger, see him as an unwitcher come to fight him. In time of war, nothing so resembles the characteristic weapons of the magician (words, look and touch) as an innocent 'how are you?' followed by a handshake.

3 One cannot investigate in one's own '*quartier*' [neighbourhood] so dreaded is the magic effectiveness of speech. The peasant thinks it wise to maintain a certain distance between the speaker and the listener, to prevent the latter from taking advantage of the situation. A serious crisis will never be taken to the local unwitcher. People prefer to choose their therapist beyond some boundary (in a neighbouring diocese or *département*), in any case outside the network of acquaintanceship. For this reason, I never worked less than ten kilometres from where I was living. So in general, I remained unaware of the sociological context of witchcraft matters and especially of the particular positions of the opponents in the local struggles for prestige and power – and these usually constitute the subject-matter of ethnographic investigations into witchcraft.

4 One cannot set up any strategy of observation (even a 'participating' one) which keeps the agreed amount of distance that this implies. More generally, to claim an external position for oneself is to abandon hope of ever learning this discourse: first (remember) because those concerned react with silence or duplicity to anyone who claims to be outside. But more profoundly because any attempt at making things explicit comes up against a much more formidable barrier: that of the native's amnesia and his incapacity to formulate what must remain unsaid. These are the limits of what one can ask a willing informer (in so far as such persons exist in the Bocage), and they are soon reached.

To take one example: if you want to know the substance of a diviner's consultation, you can simply ask him what usually takes place in a séance, or what his clients consult him about. But you should not be surprised at trivial answers: '*They come because of illness, love affairs, animals, to recover money they have lost* . . .' – 'And what about spells?' – '*That might be the case, but I don't deal with that*' will be the diviner's systematic reply. A barrier, then, of silence and duplicity: the diviner can only admit '*dealing with that*' in front of someone who puts forward a personal request for divination. About the séances, on the other hand, he claims he honestly has nothing more to impart than a few matters of technique: '*I begin with the game of piquet and go on to tarot cards.*' – 'But how do you guess their story?' – '*Well, I have the gift.*' Even when the ethnographer's questions are more subtle, they soon come up against the bounds of the unstatable, represented here by the reference to a '*gift*'. Pressed to make himself more clear, the diviner can do no more than illustrate his statements by recounting the enigmatic circumstances in which, one day, a long time ago, when becoming a seer had not yet entered his head, a patient seeking for revelations sensed the '*gift*' in him, and announced it to the professional diviner who then initiated him.

If the ethnographer resorts to the patients, he obtains uniformly improbable statements: the diviner, he is told, '*reads me like an open book*', or again, '*he's extraordinary, I never tell him anything and he knows everything*'. But if he has ever accompanied peasants to the diviner's and sat in the waiting room during the consultations, the ethnographer knows that they never stopped talking: it's just that, as after a hypnotic trance, they do not remember.

So the diviner and his client have a common 'misknowledge' which is not the same as the simple complicity of sharing a secret: no winning of trust will ever make the persons concerned capable of explaining what the terms '*gift*' and '*seeing everything*' really mean, because the whole institution of divination depends on the fact that they do not want to know anything about it.

For anyone who wants to understand the meaning of this discourse, there is no other solution but to practise it oneself, to become one's own informant, to penetrate one's own amnesia, and to try and make explicit what one finds unstatable in oneself. For it is difficult to see how the native could have any interest in the project of unveiling what can go on existing only if it remains veiled; or for what purpose he would give up the symbolic benefits of such important resources.

*(I am well aware that there is a fundamental gulf between my present aims and those of my Bocage interlocutors. Until now, I have been content to state that the discourse of witchcraft is such that to gain access to it one must be in a position to sustain it oneself. And yet, it is one thing to have access to it – it was a memorable adventure which has marked me for my whole subsequent life – it is another thing to want to go on to develop its theory.)*

If you want to listen to and understand a diviner, there is therefore no other solution but to become his client, i.e. to tell him your desire and ask him to interpret it.[7] Like any native – or any desiring subject – the investigator is bound on this occasion to be afflicted by misknowledge: so for several months, however carefully I tried to take notes after each divination séance, a certain part of the consultation, always the same, was censored by amnesia; similarly, when a seer, who I was hoping would teach me the everyday tricks of divination saw the '*gift*' in me and gave me her life-story for interpretation, claiming she had nothing to teach me that I did not already know, I could not help being amazed.

Persistent amnesia, dumbfoundedness, the inability to reflect when faced by the seemingly unstatable – i.e. a vague perception that *something in this cannot be coped with* – this was my ordinary lot during the adventure.[8] It may be wondered how, at a certain point, I managed to surmount this inability, that is, try to get it out in words, to convert an adventure into a theoretical project. But this question cannot be answered simply by invoking one's duty towards the demands of the scientific approach, or one's debt to the scholarly institution which acts as patron: if that respect applies, it is somewhere else and in another manner. To have been engaged in the discourse of witchcraft beyond what can be required of an ethnographer in the ordinary practice of her profession poses first the problem of motive; what could have been my own desire to know; why was I personally involved in the ambition to give a solid basis to the 'social sciences', and why, in the case of divination for example, was I not content to resolve the issue by invoking the concept of '*gift*', or, even sooner, by accepting the findings of the folklorists.

So the distance necessary if one is to be able to theorize does not have to be established between the ethnographer and his 'object', i.e. the native. But of all the snares which might imperil our work, there are two we had learnt to avoid like the plague: that of agreeing to 'participate' in the native discourse, and that of succumbing to the temptations of subjectivism. Not only could I not possibly avoid them; it is by means of them that I was able to work out most of my ethnographic work. Whatever you may think of it, it must be granted that the masters' predictions do not always turn out to be true, which state that in such cases it becomes impossible to put any distance between oneself and the native or between oneself and oneself.

Anyway, I was never able to choose between subjectivism and the objective method as it was taught me, so long, that is, as I still wished to find an answer to my initial question – what are the people involved trying to shape out through a witchcraft crisis one? Working in this way has at least preserved me from one limitation regularly met by the objectivizing ethnographer and which is never emphasized, since it is taken for granted: I mean the ethnographer's dependence on a finite *corpus* of empirical observations and native texts collected in the field. This kind of ethnography meets any new question with the answer that it is included, or not, in the *corpus*; it can be verified, or not, in the empirical data – and of anything not referred to in the *corpus*, nothing can be asserted. In my case, the fact that Bocage peasants forced me to come up with a number of statements in the same way as they did (i.e. to be an encoder) enabled me to break away from the limits of the *corpus*; or, and this comes to the same thing, to include my own discourse in it. For the sort of question posed by comparative grammarians, I was able to substitute that posed by transformationalists: can this utterance be produced or not? Hazarding my own words in the presence of native decoders, I became able to discriminate accepted from unacceptable meaning whatever the utterance and whether or not it was produced during my stay in the field. The limits of ordinary ethnography are those of its *corpus*. In the case of the ethnography I was practising, the problem was, each time, to evaluate correctly the limits of my position in speech. But my having occupied at one time or another all the positions in this discourse, knowingly or not, or willingly or not, at least enables me to have a view on everything that is statable.

It is now time to give a little information about the position of the witch. No one, in the Bocage, calls himself a witch; it is not a position from which one can speak. A witch never admits his crimes, not even when he is delirious in a psychiatric hospital (this is considerably different from exotic witchcrafts). The witch is the person referred to by those who utter the discourse on witchcraft (bewitched and unwitchers), and he only figures in it as the subject of the statement. His victims claim that it is unnecessary for him to admit he is a witch, since his death speaks for him: everyone laughs at his funeral because he died in a significant way, carried off in only a few hours as a result of the diviner's curse, or neighing like the mare he had cast an evil spell on, and so on. This makes it highly unlikely that there are witches who actually cast evil spells, but this is surely not in the least necessary for the system to function.

# 35

# On Ethnographic Authority

*James Clifford*

## 1 On Ethnographic Authority

The 1724 frontispiece of Father Lafitau's *Moeurs des sauvages ameriquains* portrays the ethnographer as a young woman sitting at a writing table amid artifacts from the New World and from classical Greece and Egypt. The author is accompanied by two cherubs who assist in the task of comparison and by the bearded figure of Time, who points toward a tableau representing the ultimate source of the truths issuing from the writer's pen. The image toward which the young woman lifts her gaze is a bank of cloud where Adam, Eve, and the serpent appear. Above them stand the redeemed man and woman of the Apocalypse, on either side of a radiant triangle bearing the Hebrew script for *Yahweh*.

The frontispiece for Malinowski's *Argonauts of the Western Pacific* is a photograph with the caption "A Ceremonial Act of the Kula." A shell necklace is being offered to a Trobriand chief, who stands at the door of his dwelling. Behind the man presenting the necklace is a row of six bowing youths, one of them sounding a conch. All the figures stand in profile, their attention apparently concentrated on the rite of exchange, a real event of Melanesian life. But on closer inspection one of the bowing Trobrianders may be seen to be looking at the camera.

Lafitau's allegory is the less familiar: his author transcribes rather than originates. Unlike Malinowski's photo, the engraving makes no reference to ethnographic experience – despite Lafitau's five years of research among the Mohawks, research that has earned him a respected place among the fieldworkers of any generation. His account is presented not as the product of firsthand observation but of writing, in a crowded workshop. The frontispiece from *Argonauts*, like all photographs, asserts presence – that of the scene before the lens; it also suggests another presence – that of the ethnographer actively composing this fragment of Trobriand reality. Kula exchange, the subject of Malinowski's book, has been made perfectly visible, centered in the perceptual frame, while a participant's glance redirects our attention to the observational standpoint we share, as readers, with the ethnographer and his camera. The predominant mode of modern fieldwork authority is signaled: "You are there . . . because I was there."

This chapter traces the formation and breakup of ethnographic authority in twentieth-century social anthropology. It is not a complete account, nor is it based on a fully realized theory of ethnographic interpretation

James Clifford, "On Ethnographic Authority," pp. 21–35, 37–54 from James Clifford, *The Predicament of Culture: Twentieth-Century Ethnography, Literature, and Art* (Cambridge, MA: Harvard University Press, 1998). Copyright © 1988 by the President and Fellows of Harvard College.

and textuality.[1] Such a theory's contours are problematic, since the activity of cross-cultural representation is now more than usually in question. The present predicament is linked to the breakup and redistribution of colonial power in the decades after 1950 and to the echoes of that process in the radical cultural theories of the 1960s and 1970s. After the negritude movement's reversal of the European gaze, after anthropology's *crise de conscience* with respect to its liberal status within the imperial order, and now that the West can no longer present itself as the unique purveyor of anthropological knowledge about others, it has become necessary to imagine a world of generalized ethnography. With expanded communication and intercultural influence, people interpret others, and themselves, in a bewildering diversity of idioms – a global condition of what Mikhail Bakhtin (1953) called "heteroglossia."[2] This ambiguous, multivocal world makes it increasingly hard to conceive of human diversity as inscribed in bounded, independent cultures. Difference is an effect of inventive syncretism. In recent years works such as Edward Said's *Orientalism* (1978) and Paulin Hountondji's *Sur la "philosophie" africaine* (1977) have cast radical doubt on the procedures by which alien human groups can be represented without proposing systematic, sharply new methods or epistemologies. These studies suggest that while ethnographic writing cannot entirely escape the reductionist use of dichotomies and essences, it can at least struggle self-consciously to avoid portraying abstract, ahistorical "others." It is more than ever crucial for different peoples to form complex concrete images of one another, as well as of the relationships of knowledge and power that connect them; but no sovereign scientific method or ethical stance can guarantee the truth of such images. They are constituted – the critique of colonial modes of representation has shown at least this much – in specific historical relations of dominance and dialogue.

The experiments in ethnographic writing surveyed in this chapter do not fall into a clear reformist direction or evolution. They are ad hoc inventions and cannot be seen in terms of a systematic analysis of postcolonial representation. They are perhaps best understood as components of that "toolkit" of engaged theory recently recommended by Gilles Deleuze and Michel Foucault: "The notion of theory as a toolkit means (i) The theory to be constructed is not a system but an instrument, a *logic* of the specificity of power relations and the struggles around them; (ii) That this investigation can only be carried out step by step on the basis of reflection (which will necessarily be historical in some of its aspects) on given situations" (Foucault 1980:145; see also 1977:208). We may contribute to a practical reflection on cross-cultural representation by undertaking an inventory of the better, though imperfect, approaches currently at hand. Of these, ethnographic fieldwork remains an unusually sensitive method. Participant observation obliges its practitioners to experience, at a bodily as well as an intellectual level, the vicissitudes of translation. It requires arduous language learning, some degree of direct involvement and conversation, and often a derangement of personal and cultural expectations. There is, of course, a myth of fieldwork. The actual experience, hedged around with contingencies, rarely lives up to the ideal; but as a means for producing knowledge from an intense, intersubjective engagement, the practice of ethnography retains a certain exemplary status. Moreover, if fieldwork has for a time been identified with a uniquely Western discipline and a totalizing science of "anthropology," these associations are not necessarily permanent. Current styles of cultural description are historically limited and are undergoing important metamorphoses.

The development of ethnographic science cannot ultimately be understood in isolation from more general political-epistemological debates about writing and the representation of otherness. In this discussion, however, I have maintained a focus on professional anthropology, and specifically on ethnography since 1950.[3] The current crisis – or better, dispersion – of ethnographic authority makes it possible to mark off a rough period, bounded by the years 1900 and 1960, during which a new conception of field research established itself as the norm for European and American anthropology. Intensive fieldwork, pursued by university-trained specialists, emerged as a privileged, sanctioned source of data about exotic peoples. It is not a question here of the dominance of a single research method. "Intensive" ethnography has been variously defined. (Compare Griaule 1957 with Malinowski 1922:chap. 1). Moreover, the hegemony of fieldwork was established earlier and more thoroughly in the United States and in England than in France. The early examples of Franz Boas and the Torres Straits expedition were matched only belatedly by the founding of the Institut d'Ethnologie in 1925 and the much-publicized Mission Dakar-Djibouti of 1932 (Karady 1982; Jamin 1982; Stocking 1983). Nevertheless, by the mid-1930s one can fairly speak of a developing international consensus: valid anthropological abstractions were to be based, wherever possible, on intensive cultural descriptions by qualified scholars. By this point the new style had been made popular, institutionalized, and embodied in specific textual practices.

It has recently become possible to identify and take a certain distance from these conventions.[4] If ethnography produces cultural interpretations through intense research experiences, how is unruly experience transformed into an authoritative written account? How, precisely, is a garrulous, overdetermined cross-cultural encounter shot through with power relations and personal cross-purposes circumscribed as an adequate version of a more or less discrete "other world" composed by an individual author?

In analyzing this complex transformation one must bear in mind the fact that ethnography is, from beginning to end, enmeshed in writing. This writing includes, minimally, a translation of experience into textual form. The process is complicated by the action of multiple subjectivities and political constraints beyond the control of the writer. In response to these forces ethnographic writing enacts a specific strategy of authority. This strategy has classically involved an unquestioned claim to appear as the purveyor of truth in the text. A complex cultural experience is enunciated by an individual: *We the Tikopia by* Raymond Firth; *Nous avons mangé la forêt* by Georges Condominas; *Coming of Age in Samoa* by Margaret Mead; *The Nuer* by E. E. Evans-Pritchard.

The discussion that follows first locates this authority historically in the development of a twentieth-century science of participant observation. It then proceeds to a critique of underlying assumptions and a review of emerging textual practices. Alternate strategies of ethnographic authority may be seen in recent experiments by ethnographers who self-consciously reject scenes of cultural representation in the style of Malinowski's frontispiece. Different secular versions of Lafitau's crowded scriptorial workshop are emerging. In the new paradigms of authority the writer is no longer fascinated by transcendent figures – a Hebrew–Christian deity or its twentieth-century replacements, Man and Culture. Nothing remains of the heavenly tableau except the anthropologist's scumbled image in a mirror. The silence of the ethnographic workshop has been broken – by insistent, heteroglot voices, by the scratching of other pens.[5]

At the close of the nineteenth century nothing guaranteed, a priori, the ethnographer's status as the best interpreter of native life – as opposed to the traveler, and especially the missionary and administrator, some of whom had been in the field far longer and had better research contacts and linguistic skills. The development of the fieldworker's image in America, from Frank Hamilton Cushing (an oddball) to Margaret Mead (a national figure) is significant. During this period a particular form of authority was created – an authority both scientifically validated and based on a unique personal experience. During the 1920s Malinowski played a central role in establishing credit for the fieldworker, and we should recall in this light his attacks on the competence of competitors in the field. For example the colonial magistrate Alex Rentoul, who had the temerity to contradict science's findings concerning Trobriand conceptions of paternity, was excommunicated in the pages of *Man* for his unprofessional "police court perspective" (see Rentoul 1931a,b; Malinowski 1932). The attack on amateurism in the field was pressed even further by A. R. Radcliffe-Brown, who, as Ian Langham has shown, came to epitomize the scientific professional, discovering rigorous social laws (Langham 1981:chap. 7). What emerged during the first half of the twentieth century with the success of professional fieldwork was a new fusion of general theory and empirical research, of cultural analysis with ethnographic description.

The fieldworker-theorist replaced an older partition between the "man on the spot" (in James Frazer's words) and the sociologist or anthropologist in the metropole. This division of labor varied in different national traditions. In the United States for example Morgan had personal knowledge of at least some of the cultures that were raw material for his sociological syntheses; and Boas rather early on made intensive fieldwork the sine qua non of serious anthropological discourse. In general, however, before Malinowski, Radcliffe-Brown, and Mead had successfully established the norm of the university-trained scholar testing and deriving theory from firsthand research, a rather different economy of ethnographic knowledge prevailed. For example *The Melanesians* (1891) by R. H. Codrington is a detailed compilation of folklore and custom, drawn from his relatively long term of research as an evangelist and based on intensive collaboration with indigenous translators and informants. The book is not organized around a fieldwork "experience," nor does it advance a unified interpretive hypothesis, functional, historical, or otherwise. It is content with low-level generalizations and the amassing of an eclectic range of information. Codrington is acutely aware of the incompleteness of his knowledge, believing that real understanding of native life begins only after a decade or so of experience and study (pp. vi–vii). This understanding of the difficulty of grasping the world of alien peoples – the many years of learning and unlearning needed, the problems of acquiring thorough linguistic competence – tended to dominate the work of Codrington's generation. Such assumptions would soon be challenged by the more confident cultural relativism of the Malinowskian model. The new fieldworkers sharply distinguished themselves from the earlier "men on the spot" – the missionary, the administrator, the trader, and the traveler – whose knowledge of indigenous peoples, they argued, was not informed by the best scientific hypotheses or a sufficient neutrality.

Before the emergence of professional ethnography, writers such as J. F. McLennan, John Lubbock, and E. B. Tylor had attempted to control the quality of the reports on which their anthropological syntheses were based. They did this by means of the guidelines of *Notes and Queries* and, in Tylor's case, by cultivating long-term working relations with sophisticated researchers in the field such as the missionary Lorimer Fison. After 1883, as newly appointed reader in anthropology at Oxford, Tylor worked to encourage the systematic gathering of ethnographic data by qualified professionals. The United States Bureau of Ethnology, already committed to the undertaking, provided a model. Tylor was active in founding a committee on the Northwestern Tribes of Canada. The committee's first agent in the field was the nineteen-year-veteran missionary among the Ojibwa, E. F. Wilson. He was replaced before long by Boas, a physicist in the process of turning to professional ethnography. George Stocking has persuasively argued that the replacement of Wilson by Boas "marks the beginning of an important phase in the development of British ethnographic method: the collection of data by academically trained natural scientists defining themselves as anthropologists, and involved also in the formulation and evaluation of anthropological theory" (1983:74). With Boas' early survey work and the emergence in the 1890s of other natural-scientist fieldworkers such as A. C. Haddon and Baldwin Spencer, the move toward professional ethnography was under way. The Torres Straits expedition of 1899 may be seen as a culmination of the work of this "intermediate generation," as Stocking calls them. The new style of research was clearly different from that of missionaries and other amateurs in the field, and part of a general trend since Tylor "to draw more closely together the empirical and theoretical components of anthropological inquiry" (1983:72).

The establishment of intensive participant observation as a professional norm, however, would have to await the Malinowskian cohort. The "intermediate generation" of ethnographers did not typically live in a single locale for a year or more, mastering the vernacular and undergoing a personal learning experience comparable to an initiation. They did not speak as cultural insiders but retained the natural scientist's documentary, observational stance. The principal exception before the third decade of the century, Frank Hamilton Cushing, remained an isolated instance. As Curtis Hinsley has suggested, Cushing's long firsthand study of the Zunis, his quasi-absorption into their way of life, "raised problems of verification and accountability... A community of scientific anthropology on the model of other sciences required a common language of discourse, channels of regular communication, and at least minimal consensus on judging method" (1983:66). Cushing's intuitive, excessively personal understanding of the Zuni could not confer scientific authority.

Schematically put, before the late nineteenth century the ethnographer and the anthropologist, the describer-translator of custom and the builder of general theories about humanity, were distinct. (A clear sense of the *tension* between ethnography and anthropology is important in correctly perceiving the recent, and perhaps temporary, conflation of the two projects.) Malinowski gives us the image of the new "anthropologist" – squatting by the campfire; looking, listening, and questioning; recording and interpreting Trobriand life. The literary charter of this new authority is the first chapter of *Argonauts*, with its prominently displayed photographs of the ethnographer's tent pitched among Kiriwinian dwellings. The sharpest methodological justification for the new mode is to be found in Radcliffe-Brown's *Andaman Islanders* (1922). The two books were published within a year of each other. And although their authors developed quite different fieldwork styles and visions of cultural science, both early texts provide explicit arguments for the special authority of the ethnographer-anthropologist.

Malinowski, as his notes for the crucial introduction to *Argonauts* show, was greatly concerned with the rhetorical problem of convincing his readers that the facts he was putting before them were objectively acquired, not subjective creations (Stocking 1983:105). Moreover, he was fully aware that "in Ethnography, the distance is often enormous between the brute material of information – as it is presented to the student in his own observations, in native statement, in the kaleidoscope of tribal life – and the final authoritative presentation of the results" (Malinowski 1922:3–4). Stocking has nicely analyzed the various literary artifices of *Argonauts* (its engaging narrative constructs, use of the active voice in the "ethnographic present," illusive dramatizations of the author's participation in scenes of Trobriand life), techniques Malinowski used so that "his own experience of the natives' experience [might] become the reader's experience as well" (Stocking 1983:106; see also Payne 1981). The problems of verification and accountability that had relegated Cushing to the professional margin were very much on Malinowski's mind. This anxiety is reflected in the mass of data contained in *Argonauts*, its sixty-six photographic plates, the now rather curious "Chronological List of Kula Events Witnessed by the Writer," the constant alternation between impersonal description of typical behavior and statements on the order of "I witnessed..." and "Our party, sailing from the North..."

*Argonauts* is a complex narrative simultaneously of Trobriand life and ethnographic fieldwork. It is archetypical of the generation of ethnographies that successfully established the scientific validity of participant observation. The story of research built into *Argonauts*, into Mead's popular work in Samoa, and into *We the Tikopia* became an implicit narrative underlying all professional reports on exotic worlds. If subsequent ethnographies did not need to include developed fieldwork accounts, it was because such accounts were assumed, once a statement was made on the order of, for example, Godfrey Lienhardt's single sentence at the beginning of *Divinity and Experience* (1961:vii): "This book is based upon two years' work among the Dinka, spread over the period of 1947–1950."

In the 1920s the new fieldworker-theorist brought to completion a powerful new scientific and literary genre, the ethnography, a synthetic cultural description based on participant observation (Thornton 1983). The new style of representation depended on institutional and methodological innovations circumventing the obstacles to rapid knowledge of other cultures that had preoccupied the best representatives of Codrington's generation. These may be briefly summarized.

First, the persona of the fieldworker was validated, both publicly and professionally. In the popular domain, visible figures such as Malinowski, Mead, and Marcel Griaule communicated a vision of ethnography as both scientifically demanding and heroic. The professional ethnographer was trained in the latest analytic techniques and modes of scientific explanation. This conferred an advantage over amateurs in the field: the professional could claim to get to the heart of a culture more quickly, grasping its essential institutions and structures. A prescribed attitude of cultural relativism distinguished the fieldworker from missionaries, administrators, and others whose view of natives was, presumably, less dispassionate, who were preoccupied with the problems of government or conversion. In addition to scientific sophistication and relativist sympathy, a variety of normative standards for the new form of research emerged: the fieldworker was to live in the native village, use the vernacular, stay a sufficient (but seldom specified) length of time, investigate certain classic subjects, and so on.

Second, it was tacitly agreed that the new-style ethnographer, whose sojourn in the field seldom exceeded two years, and more frequently was much shorter, could efficiently "use" native languages without "mastering" them. In a significant article of 1939 Margaret Mead argued that the ethnographer following the Malinowskian prescription to avoid interpreters and to conduct research in the vernacular did not, in fact, need to attain "virtuosity" in native tongues, but could "use" the vernacular to ask questions, maintain rapport, and generally get along in the culture while obtaining good research results in particular areas of concentration. This in effect justified her own practice, which featured relatively short stays and a focus on specific domains such as childhood or "personality," foci that would function as "types" for a cultural synthesis. Her attitude toward language "use" was broadly characteristic of an ethnographic generation that could, for example, credit as authoritative a study called *The Nuer* that was based on only eleven months of very difficult research. Mead's article provoked a sharp response from Robert Lowie (1940), writing from the older Boasian tradition, more philological in its orientation. But his was a rear-guard action; the point had been generally established that valid research could, in practice, be accomplished on the basis of one or two years' familiarity with a foreign vernacular (even though, as Lowie suggested, no one would credit a translation of Proust that was based on an equivalent knowledge of French).

Third, the new ethnography was marked by an increased emphasis on the power of observation. Culture was construed as an ensemble of characteristic behaviors, ceremonies, and gestures susceptible to recording and explanation by a trained onlooker. Mead pressed this point furthest (indeed, her own powers of visual analysis were extraordinary). As a general trend the participant-*observer* emerged as a research norm. Of course successful fieldwork mobilized the fullest possible range of interactions, but a distinct primacy was accorded to the visual: interpretation was tied to description. After Malinowski a general suspicion of "privileged informants" reflected this systematic preference for the (methodical) observations of the ethnographer over the (interested) interpretations of indigenous authorities.

Fourth, certain powerful theoretical abstractions promised to help academic ethnographers "get to the heart" of a culture more rapidly than someone undertaking, for example, a thorough inventory of customs and belief. Without spending years getting to know natives, their complex languages and habits, in intimate detail, the researcher could go after selected data that would yield a central armature or structure of the cultural whole. Rivers' "genealogical method," followed by Radcliffe-Brown's model of "social structure," provided this sort of shortcut. One could, it seemed, elicit kin terms without a deep understanding of local vernacular, and the range of necessary contextual knowledge was conveniently limited.

Fifth, since culture, seen as a complex whole, was always too much to master in a short research span, the new ethnographer intended to focus thematically on particular institutions. The aim was not to contribute to a complete inventory or description of custom but rather to get at the whole through one or more of its parts. I have noted the privilege given for a time to social structure. An individual life cycle, a ritual complex like the Kula ring or the Naven ceremony, could also serve, as could categories of behavior like economics, politics, and so on. In the predominantly synecdochic rhetorical stance of the new ethnography, parts were assumed to be microcosms or analogies of wholes. This setting of institutional foregrounds against cultural backgrounds in the portrayal of a coherent world lent itself to realist literary conventions.

Sixth, the wholes thus represented tended to be synchronic, products of short-term research activity. The intensive fieldworker could plausibly sketch the contours of an "ethnographic present" – the cycle of a year, a ritual series, patterns of typical behavior. To introduce long-term historical inquiry would have impossibly complicated the task of the new-style fieldwork. Thus, when Malinowski and Radcliffe-Brown established their critique of the "conjectural history" of the diffusionists, it was all too easy to exclude diachronic processes as objects of fieldwork, with consequences that have by now been sufficiently denounced.

These innovations served to validate an efficient ethnography based on scientific participant observation. Their combined effect can be seen in what may well be the tour de force of the new ethnography, Evans-Pritchard's study *The Nuer*, [first] published in 1940. Based on eleven months of research conducted – as the book's remarkable introduction tells us – in almost impossible conditions, Evans-Pritchard nonetheless was able to compose a classic. He arrived in Nuerland on the heels of a punitive military expedition and at the urgent request of the government of the Anglo-Egyptian Sudan. He was the object of constant and intense suspicion. Only in the final few months could he converse at all effectively with informants, who, he tells us, were skilled at evading his questions. In the circumstances his monograph is a kind of miracle.

While advancing limited claims and making no secret of the constraints on his research, Evans-Pritchard manages to present his study as a demonstration of the effectiveness of theory. He focuses on Nuer political and social "structure," analyzed as an abstract set of relations between territorial segments, lineages, age sets, and other more fluid groups. This analytically derived ensemble is portrayed against an "ecological" backdrop composed of migratory patterns, relationships with cattle, notions of time and space. Evans-Pritchard sharply distinguishes his method from what he calls "haphazard" (Malinowskian) documentation. *The Nuer* is not an extensive compendium of observations and vernacular texts in the style of Malinowski's *Argonauts* and *Coral Gardens*. Evans-Pritchard argues rigorously that "facts can only be selected and arranged in the light of theory." The frank abstraction of a political-social structure offers the necessary framework. If I am accused of describing facts as exemplifications of my theory, he then goes on to note, I have been understood (1969:261).

In *The Nuer* Evans-Pritchard makes strong claims for the power of scientific abstraction to focus research and arrange complex data. The book often presents itself as an argument rather than a description, but not consistently: its theoretical argument is surrounded by skillfully observed and narrated evocations and interpretations of Nuer life. These passages function rhetorically as more than simple "exemplification," for they effectively implicate readers in the complex subjectivity of participant observation. This may be seen in a characteristic paragraph, which progresses through a series of discontinuous discursive positions:

> It is difficult to find an English word that adequately describes the social position of *diel* in a tribe. We have called them aristocrats, but do not wish to imply that Nuer regard them as of superior rank, for, as we have emphatically declared, the idea of a man lording it over others is repugnant to them. On the whole – we will qualify the statement later – the *diel* have prestige rather than rank and influence rather than power. If you are a *dil* of the tribe in which you live you are more than a simple tribesman. You are one of the owners of the country, its village sites, its pastures, its fishing pools and wells. Other people live there by virtue of marriage into your clan, adoption into your lineage, or of some other social tie. You are a leader of the tribe and the spear-name of your clan is invoked when the tribe goes to war. Whenever there is a *dil* in the village, the village clusters around him as

a herd of cattle clusters around its bull. (1969:215).

The first three sentences are presented as an argument about translation, but in passing they attribute to "Nuer" a stable set of attitudes. (I will have more to say later about this style of attribution.) Next, in the four sentences beginning "If you are a *dil*...," the second-person construction brings together reader and native in a textual participation. The final sentence, offered as a direct description of a typical event (which the reader now assimilates from the standpoint of a participant-observer), evokes the scene by means of Nuer cattle metaphors. In the paragraph's eight sentences an argument about translation passes through a fiction of participation to a metaphorical fusion of external and indigenous cultural descriptions. The subjective joining of abstract analysis and concrete experience is accomplished.

Evans-Pritchard would later move away from the theoretical position of *The Nuer*, rejecting its advocacy of "social structure" as a privileged framework. Indeed each of the fieldwork "shortcuts" I enumerated earlier was and remains contested. Yet by their deployment in different combinations, the authority of the academic fieldworker-theorist was established in the years between 1920 and 1950. This peculiar amalgam of intense personal experience and scientific analysis (understood in this period as both "rite of passage" and "laboratory") emerged as a method: participant observation. Though variously understood, and now disputed in many quarters, this method remains the chief distinguishing feature of professional anthropology. Its complex subjectivity is routinely reproduced in the writing and reading of ethnographies.

"Participant observation" serves as shorthand for a continuous tacking between the "inside" and "outside" of events: on the one hand grasping the sense of specific occurrences and gestures empathetically, on the other stepping back to situate these meanings in wider contexts. Particular events thus acquire deeper or more general significance, structural rules, and so forth. Understood literally, participant observation is a paradoxical, misleading formula, but it may be taken seriously if reformulated in hermeneutic terms as a dialectic of experience and interpretation. This is how the method's most persuasive recent defenders have restated it, in the tradition that leads from Wilhelm Dilthey, via Max Weber, to "symbols and meanings" anthropologists like Clifford Geertz. Experience and interpretation have, however, been accorded different emphases when presented as claims to authority. In recent years there has been a marked shift of emphasis from the former to the latter. This section and the one that follows will explore the rather different claims of experience and interpretation as well as their evolving interrelation.

The growing prestige of the fieldworker-theorist downplayed (without eliminating) a number of processes and mediators that had figured more prominently in previous methods. We have seen how language mastery was defined as a level of use adequate for amassing a discrete body of data in a limited period of time. The tasks of textual transcription and translation, along with the crucial dialogical role of interpreters and "privileged informants," were relegated to a secondary, sometimes even despised status. Fieldwork was centered in the *experience* of the participant-observing scholar. A sharp image, or narrative, made its appearance – that of an outsider entering a culture, undergoing a kind of initiation leading to "rapport" (minimally acceptance and empathy, but usually implying something akin to friendship). Out of this experience emerged, in unspecified ways, a representational text written by the participant-observer. As we shall see, this version of textual production obscures as much as it reveals. But it is worth taking seriously its principal assumption: that the experience of the researcher can serve as a unifying source of authority in the field.

[...]

Precisely because it is hard to pin down, "experience" has served as an effective guarantee of ethnographic authority. There is, of course, a telling ambiguity in the term. Experience evokes a participatory presence, a sensitive contact with the world to be understood, a rapport with its people, a concreteness of perception. It also suggests a cumulative, deepening knowledge ("her ten years' experience of New Guinea"). The senses work together to authorize an ethnographer's real but ineffable feel or flair for "his" or "her" people. It is worth noting, however, that this "world," when conceived as an experiential creation, is subjective, not dialogical or intersubjective. The ethnographer accumulates personal knowledge of the field (the possessive form *my people* has until recently been familiarly used in anthropological circles, but the phrase in effect signifies "my experience").

It is understandable, given their vagueness, that experiential criteria of authority – unexamined beliefs in the "method" of participant observation, in the power of rapport, empathy, and so on – have come under criticism by hermeneutically sophisticated anthropologists. The second moment in the dialectic of experience and interpretation has received increasing attention and elaboration (see, for example, Geertz 1973, 1976; Rabinow and Sullivan 1979; Winner 1976; Sperber 1981). Interpretation, based on a philological model of textual "reading," has emerged as a sophisticated alternative to the now apparently naive claims for experiential authority. Interpretive anthropology demystifies much of what had previously passed unexamined in the construction of ethnographic narratives, types, observations, and descriptions. It contributes to an increasing visibility of the creative (and in a broad sense poetic) processes by which "cultural" objects are invented and treated as meaningful.

What is involved in looking at culture as an assemblage of texts to be interpreted? A classic account has been provided by Paul Ricoeur, in his essay "The Model of Text: Meaningful Action Considered as a Text" (1971). Clifford Geertz in a number of stimulating and subtle discussions has adapted Ricoeur's theory to anthropological fieldwork (1973:chap. 1). "Textualization" is understood as a prerequisite to interpretation, the constitution of Dilthey's "fixed expressions." It is the process through which unwritten behavior, speech, beliefs, oral tradition, and ritual come to be marked as a corpus, a potentially meaningful ensemble separated out from an immediate discursive or performative situation. In the moment of textualization this meaningful corpus assumes a more or less stable relation to a context; and we are familiar with the end result of this process in much of what counts as ethnographic thick description. For example, we say that a certain institution or segment of behavior is typical of, or a communicative element within, a surrounding culture, as when Geertz's famous cockfight (1973:chap. 15) becomes an intensely significant locus of Balinese culture. Fields of synecdoches are created in which parts are related to wholes, and by which the whole – what we often call culture – is constituted.

Ricoeur does not actually privilege part-whole relations and the specific sorts of analogies that constitute functionalist or realist representations. He merely posits a necessary relation between text and "world." A world cannot be apprehended directly; it is always inferred on the basis of its parts, and the parts must be conceptually and perceptually cut out of the flux of experience. Thus, textualization generates sense through a circular movement that isolates and then contextualizes a fact or event in its englobing reality. A familiar mode of authority is generated that claims to represent discrete, meaningful worlds. Ethnography is the interpretation of cultures.

A second key step in Ricoeur's analysis is his account of the process by which "discourse" becomes text. Discourse, in Emile Benveniste's classic discussion (1971:217–30), is a mode of communication in which the presence of the speaking subject and of the immediate situation of communication are intrinsic. Discourse is marked by pronouns (pronounced or implied) *I* and *you*, and by deictic indicators – *this, that, now*, and so on – that signal the present instance of discourse rather than something beyond it. Discourse does not transcend the specific occasion in which a subject appropriates the resources of language in order to communicate dialogically. Ricoeur argues that discourse cannot be interpreted in the open-ended, potentially public way in which a text is "read." To understand discourse "you had to have been there," in the presence of the discoursing subject. For discourse to become text it must become "autonomous," in Ricoeur's terms, separated from a specific utterance and authorial intention. Interpretation is not interlocution. It does not depend on being in the presence of a speaker.

The relevance of this distinction for ethnography is perhaps too obvious. The ethnographer always ultimately departs, taking away texts for later interpretation (and among those "texts" taken away we can include memories – events patterned, simplified, stripped of immediate context in order to be interpreted in later reconstruction and portrayal). The text, unlike discourse, can travel. If much ethnographic writing is produced in the field, actual composition of an ethnography is done elsewhere. Data constituted in discursive, dialogical conditions are appropriated only in textualized forms. Research events and encounters become field notes. Experiences become narratives, meaningful occurrences, or examples.

This translation of the research experience into a textual corpus separate from its discursive occasions of production has important consequences for ethnographic authority. The data thus reformulated need no longer be understood as the communication of specific persons. An informant's explanation or description of custom need not be cast in a form that includes the message "so and so said this." A textualized ritual or event is no longer closely linked to the production of that event by specific actors. Instead these texts become evidences of an englobing context, a "cultural" reality. Moreover, as specific authors and actors are severed from their productions, a generalized "author" must be invented to account for the world or context within which the texts are fictionally relocated. This generalized author goes under a variety of names: the native point of view, "the Trobrianders," "the Nuer," "the Dogon," as these and similar phrases appear in ethnographies. "The Balinese" function as author of Geertz's textualized cockfight.

The ethnographer thus enjoys a special relationship with a cultural origin or "absolute subject" (Michel-Jones 1978:14). It is tempting to compare the ethnographer with the literary interpreter (and this comparison is increasingly commonplace) – but more specifically with the traditional critic, who sees the task at hand as locating the unruly meanings of a text in a single coherent intention. By representing the Nuer, the Trobrianders, or the Balinese as whole subjects, sources of a meaningful intention, the ethnographer transforms the research situation's ambiguities and diversities of meaning into an integrated portrait. It is important, though, to notice what has dropped out of sight. The research process is separated from the texts it generates and from the fictive world they are made to call up. The actuality of discursive situations and individual interlocutors is filtered out. But informants – along with field notes – are crucial intermediaries, typically excluded from authoritative ethnographies. The dialogical, situational aspects of ethnographic interpretation tend to be banished from the final representative text. Not entirely banished, of course; there exist approved *topoi* for the portrayal of the research process.

We are increasingly familiar with the separate fieldwork account (a subgenre that still tends to be classified as subjective, "soft", or unscientific), but even within classic ethnographies, more-or-less stereotypic "fables of rapport" narrate the attainment of full participant-observer status. These fables may be told elaborately or in passing, naively or ironically. They normally portray the ethnographer's early ignorance, misunderstanding, lack of contact – frequently a sort of childlike status within the culture. In the *Bildungsgeschichte* of the ethnography these states of innocence or confusion are replaced by adult, confident, disabused knowledge. We may cite again Geertz's cockfight, where an early alienation from the Balinese, a confused "nonperson" status, is transformed by the appealing fable of the police raid with its show of complicity (1973:412–17). The anecdote establishes a presumption of connectedness, which permits the writer to function in his subsequent analyses as an omnipresent, knowledgeable exegete and spokesman. This interpreter situates the ritual sport as a text in a contextual world and brilliantly "reads" its cultural meanings. Geertz's abrupt disappearance into his rapport – the quasi-invisibility of participant observation – is paradigmatic. Here he makes use of an established convention for staging the attainment of ethnographic authority. As a result, we are seldom made aware of the fact that an essential part of the cockfight's construction as a text is dialogical – the author's talking face to face with particular Balinese rather than reading culture "over the[ir] shoulders" (1973:452).

Interpretive anthropology, by viewing cultures as assemblages of texts, loosely and sometimes contradictorily united, and by highlighting the inventive poesis at work in all collective representations, has contributed significantly to the defamiliarization of ethnographic authority. In its mainstream realist strands, however, it does not escape the general strictures of those critics of "colonial" representation who, since 1950, have rejected discourses that portray the cultural realities of other peoples without placing their own reality in jeopardy. In Michel Leiris' early critiques, by way of Jacques Maquet, Talal Asad, and many others, the unreciprocal quality of ethnographic interpretation has been called to account (Leiris 1950; Maquet 1964; Asad 1973). Henceforth neither the experience nor the interpretive activity of the scientific researcher can be considered innocent. It becomes necessary to conceive ethnography not as the experience and interpretation of a circumscribed "other" reality, but rather as a constructive negotiation involving at least two, and usually more, conscious, politically significant subjects. Paradigms of experience and interpretation are yielding to discursive paradigms of dialogue and polyphony. The remaining sections of this chapter will survey these emergent modes of authority.

A discursive model of ethnographic practice brings into prominence the intersubjectivity of all speech, along with its immediate performative context. Benveniste's work on the constitutive role of personal pronouns and deixis highlights just these dimensions. Every use of *I* presupposes a *you*, and every instance of discourse is immediately linked to a specific, shared situation: no discursive meaning, then, without interlocution and context. The relevance of this emphasis for ethnography is evident. Fieldwork is significantly composed of language events; but language, in Bakhtin's words, "lies on the borderline between oneself and the other. The word in language is half someone else's." The Russian critic urges a rethinking of language in terms of specific discursive situations: "There are," he writes, "no 'neutral' words and forms – words and forms that can belong to 'no one'; language has been completely taken over, shot through with intentions and accents." The words of ethnographic writing, then, cannot be construed as monological, as the authoritative statement about, or interpretation of, an abstracted, textualized reality. The language of ethnography is shot through with other subjectivities and specific contextual overtones, for all language, in Bakhtin's view, is "a concrete heteroglot conception of the world" (1953:293).

Forms of ethnographic writing that present themselves in a "discursive" mode tend to be concerned with the representation of research contexts and situations of interlocution. Thus a book like Paul Rabinow's *Reflections on Fieldwork in Morocco* (1977) is concerned with the representation of a specific research situation (a series of constraining times and places) and (in somewhat fictionalized form) a sequence of individual interlocutors. Indeed an entire new subgenre of "fieldwork accounts" (of which Rabinow's is one of the most trenchant) may be situated within the discursive paradigm of ethnographic writing. Jeanne Favret-Saada's *Les mots, la mort, les sorts* (1997) is an insistent, self-conscious experiment with ethnography in a discursive mode.[6] She argues that the event of interlocution always assigns to the ethnographer a specific position in a web of intersubjective relations. There is no neutral standpoint in the power-laden field of discursive positionings, in a shifting matrix of relationships, of *I*'s and *you*'s.

A number of recent works have chosen to present the discursive processes of ethnography in the form of a dialogue between two individuals. Camille Lacoste-Dujardin's *Dialogue des femmes en ethnologie* (1977), Jean-Paul Dumont's *The Headman and I* (1978), and Marjorie Shostak's *Nisa: The Life and Words of a !Kung Woman* (1981) are noteworthy examples. The dialogical mode is advocated with considerable sophistication in two other texts. The first, Kevin Dwyer's theoretical reflections on the "dialogic of ethnology" springs from a series of interviews with a key informant and justifies Dwyer's decision to structure his ethnography in the form of a rather literal record of these exchanges (1977, 1979, 1982). The second work is Vincent Crapanzano's more complex *Tuhami: Portrait of a Moroccan*, another account of a series of interviews that rejects any sharp separation of interpreting self from a textualized other (1980; see also 1977). Both Dwyer and Crapanzano locate ethnography in a process of dialogue where interlocutors actively negotiate a shared vision of reality. Crapanzano argues that this mutual construction must be at work in any ethnographic encounter, but that participants tend to assume that they have simply acquiesced to the reality of their counterpart. Thus, for example, the ethnographer of the Trobriand Islanders does not openly concoct a version of reality in collaboration with his informants but rather interprets the "Trobriand point of view." Crapanzano and Dwyer offer sophisticated attempts to break with this literary–hermeneutical convention. In the process the ethnographer's authority as narrator and interpreter is altered. Dwyer proposes a hermeneutics of "vulnerability," stressing the ruptures of fieldwork, the divided position and imperfect control of the ethnographer. Both Crapanzano and Dwyer seek to represent the research experience in ways that tear open the textualized fabric of the other, and thus also of the interpreting self.[7] (Here etymologies are evocative: the word *text* is related, as is well know, to weaving, *vulnerability* to rending or wounding, in this instance the opening up of a closed authority.)

The model of dialogue brings to prominence precisely those discursive – circumstantial and intersubjective – elements that Ricoeur had to exclude from his model of the text. But if interpretive authority is based on the exclusion of dialogue, the reverse is also true: a purely dialogical authority would repress the inescapable fact of textualization. While ethnographies cast as encounters between two individuals may successfully dramatize the intersubjective give-and-take of fieldwork and introduce a counterpoint of authorial voices, they remain *representations* of dialogue. As texts they may not be dialogical in structure, for as Steven Tyler (1981) points out, although Socrates appears as a decentered participant in his encounters, Plato retains full control of the dialogue. This displacement but not elimination of monological authority is characteristic of any approach that portrays the ethnographer as a discrete character in the fieldwork narrative. Moreover, there is a frequent tendency in fictions of dialogue for the ethnographer's counterpart to appear as a representative of his or her culture – a type, in the language of traditional realism – through which general social processes are revealed.[8] Such a portrayal reinstates the synecdochic interpretive authority by which the ethnographer reads text in relation to context, thereby constituting a meaningful "other" world. If it is difficult for dialogical portrayals to escape typifying procedures, they can, to a significant degree, resist the pull toward authoritative representation of the other. This depends on their ability fictionally to maintain the strangeness of the other voice and to hold in view the specific contingencies of the exchange.

To say that an ethnography is composed of discourses and that its different components are dialogically related is not to say that its textual form should be that of a literal dialogue. Indeed as Crapanzano recognizes in *Tuhami*, a third participant, real or imagined, must function as mediator in any encounter between two individuals (1980:147–51). The fictional dialogue is in fact a condensation, a simplified representation of complex multivocal processes. An alternative way of representing this discursive complexity is to understand the overall course of the research as an ongoing negotiation. The case of Marcel Griaule and the Dogon is well known and particularly clear-cut. Griaule's account of his instruction in Dogon cosmological wisdom, *Dieu d'eau* (1948), was an early exercise in dialogical ethnographic narration. Beyond this specific interlocutory occasion, however, a more complex process was at work, for it is apparent that the content and timing of the Griaule team's longterm research, spanning decades, was closely monitored and significantly shaped by Dogon tribal authorities. This is no longer news. Many ethnographers have commented on the ways, both subtle and blatant, in which their research was directed or circumscribed by their informants. In his provocative discussion of this issue Ioan Lewis (1973) even calls anthropology a form of "plagiarism."

The give-and-take of ethnography is clearly portrayed in a 1980 study noteworthy for its presentation within a single work of both an interpreted other reality *and* the research process itself: Renato Rosaldo's *Ilongot Headhunting*. Rosaldo arrives in the Philippine highlands intent on writing a synchronic study of social structure; but again and again, over his objections, he is forced to listen to endless Ilongot narratives of local history. Dutifully, dumbly, in a kind of bored trance he transcribes these stories, filling notebook after notebook with what he considers disposable texts. Only after leaving the field, and after a long process of reinterpretation (a process made manifest in the ethnography), does he realize that these obscure tales have in fact provided him with his final topic, the culturally distinctive Ilongot sense of narrative and history. Rosaldo's experience of what might be called "directed writing" sharply poses a fundamental question: Who is actually the author of field notes?

The issue is a subtle one and deserves systematic study. But enough has been said to make the general point that indigenous control over knowledge gained in the field can be considerable, and even determining. Current ethnographic writing is seeking new ways to represent adequately the authority of informants. There are few models to look to, but it is worth reconsidering the older textual compilations of Boas, Malinowski, Leenhardt, and others. In these works the ethnographic genre has not coalesced around the modern interpretational monograph closely identified with a personal fieldwork experience. We can contemplate an ethnographic mode that is not yet authoritative in those specific ways that are now politically and epistemologically in question. These older assemblages include much that is actually or all but written by informants. One thinks of the role of George Hunt in Boas' ethnography, or of the fifteen "transcripteurs" listed in Leenhardt's *Documents néo-calédoniens* (1932).[9]

Malinowski is a complex transitional case. His ethnographies reflect the incomplete coalescence of the modern monograph. If he was centrally responsible for the welding of theory and description into the authority of the professional fieldworker, Malinowski nonetheless included material that did not directly support his own all-too-clear interpretive slant. In the many dictated myths and spells that fill his books, he published much data that he admittedly did not understand. The result was an open text subject to multiple reinterpretations. It is worth comparing such older compendiums with the recent model ethnography, which cites evidence to support a focused interpretation but little else.[10] In the modern, authoritative monograph there are, in effect, no strong voices present except that of the writer; but in *Argonauts* (1922) and *Coral Gardens* (1935) we read page after page of magical spells, none in any essential sense in the ethnographer's words. These dictated texts in all but their physical inscription are written by specific unnamed Trobrianders. Indeed any continuous ethnographic exposition routinely folds into itself a diversity of descriptions, transcriptions, and interpretations by a variety of indigenous "authors." How should these authorial presences be made manifest?

A useful – if extreme – standpoint is provided by Bakhtin's analysis of the "polyphonic" novel. A fundamental condition of the genre, he argues, is that it represents speaking subjects in a field of multiple discourses. The novel grapples with, and enacts, heteroglossia. For Bakhtin, preoccupied with the representation of nonhomogeneous wholes, there are no integrated cultural worlds or languages. All attempts to posit such abstract unities are constructs of monological power. A "culture" is, concretely, an open-ended, creative dialogue of subcultures, of insiders and outsiders, of diverse factions. A "language" is the interplay and struggle of regional dialects, professional jargons, generic commonplaces, the speech of different age groups, individuals, and so forth. For Bakhtin the polyphonic novel is not a tour de force of cultural or historical totalization (as realist critics such as Georg Lukács and Erich Auerbach have argued) but rather a carnivalesque arena of diversity. Bakhtin discovers a utopian textual space where discursive complexity, the dialogical interplay of voices, can be accommodated. In the novels of Dostoyevsky or Dickens he values precisely their resistance to totality, and his ideal novelist is a ventriloquist – in nineteenth-century parlance a "polyphonist." "He do the police in different voices," a listener exclaims admiringly of the boy Sloppy, who reads publicly from the newspaper in *Our Mutual Friend*. But Dickens the actor, oral performer, and polyphonist must be set against Flaubert, the master of authorial control, moving godlike among the thoughts and feelings of his characters. Ethnography, like the novel, wrestles with these alternatives. Does the ethnographic writer portray what natives think by means of Flaubertian "free indirect style," a style that suppresses direct quotation in favor of a controlling discourse always more or less that of the author? (Dan Sperber 1981, taking Evans-Pritchard as his example, has convincingly shown that *style indirect* is indeed the preferred mode of ethnographic interpretation.) Or does the portrayal of other subjectivities require a version that is stylistically less homogeneous, filled with Dickens' "different voices"?

Some use of indirect style is inevitable, unless the novel or ethnography is composed entirely of quotations, something that is theoretically possible but seldom attempted.[11] In practice, however, the ethnography and the novel have recourse to indirect style at different levels of abstraction. We need not ask how Flaubert knows what Emma Bovary is thinking, but the ability of the fieldworker to inhabit indigenous minds is always in doubt. Indeed this is a permanent, unresolved problem of ethnographic method. Ethnographers have generally refrained from ascribing beliefs, feelings, and thoughts to individuals. They have not, however, hesitated to ascribe subjective states to cultures. Sperber's analysis reveals how phrases such as "the Nuer think . . ." or "the Nuer sense of time" are fundamentally different from quotations or translations of indigenous discourse. Such statements are "without any specified speaker" and are literally equivocal, combining in a seamless way the ethnographer's affirmations with that of an informant or informants (1981:78). Ethnographies abound in unattributed sentences such as "The spirits return to the village at night," descriptions of beliefs in which the writer assumes in effect the voice of culture.

At this "cultural" level ethnographers aspire to a Flaubertian omniscience that moves freely throughout a world of indigenous subjects. Beneath the surface, though, their texts are more unruly and discordant. Victor Turner's work provides a telling case in point, worth investigating more closely as an example of the interplay of monophonic and polyphonic exposition. Turner's ethnographies offer superbly complex portrayals of Ndembu ritual symbols and beliefs; and he has provided too an unusually explicit glimpse behind the scenes. In the midst of the essays collected in *The Forest of Symbols*, his third book on the Ndembu, Turner offers a portrait of his best informant, "Muchona the Hornet, Interpreter of Religion" (1967:131–50). Muchona, a ritual healer, and Turner are drawn together by their shared interest in traditional symbols, etymologies, and esoteric meanings. They are both "intellectuals," passionate interpreters of the nuances and depths of custom; both are uprooted scholars sharing "the quenchless thirst for objective knowledge." Turner compares Muchona to a university don; his account of their collaboration includes more than passing hints of a strong psychological doubling.

There is, however, a third present in their dialogue, Windson Kashinakaji, a Ndembu senior teacher at the local mission school. He brought Muchona and Turner together and shares their passion for the interpretation of customary religion. Through his biblical education he "acquired a flair for elucidating knotty questions." Newly skeptical of Christian dogma and missionary privileges, he is looking sympathetically at pagan religion. Kashinakaji, Turner tells us, "spanned the cultural distance between Muchona and myself, transforming the little doctor's technical jargon and salty village argot into a prose I could better grasp." The three intellectuals soon "settled down into a sort of daily seminar on religion." Turner's accounts of this seminar are stylized: "eight months of exhilarating quickfire talk among the three of us, mainly about Ndembu ritual." They reveal an extraordinary ethnographic "colloquy"; but significantly Turner does not make his three-way collaboration the crux of his essay. Rather he focuses on Muchona, thus transforming trialogue into dialogue and flattening a complex productive relation into the "portrait" of an "informant." (This reduction was in some degree required by the format of the book in which the essay first appeared, Joseph Casagrande's important 1960 collection of "Twenty Portraits of Anthropological Informants," *In the Company of Man.*)[12]

[...]

The inclusion of Turner's portrait of Muchona in *The Forest of Symbols* may be seen as a sign of the times. The Casagrande collection in which it originally appeared had the effect of segregating the crucial issue of relations between ethnographers and their indigenous collaborators. Discussion of these issues still had no place within scientific ethnographies, but Casagrande's collection shook the post-Malinowski professional taboo on "privileged informants." Raymond Firth on Pa Fenuatara, Robert Lowie on Jim Carpenter – a long list of distinguished anthropologists have described the indigenous "ethnographers" with whom they shared, to some degree, a distanced, analytic, even ironic view of custom. These individuals became valued informants because they understood, often with real subtlety, what an *ethnographic* attitude toward culture entailed. In Lowie's quotation of his Crow interpreter (and fellow "philologist") Jim Carpenter, one senses a shared outlook: "When you listen to the old men telling about their visions, you've just *got* to believe them" (Casagrande 1960:428). And there is considerably more than a wink and a nod in the story recounted by Firth about his best Tikopian friend and informant:

> On another occasion talk turned to the nets set for salmon trout in the lake. The nets were becoming black, possibly with some organic growth, and tended to rot easily. Pa Fenuatara then told a story to the crowd assembled in the house about how, out on the lake with his nets one time, he felt a spirit going among the net and making it soft. When he held the net up he found it slimy. The spirit had been at work. I asked him then if this was a traditional piece of knowledge that spirits were responsible for the deterioration of the nets. He answered, "No, my own thought." Then he added with a laugh, "My own piece of traditional knowledge." (Casagrande 1960: 17–18)

The full methodological impact of Casagrande's collection remains latent, especially the significance of its accounts for the dialogical production of ethnographic texts and interpretations. This significance is obscured by a tendency to cast the book as a universalizing, humanist document revealing "a hall of mirrors ... in full variety the endless reflected image of man" (Casagrande 1960: xii). In light of the present crisis in ethnographic authority, however, these revealing portraits spill into the oeuvres of their authors, altering the way they can be read. If ethnography is part of what Roy Wagner (1980) calls "the invention of culture," its activity is plural and beyond the control of any individual.

One increasingly common way to manifest the collaborative production of ethnographic knowledge is to quote regularly and at length from informants. (A striking example is *We Eat the Mines, the Mines Eat Us* [1979] by June Nash.) But such a tactic only begins to break up monophonic authority. Quotations are always staged by the quoter and tend to serve merely as examples or confirming testimonies. Looking beyond quotation, one might imagine a more radical polyphony that would "do the natives and the ethnographer in different voices"; but this too would only displace ethnographic authority, still confirming the final virtuoso orchestration by a single author of all the discourses in his or her text. In this sense Bakhtin's polyphony, too narrowly identified with the novel, is a domesticated heteroglossia. Ethnographic discourses are not, in any event, the speeches of invented characters. Informants are specific individuals with real proper names – names that can be cited, in altered form when tact requires. Informants' intentions are overdetermined, their words politically and metaphorically complex. If accorded an autonomous textual space, transcribed at sufficient length, indigenous statements make sense in terms different from those of the arranging ethnographer. Ethnography is invaded by heteroglossia.

This possibility suggests an alternate textual strategy, a utopia of plural authorship that accords to collaborators not merely the status of independent enunciators but that of writers. As a form of authority it must still be considered utopian for two reasons. First, the few recent experiments with multiple-author works appear to require, as an instigating force, the research interest of an ethnographer who in the end assumes an executive, editorial position. The authoritative stance of "giving voice" to the other is not fully transcended. Second, the very idea of plural authorship challenges a deep Western identification of any text's order with the intention of a single author. If this identification was less strong when Lafitau wrote his *Moeurs des sauvages ameriquains*, and if recent criticism has thrown it into question, it is still a potent constraint on ethnographic writing. Nonetheless, there are signs of movement in this domain. Anthropologists will increasingly have to share their texts, and sometimes their title pages, with those indigenous collaborators for whom the term *informants* is no longer adequate, if it ever was.

Ralph Bulmer and Ian Majnep's *Birds of My Kalam Country* (1977) is an important prototype. (Separate typefaces distinguish the juxtaposed contributions of ethnographer and New Guinean, collaborators for more than a decade.) Even more significant is the collectively produced 1974 study *Piman Shamanism and Staying Sickness (Ka:cim Mumkidag)*, which lists on its title page, without distinction (though not, it may be noted, in alphabetical order): Donald M. Bahr, anthropologist; Juan Gregorio, shaman; David I. Lopez, interpreter; and Albert Alvarez, editor. Three of the four are Papago Indians, and the book is consciously designed "to transfer *to a shaman* as many as possible of the functions normally associated with authorship. These include the selection of an expository style, the duty to make interpretations and explanations, and the right to judge which things are important and which are not" (p. 7). Bahr, the initiator and organizer of the project, opts to share authority as much as possible. Gregorio, the shaman, appears as the principal source of the "theory of disease" that is transcribed and translated, at two separate levels, by Lopez and Alvarez. Gregorio's vernacular texts include compressed, often gnomic explanations, which are themselves interpreted and contextualized by Bahr's separate commentary. The book is unusual in its textual enactment of the interpretation of interpretations.

In *Piman Shamanism* the transition from individual enunciations to cultural generalizations is always visible in the separation of Gregorio's and Bahr's voices. The authority of Lopez, less visible, is akin to that of Windson Kashinakaji in Turner's work. His bilingual fluency guides Bahr through the subtleties of Gregorio's language, thus permitting the shaman "to speak at length on theoretical topics." Neither Lopez nor Alvarez appears as a specific voice in the text, and their contribution to the ethnography remains largely invisible to all but qualified Papagos, able to gauge the accuracy of the translated texts and the vernacular nuance of Bahr's interpretations. Alvarez' authority inheres in the fact that *Piman Shamanism* is a book directed at separate audiences. For most readers focusing on the translations and explanations the texts printed in Piman will be of little or no interest. The linguist Alvarez, however, corrected the transcriptions and translations with an eye to their use in language teaching, using an orthography he had developed for that purpose. Thus the book contributes to the Papagos' literary invention of their culture. This different reading, built into *Piman Shamanism*, is of more than local significance.

It is intrinsic to the breakup of monological authority that ethnographies no longer address a single general type of reader. The multiplication of possible readings reflects the fact that "ethnographic" consciousness can no longer be seen as the monopoly of certain Western cultures and social classes. Even in ethnographies lacking vernacular texts, indigenous readers will decode differently the textualized interpretations and lore. Polyphonic works are particularly open to readings not specifically intended. Trobriand readers may find Malinowski's interpretations tiresome but his examples and extended transcriptions still evocative. Ndembu will not gloss as quickly as European readers over the different voices embedded in Turner's works.

[. . .]

The textual embodiment of authority is a recurring problem for contemporary experiments in ethnography.[13] An older, realist mode – figured in the frontispiece to *Argonauts of the Western Pacific* and based on the construction of a cultural tableau vivant designed to be seen from a single vantage point, that of the writer and reader – can now be identified as only one possible paradigm for authority. Political and epistemological assumptions are built into this and other styles, assumptions the ethnographic writer can no longer afford to ignore. The modes of authority reviewed here – experiential, interpretive, dialogical, polyphonic – are available to all writers of ethnographic texts, Western and nonWestern. None is obsolete, none pure: there is room for invention within each paradigm. We have seen how new approaches tend to rediscover discarded practices. Polyphonic authority looks with renewed sympathy to compendiums of vernacular texts – expository forms distinct from the focused monograph tied to participant observation. Now that naive claims to the authority of experience have been subjected to hermeneutic suspicion, we may anticipate a renewed attention to the subtle interplay of personal and disciplinary components in ethnographic research.

Experiential, interpretive, dialogical, and polyphonic processes are at work, discordantly, in any ethnography, but coherent presentation presupposes a controlling mode of authority. I have argued that this imposition of coherence on an unruly textual process is now inescapably a matter of strategic choice. I have tried to distinguish important styles of authority as they have become visible in recent decades. If ethnographic writing is alive, as I believe it is, it is struggling within and against these possibilities.

# *Part X*

# Fictive Fieldwork and Fieldwork Novels

*Jeffrey A. Sluka*

> If the dual nature of anthropology — an art and a science, a humanistic science — is accepted, there is no reason why each cannot be expanded. The inherent ambiguities of this approach are only a reflection of those which exist in life itself.
>
> — Powdermaker 1966:306

Some anthropologists have been exploring literary styles to transmit their fieldwork experiences, thus blurring the boundaries between experience, impression, and emotion. Others have turned to poetry, experimental writing, and fiction to make readers share in the ethnographic encounter which makes fieldwork such a singular experience. In the final part of this anthology, we present chapters selected from three partially fictionalized accounts of fieldwork written by anthropologists, all of them great publishing successes: Elenore Smith Bowen's *Return to Laughter* (1964) is a fictionalized account of real fieldwork presented as an "anthropological novel," and Carlos Castaneda's *The Teachings of Don Juan: A Yaqui Way of Knowledge* (1968) and Florinda Donner's *Shabono: A Visit to a Remote and Magical World in the South American Rainforest* (1982) are generally regarded as fictionalized accounts of fieldwork presented as "real" ethnographies.

In the introduction to this anthology, we stressed the dual nature of ethnographic fieldwork as both an art and a science. Particularly since the 1980s, ethnography and fiction have been juxtaposed in a number of theoretical areas, such as the literary and postmodern interest in ethnography as a form of literature. Ethnographic fiction ties in with the humanist tradition in anthropology, and with alternative and experimental forms of ethnographic writing. Even though ethnographic fiction has been ignored and hidden by anthropologists who felt it was inappropriate for consideration as ethnography, there is, in fact, a long and well-established tradition of anthropologists writing fiction informed by their research and fieldwork experiences.

In "Ethnography and Fiction: Where is the Border?," Kirin Narayan (1999) presents a history of anthropologists writing fiction, and argues that the border between ethnography and fiction is "gradual and shifting" (1999:143) and allows for "mindful border crossings" that may enrich both genres (1999:134). Narayan reveals that there has been "a mostly hidden lineage of anthropologists who write both ethnography and fiction" (1999:135) that dates back to the nineteenth century, including the following ethnographic novels written by anthropologists, based on their fieldwork (1999:136):

- 1890, Adolph Bandelier's *The Delight Makers.*
- 1922, Elsie Clews Parsons' edited volume of stories *American Indian Life*, which included short stories from established anthropologists of the day including Franz Boas, Robert Lowie, Edward Sapir, and Paul Radin.
- 1929, Oliver La Farge's Pulitzer Prize-winning *Laughing Boy.*
- 1934, Zora Neald Hurston's *Jonah's Gourd Vine.*
- 1936, D'Arcy McNickle's *The Surrounded.*
- 1940s, Ella Deloria's *Waterlilly* (not published until 1988).
- 1954, Laura Bohannan's *Return to Laughter* (published in 1964, under the pseudonym Elenore Smith Bowen).
- 1960s, Carter Wilson's *Crazy February* (published in 1974).
- 1969, Carlos Castaneda's *The Teachings of Don Juan: A Yaqui Way of Knowledge.*

Narayan (1999:136) observes that as a result of the growth of experimentation in forms of ethnographic writing since the 1970s, a number of anthropologists have now openly turned to fiction, including Michael Jackson (1986), Timothy Knabb (1995), Richard and Sally Price (1994), John Stewart (1989), and Margery Wolf (1992).

The practice of anthropologists writing fiction is not only alive and well but appears to be thriving. It is particularly supported by the Society for Humanist Anthropology, and seen in articles published in their journal *Anthropology and Humanism* and the annual fiction awards they present. An illustrative example is Colleen Springwater's "The Stories of Red Clay: Man of the Mesa" (1995), in which she artfully tells four stories as an alternative model for the presentation of informant accounts. Four "true" stories told to her by a Native American informant reemerge as "fiction" in the ethnographer's retelling, in a style Springwater terms "anthropoetry" (1995:193). That this interest in exploring the boundaries between ethnography and fiction is growing was also demonstrated in the April, 2005 issue of the *Anthropology Newsletter*, which published a list of texts by anthropologists who have "ventured beyond the conventional scholarly journal article or monograph" (Gottlieb 2005:27), including fictionalized ethnographies and ethnographically grounded novels, plays, poetry, comic strips, graphic novels, biographies, memoirs, childrearing manuals, and even murder mysteries.

For those anthropologists who prefer to think of fieldwork strictly as a formal scientific method of investigation, developments along these lines are frivolous and possibly damaging to the academic authority of the discipline. In the 1960s, such skepticism compelled Laura Bohannan to use the *nom de plume* Elenore Smith Bowen in place of her own name, as the author of her highly successful "anthropological novel" *Return to Laughter* (1964). But fieldwork is far more than just a method; the very idea or image of "fieldwork" has entered the popular imagination, and shows up outside of academia in many forms of popular culture and as a trope in many forms of popular media. While the "romance" of fieldwork is more a popular than scientific perception, this trope has been increasingly exploited by writers, filmmakers, and other artists.

Today, writers with no background in anthropology write books, plays, short stories, and films with anthropologists in them, frequently focusing on fieldwork experiences. For example, while Susanna Kaysen is not a trained anthropologist, in her novel *Far Afield* (1990) she nonetheless seems to achieve what Bohannan, Castaneda, and Donner are all acclaimed for – conveying both ethnographic insights about the people and culture involved and what fieldwork is "really" like, through a fictionalized account. But in this case, it is entirely fiction, and not based on professional anthropological training or fieldwork experience. *Far Afield* is described as an "anthropological novel," set on the remote Faroe Islands in the North Atlantic, where an anthropology graduate student has arrived to do PhD fieldwork. Kaysen is an accomplished novelist who employs a reflexive trope; she turns her powers of observation on observation itself – who is the observer and who the observed? Humorous and poignant, the novel evokes the complexities of the ethnographic encounter, and applies many of the key tropes of narrative reflexive ethnography – culture shock on first arrival, suspicion that the anthropologist might be a CIA agent, establishing and maintaining rapport, competition with other anthropologists who do research on the island, adapting to alien local customs and cuisine, an affair with a woman the locals want him to marry, the threat of "going native," and, finally, the pain and exhilaration of withdrawal from the field.

The *Anthropology Newsletter* has published lists of popular films featuring anthropologist characters, who are nearly always identified with their (usually exotic) fieldwork. For example, *Nomads* (1986) – one of my favorites – a "psychic thriller" about a cultural anthropologist haunted by "evil Eskimo spirits," directed by John McTiernan, which provided the blueprint for his later Hollywood blockbusters *Predator*, *Die Hard*, and *The Last Action Hero*. Leading cartoonists, such as Gary Larson, and Garry Trudeau, whose Doonesbury cartoon, unfortunately associating anthropological fieldwork with counterinsurgency, appears in the introduction to Part VI, have also presented images of anthropologists in their work.

In 1964, Laura Bohannan, renowned in the discipline for her work with the Tiv of Northern Nigeria, pseudonymously published *Return to Laughter: An Anthropological Novel*. Based on her real experiences, the book presented a fictionalized account of fieldwork with an African tribe, which was a pastiche composed of the stories of several people and numerous field trips. Because at that time fieldwork experiences were not yet an acceptable topic for anthropologists to write about, Bohannan felt it necessary to publish the book under a pseudonym in order to detach the mistakes, problems, and embarrassments recounted in the "novel" from the scientific monograph based on her fieldwork in Nigeria. Despite the literary license used, the book succeeded in both providing ethnographic insights into the indigenous culture of West Africa, and conveying what fieldwork may be like better than many of the later reflexive, nonfictionalized accounts.

In *The Teachings of Don Juan*, the first of five don Juan books he wrote, Carlos Castaneda describes his experience of becoming a "sorcerer's apprentice" to a Yaqui shaman while an anthropology postgraduate student at UCLA. Before he died in 1998, Castaneda wrote ten books in all; he had millions of followers, and his books continue to sell in 17 languages. In his foreword to the book, Walter Goldschmidt observes:

> This book is both ethnography and allegory. Carlos Castaneda, under the tutelage of don Juan, takes us . . . into a world not merely other than our own, but of an entirely different order of reality . . . The central importance of entering into worlds other than our own – and hence of anthropology itself – lies in the fact that the experience leads us to understand that our own world is also a cultural construct. By experiencing other worlds, then, we see our own for what it is . . . Hence the allegory, as well as the ethnography . . . In this work he demonstrates the essential skill of good ethnography – the capacity to enter into an alien world. (1968:vii–viii).

While Castaneda always maintained that all his experiences were real rather than allegorical, his critics concluded that his don Juan books were a hoax, and that don Juan himself probably never existed. The definitive account of the controversy surrounding the veracity of Castaneda's books is *The Don Juan Papers: Further Castaneda Controversies*, edited by Richard de Mille. Published in 1980, it includes 49 articles looking at the authenticity and contribution of Castaneda's books, and concludes that, in the tradition of the Piltdown forgery, Castaneda was one of the great intellectual hoaxers of all time. It includes an "alleglossary" with 200 passages, commonly found in libraries, which furnish certain or likely origins of don Juan's teachings and Carlos' adventures, but also thoroughly examines the positive contributions these books nonetheless have made. For example, de Mille looks at how Castaneda functioned as a "trickster-teacher," a deceptive truth-bringer who brought a new concept of "reality" to millions of readers.

Florinda Donner's *Shabono* (1982) is presented as a personal account of the experiences of an anthropology graduate student doing fieldwork with the Iticoteri, a kinship group of the Yanomamo, in Venezuela. The bioblurb at the front identifies Donner as "an anthropologist currently working on her doctoral dissertation based on fieldwork on non-Western healing practices in Venezuela. She has done extensive work among shamans in Sonora and Oaxaca, Mexico as well as in Venezuela." Controversy about the book broke out in 1983, when the *American Anthropologist* published an article accusing Donner of plagiarism and fraud. Rebecca De Holmes wrote: "I find it hard to believe that Donner spent any length of time with the Yanomamo," and suggested that Donner's ethnographic data were "rather expertly borrowed from other sources and assembled in a kind of mélange of fact and fantasy for which Castaneda is so famous" (1983:665). De Holmes suggested that much of Donner's "borrowing" was outright plagiarism from another book, a captivity narrative titled *Yanoáma: The Narrative of a White Girl Kidnapped by Amazonian Indians*, written by Helena Valero (1970). De Holmes supported her accusation by presenting a series of sample passages from the two books, and a list of identical time sequences. However, in the same issue of *American Anthropologist*, Debra Pichi (1983) concluded that the book is in fact an authentic ethnography of the Yanomamo, based on 12 months of fieldwork, but reconstructed from memory and secondary sources because Donner destroyed her notes while in the field.

In the debate which followed, questions arose as to whether the whole book was a fabrication and whether Donner ever lived with the Yanomamo at all, and it centered on the issue of whether the book was "true" or "false." One consequence of this debate was a heightened recognition that it is possible, given good secondary material, to construct a convincing and ethnographically accurate account of life in another culture without having actually conducted fieldwork there. Of course, this had already been demonstrated by Ruth Benedict's highly successful ethnography of the society and culture of Japan, *The Chrysanthemum and the Sword* (1946); Benedict had no direct experience of Japan, and relied solely on secondhand sources.

Mary Louise Pratt (1986) has discussed the controversy generated by Castaneda's and Donner's books, asking if they are really ethnographies. While both claim to be "real" ethnographies by "real" anthropologists, they have been revealed to essentially be ethnographically informed fiction or literature. With regard to *Shabono*, Pratt concluded:

> What Donner did was write an infuriatingly ambiguous book, which may or may not be "true," is and is not ethnography, is and is not autobiography, does and does not claim professional and academic authority, is and is not based on fieldwork, and so on. An ungrateful apprentice can do no worse than this. For if Florinda Donner did fabricate much of her story (as she may have), she has disgraced the profession by lying, and lying so well no one could tell. If she did not fabricate her story, she scored one of the anthropological scoops of the century. (1986:31)

Regardless of the question of veracity, Pratt argues that anthropologists have much to gain from writing outside the discursive traditions that preceded them, because this has the potential to change or enrich the repertoire of ethnographic writing. She argues that once we recognize the writing conventions and tropes we use "it becomes possible, if one wishes, to liberate oneself from them, not by doing away with tropes (which is not possible) but by appropriating and inventing new ones (which is)" (1986:49–50).

Like Kenneth Good, discussed in the introduction to this reader, Donner was rumored to have "gone native." She married an Iticoteri chief, and the cover blurb describes a classic "gone-native" scenario:

> Normal practice for anthropologists is to remain as detached as possible from the societies in which they do their fieldwork. Objectivity is held to be at risk where an anthropologist becomes personally involved with the people under observation. When Florinda Donner went deep into the jungle between Venezuela and Brazil, she took her notebooks with her, and her camera. But soon these were discarded, along with her Western clothes. For Ms Donner was learning to live as an Indian: adopted by a native couple in the village, or *shabono*, of the Iticoteri, she began to go beyond observation, and to absorb their totems and taboos as her own.

It is perhaps ironic that the back cover blurb also presents supportive quotes from Carlos Castaneda, who describes it as a "masterpiece" and "superb social science," and a quote from *Newsweek* stating that the book "Has the vividness of good fiction."

Narayan has argued that crossings between ethnography and fiction – such as those presented in this part of the anthology – have been "mutually enriching" for both genres (1999:143). There is debate about whether this "creative nonfiction" strays too far from fact into the realm of fiction, and about the ethics of injecting fictional inventions within works labeled "ethnography," "memoir," or "nonfiction." But on the positive side, "accessibly written ethnographies and novels drawing on ethnographically honed insights have the potential to bring the practices of our discipline to wider audiences. In a globalized world of many crosscutting, transcultural conversations, to reach audiences beyond our disciplinary purview is to enhance anthropology's relevance" (1999:144).

While there appears to be great promise in the development of "alternative" forms of ethnography that blend fact and fiction, it seems to us that honesty is the central issue. Anthropologists have a responsibility to make it clear what their work is based on, how it has been constructed, what the sources of data or knowledge are, and whether it is partly fictionalized or not.

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# Return to Laughter

## *Elenore Smith Bowen*

### Chapter Two

Early the next morning, I sat contentedly on my veranda drinking coffee made just to my taste. My emphatic orders – "Fruit and coffee, and then leave me alone" – had been obeyed. I don't like being rushed into a day before I am ready for it. I need to stretch my senses awake, slowly, without disturbance, just as I need to stretch my limbs into life before I get out of bed. Certainly, it's not until my third cup of coffee that I am fully awake and willing to face the consequences of that condition.

I was still drinking my second cup when Sunday suddenly materialized at my elbow. Surprised, I looked up and met at eye level a bowl of seething gray ooze. At my irritated frown, Sunday vanished as rapidly and quietly as he had come; he knew some Europeans dislike porridge. Before I could rouse my sleeping wits, he was back with a large plate of eggs and fried toast.

It was well for justice that I could expostulate only through the cook's interpretation. While he crossed the yard and Sunday stood stiffly in the doorway, I reminded myself into patience. The English like a large breakfast. Indeed, in a country like theirs, where people apply the principles of insulation and central heating to themselves rather than to their houses, a large and greasy breakfast has a certain functional value which disappears in the tropics. The Englishman abroad maintains his traditions tenaciously; he drills them into his servants. I had asked for trained servants; I had, perforce, British-trained servants. I could scarcely communicate with my boys; they had misunderstood. I must explain.

Once more I tried to tell the cook that I wanted nothing but fruit and coffee in the morning. His air of reproachful resignation showed me that he thought his skill as cook was involved. I wanted to tell him it was not so; the chef who could tempt me to breakfast does not exist. But explanation without words is impossible. "Fruit, coffee good; eggs, porridge bad." We could say no more to each other.

A stir on the path released our deadlock. Once again Kako paced slowly into the rest-house yard, followed by his bright train of notables. He had come, as he had told Sackerton he would, to teach me his language. I sent the boys out to welcome him, while I searched for a notebook and gulped my coffee like a harassed commuter.

Once again we sat outside in the shade of the tree, smiling and shaking fists in greeting. Once again Kako and his notables produced pipes; the same little boy ran into the kitchen for coals; the same two women stood behind Kako. Today, however each action was a lesson. By the time their pipes were going, I had written and repeated pipe, pipes, coals,

Elenore Smith Bowen (pseudonym for Laura Bohannan), Chapter Two, pp. 13–28 from Elenore Smith Bowen, *Return to Laughter* (New York: Doubleday Anchor, 1964). Copyright © 1954 by Laura Bohannan.

flint and steel. At Kako's prompting they showed me bags and spears, pointed to chairs, sheepskins and articles of dress, drilling me on each word by the simple technique of saying it a bit louder with each repetition. They opened bags: out came snuff stands, kola nuts, odd bits of cloth wrapped around shillings and pennies. With them, we came to an end.

Kako sent the younger woman and the little boy running. Through the cook he told me that now we were to start on the real pith of the lesson. I turned to a fresh page in my notebook.

Kako pointed to himself and pronounced his name. I beamed encouragingly upon him, for above all I wanted to learn people by name – as many and as quickly as possible. When Kako named, I repeated and wrote. When he pointed, I looked intently for identifying signs: that man was very thin; that one was lame; that one was reddish; that one almost purple-black. All of them were elaborately scarred, and the more distinctive patterns of scarification proved the most reliable means of identification. Beards can be removed. What's worse, when these people shave, beard, mustache, hair and eyebrows all go, transforming a white-haired elder into a youthful billiard ball. However, this time – with all of them sitting together and in the same place – I was able to close my notebook and repeat all their names correctly. Kako informed me that I had done well.

The woman and the little boy returned, each with armfuls of leaves. Kako spread about a dozen out on the ground before me, and named them one by one; then the next dozen, and on and on. Some, he told me, were edible. By pointing at the farms to the north of the resthouse and the bush to the south, he informed me which were cultivated. Kako broke off; he needed all his attention for lighting his pipe with the matches I had given him. My instruction was taken over by Ikpoom, whose name and face I could easily remember because his eyes were so sad and he was so very ugly. Ikpoom, also pointing, taught me the words for path and bush, farm and fallow, earth and heavens, correcting my constant mistakes far more patiently than Kako had done. And I made many. Theirs is one of the simpler African languages, yet it was months before it seemed the only natural way to speak: a flow of fat, firm consonants and comfortable vowels, quite unlike the breathy hisses of English.

By nine o'clock that morning, I had several pages of words, and my tongue was limp from unaccustomed twisting. Unable to take in any more, I instituted a review by again naming the notables. I again got most of them right: the right man and almost the right sound. Kako looked on me with favor. Encouraged, I demanded the names of the women. They smiled, but Kako ignored my question and turned firmly back to the leaves. Rather reluctantly I began to name them. With every word Kako became more dour. I spoke more loudly; my pronunciation couldn't be that bad. Ikpoom's eyes grew sadder; the women seemed incredulous. The little boy could bear it no longer. He snatched from me the leaf I was naming and handed me another. The order had been mixed, and not once had I put the right name to the right plant.

These people are farmers: to them plants are as important and familiar as people. I'd never been on a farm and am not even sure which are begonias, dahlias or petunias. Plants, like algebra, have a habit of looking alike and being different, or looking different and being alike; consequently mathematics and botany confused me. For the first time in my life I found myself in a community where ten-year-old children weren't my mathematical superiors. I also found myself in a place where every plant, wild or cultivated, had a name and a use, and where every man, woman and child knew literally hundreds of plants. None of them could ever believe that I could not if I only would.

Kako gave me that long and incredulous glare with which a brilliant father regards his backward child. Then he insisted that we start all over again. I stared at the leaves. I fingered the leaves, and drew the leaves. But the only leaf I could identify almost every time was the very distinctively pronged cassava leaf. I confused corn with guinea corn and at least three, very similar wild grasses. I couldn't and still can't, tell one kind of yam leaf from the next. The little boy, no more than eight years old, stood beside me and prompted me; he knew them all. I was discouraged. Kako lost heart.

He became politely bored, and promised we should try again some other, unspecified time. Then he drew his toga more closely about him and withdrew.

I was left with the leaves and my notebook and a strong regret that I hadn't chosen to study a people like the Bedouin who have camels and a desert, both readily identified. I went back to the veranda; after one brief glance at my notebook, I stacked the leaves neatly in a corner. I was tired of leaves; I hadn't come here to learn botany. I moved outside again and occupied myself by staring at the stream of people that had been filing along the path behind the saplings since early morning.

They were all going in the same direction, scores of them, almost on each other's heels. Most of them turned their heads to stare at me, and some shouted greetings, but none of them paused. Men passed, dragging recalcitrant goats, sheep and sometimes pigs by a rope around the animal's neck. Women in gaudy waist cloths balanced on their heads calabashes filled with greens and gourds, cotton, peppers or bundles tied in leaves. They were followed by their children, also with something on their heads: smaller calabashes with smaller bundles, or sometimes a tiny stool. A solitary boy, knock-kneed and naked, carried a tiny bamboo cage with a disconsolate song bird inside it. A young dandy clad in a very small cloth, but smeared bright red with camwood, headed a group of musicians – their drums slung over their shoulders, one of them piping a bamboo flute.

"A market." Excitedly I thought of going myself. Then I felt my blisters and decided against it. The boys, however, took turns at visiting the market; from them I learned it was held every fifth day and was only a few hundred yards down the path. By eleven the procession toward the market trickled to an end. From the east I could hear the dull roar of voices.

A man, who had been sitting all day by the path leading into the resthouse yard, looked up from his knitting. He nodded in the direction of the noise and said something. Word list and notebook in hand, I sat down beside him. "The market is roaring," he knitted on; it was a red and black scarf, I noticed, and he was using bicycle spokes as knitting needles. Like everything else that can be done while attending court sessions and other important meetings, knitting is man's work. Woman's proper occupation is weeding and cooking.

"Everyone comes to Kako's market," the man continued. I began to wonder what he wanted here and of me, and why he should prefer to sit here. He could not mean to go on to market later: people were already beginning to return, though still only one and two at a time.

After lunch, when I went outside again, the man was still sitting there. A rather burly young woman, with a clown's mask of white bath powder on her face, started to come past him, beyond the pale of saplings and into my yard. He shouted. She ignored him. He dashed toward her, his arm raised as though to strike. I jumped up, shouting and frowning at the man, greeting and smiling at the woman. She turned toward me, volubly. I could only greet and smile. She shrugged, then stood stock still and stared at me.

Another woman saw us; she too started in. Again the man tried to drive her out. I objected through the cook, but the cook refused to translate. He managed to tell me that Kako had placed this man here to keep the people coming to and from the market out of the resthouse grounds.

I was furious: so Kako meant to keep me from meeting people. Hastily and angrily I announced that anyone who wished to see me should be free to do so; I had come here to meet people and to get to know them; therefore I was going to talk to everyone who wished to greet me. My boys protested: no European did such a thing. They were quite right; mentally – and conditionally – I absolved Kako of unwarranted interference. Aloud I firmly overrode my boys: I was not like other Europeans; I had come here with the sole purpose of meeting people and talking to them. They refused to move from the entrance. I countered by dragging my chair out to the path. There I sat down.

By this time more people were leaving the market. One after the other they stopped, greeted me, and drew to the side of the path to join the cluster of people that just stood and stared at me. The cluster grew into a crowd. I found myself sitting like a smiling waxwork figure on silent exhibition to the curious. I sat on, determined to make clear by my actions what I could not explain in words. Surely coming out here to greet people would prove that I welcomed them all.

I grew bored, and I was very glad to catch sight of an almost familiar face. It was one of the notables, Poorgbilin, the fat friendly one. I hailed him with loud cordiality, and escorted him and his wife into the resthouse yard. The crowd streamed in after us. I had been too successfully emphatic with the knitting man. He sat back, aloof and sullen.

Poorgbilin and I sat down in the shade of the tree. The crowd formed a tight palisade around us. Poorgbilin ignored them. I could not: they were so many and so close, yet so inaccessible beyond the barrier of language. I hadn't bargained for so many people. Poorgbilin had his wife take her market purchases out of her calabash. He named each object as she laid it on the ground: indigo, cotton, okra, palm oil. The crowd watched and listened, quietly, intently. Beniseed, beans, camwood – a word with one simple vowel safe between unspeakable consonants. I rushed at the word, and stuttered to an ignominious stop. Irrepressible laughter swept about me. I smiled, half embarrassed, at the grinning mob; they laughed again. Poorgbilin shook his head; his wife picked up her purchases. The crowd opened for them, and they left.

I also started to leave the yard, but no path opened for me. The circle of people merely extended itself, moving amoebalike as I moved, still I stood, still among them, just outside my door. I motioned them to one side. Many of them smiled; no one budged.

An old woman stepped forward, holding out a bottle filled with a thick orange liquid. Automatically I smiled and spoke: "Palm oil." A pleased murmur rippled about me. The crowd had found an amusement. They too would teach me their language. A youth waved some corn at me, calling out the word. I was silent. He spoke more loudly, shaking a corncob at me like a scolding finger. I looked at him, undecided: all my life I have had to avoid crowds for fear of fainting; I didn't want to offend anyone – they were so many . . . I looked again at their expectant faces, and spoke: "Corncob." Two raised voices corrected me. Again I tried. Several more took up the refrain, chorusing "corncob" more loudly and yet more loudly each time, until the whole throng was screaming "corncob." Someone giggled at the noise; the shouting became a gale of laughter. A boy held out a banana. A woman poked a spindle at me. Again I responded. Again I became the target of their screaming.

More and more people. I could no longer see through the press of sweating bodies. A gang of tipsy youths elbowed forward. They pulled things out of the women's calabashes to thrust at me, yelling until my ears ached from their noise. I couldn't hear. I couldn't think. I could no longer distinguish anything individual in the laughing mass of mobile faces. There was no unpeopled air to breathe.

In all earnest now, I pushed toward my door. Those just before my hands tried to draw back; they could clear no more than a narrow lane. I slipped through, and collapsed in the corner of my veranda. They would not follow: I had learned in the station that a European's house is sanctuary.

They made no attempt to follow, though the pressure behind made those in front bulge slightly into the doorway. The rest flowed, like deflected water, along the edge of my veranda – crouching outside to peer through the slit between thatch and veranda wall, inserting head and elbows to see the better. Women held their children high and at arm's length over the wall: "Look at the European!" The European is their bogy man. The children howled in their mothers' stiff clutch. A drunken young man snatched a child: "I'll give you to the European!" The child writhed in desperation and fell terrified onto the veranda floor. His mother forced her way in.

The mob poured in behind her. I jumped to my feet.

They surged forward, laughing, shouting. Cheerfully, in friendliest fashion, they swarmed about me, penning me into the corner. The odor of recently consumed beer and the indescribably rotten smell of locust beans hit me like a blow. Nauseated and half faint I leaned against the wall. I called for my boys. Cassava, corn, indigo – anything with a name – thrust against my face. Words hurled at me. Again I called. No one could hear. A girl tugged at my skirts to show me her castanets. A bent hag reached out to touch my hair. The noise, the smell of hot, excited bodies closed over me.

A determined young woman shoved her way to the fore: broad-cheeked, a stubby pipe clenched between her teeth, her hair in a score of tight French braids close against the scalp. She put her hand on my arm. I moved to dash it away. She clasped my outstretched hand tightly by the thumb – the sign of friendship. She shouted her name: Atakpa.

Then she turned her strong, straight back square upon me, like a shield against the mob. Arms akimbo, she harangued them, and one by one, raggedly, they dropped their shouting. She spoke on, assured, without hesitation. She gestured at me with a toss of her head. There was a great roar of laughter. Then, without more ado, she grabbed me by the wrist, talking all the while, and drew me behind her, limp and unresisting through the crowd.

Out onto the path we went, the crowd parting before us and closing in behind to follow, in single file, for the path was narrow and the grass sharp. Atakpa released me. I followed her gladly, hobbling a bit on my blisters, but delighted to breathe fresh air. Eventually we turned aside into a rather small and ramshackle homestead. The thatch on the huts was smoke blackened even to the outside, and many of the roofs were bent and gaping. A plump baby sat in a litter of leaves and corn husks, eating dirt. Atakpa, again taking my wrist, pulled me into the central reception hut, on hands and knees through the low doorway. I stood up, coughing with the smoke and feeling, rather than seeing, the first of the crowd push in behind me.

Beyond me, invisible in the smoke and dark, an old man's voice exploded angrily: "Get out! Get out!" As I turned to go, Atakpa pulled me down onto a very low bench – actually a plank bed cut from a single tree trunk. Here, below the smoke, my eyes began to clear. While the old man's furious voice raged on, I made out the fire, a high platform over it supported by four teakwood posts, and close to the fire, the author of all the noise: a hale old man in dark red camwood and a ragged toga fallen from his shoulders. He rose, clutching the cloth to him with both hands, still snarling loudly. The crowd which had followed us disappeared – out of the reception hut we sat in, out of the homestead.

I sat on where Atakpa had placed me, dazed, without curiosity. I realized only that it was quiet. Atakpa vanished. The old man, Yabo, again lay back in his reclining chair and stared into the fire. I made out one or two old women in the shadows behind him. Atakpa returned with a young man. She introduced him. She pointed to him, to herself and then to the old man, repeating words. I smiled at them all, vaguely and without real interest. Atakpa began to scold. I shook my head. With a short, exasperated grunt, she tapped the back of my hand. I looked down and saw my notebook and pencil still in my hand, still open to the few erratic scribbles that marked the onset of the crowd.

While I still stared, Yabo stirred and growled at Atakpa. Soon I found myself eating a freshly roasted ear of corn. Then she brought what looked like a mass of wet, white clay and, in a separate dish, a slimy green substance. I smiled, rubbed my stomach, and waved my ear of corn in my effort to look well fed. Atakpa rather reluctantly removed the dishes: Yabo and the young man ate them. In their place, she brought me the dirt-eating child and once more started pointing and talking. To please her, I listlessly jotted some syllables of nonsense. But Atakpa had a genius for making her meaning clear. I suddenly realized that she was teaching me those anthropological favorites – kinship terms – and I fell to writing happily. This, I resolved, was the way I wanted to do fieldwork: in quiet, among just a few people at a time, and in their own homes. But how? I might not find everyone as hospitable as Atakpa.

The sun was low when Atakpa dragged me back to the resthouse. I went unwillingly, half afraid that the crowd might still be there. I slumped together with relief when I saw the knitting man and my boys once more standing guard against passers-by. With an I-told-you-so attitude just short of impertinence, they inquired whether they were to let people in. I replied rather stiffly that the day was over. Privately, while I nursed my blistered feet and my shattered nerves in a hot bath, I wondered how to hit the happy medium between seeing no one and being hounded by a mob. I could not expect to be welcomed in their homesteads if I refused to receive them in mine.

Sunday came at my call. Once again he started to lead me out into the yard to my deck chair and my evening selection of drinks. I, however, wanted to stay inside, where I could not be seen. I motioned him to leave the lamp. Sunday pleaded; his frantic gestures of explanation included all the heavens. I was adamant. Monday came in, dragging my tent. He laid it at my feet. Sunday's finger pointed to an unmistakable hole in the thatch. A roll of thunder from the southern mountains completed the demonstration. In sudden comprehension, bowing and waving like animated puppets, Sunday and I pointed to roof and tent and ropes: he meant to give me a second roof of canvas inside my hut, hanging the ground sheet over the veranda and the tent itself over my bed and boxes. I would be underfoot inside.

I went out to my deck chair. There was no one about. Even the knitting man had gone. Only the swelling thunder broke a silence so clear that I could hear Sunday's muttered instructions to Monday as the two of them worked to secure me against the wet.

I, however, was even more concerned with my protection against people. I had come prepared to combat shyness and suspicion, expecting to be shunned and alone. I had known I must struggle to break through the fear and respect usually shown to Europeans – a respect that leaves one behind a glass wall, visible and untouchable. Today I had learned that these people were quite as willing to touch me as to stare. It was, it seemed, easy enough to make oneself the center of a curious crowd. But still withheld from any real contact.

Wrapped in my difficulties, I ate absently, inattentively waved Sunday good night, and sat on gazing blindly into the dark beyond my circle of lamplight. The heralding thunder, the approaching storm wind that blew keen under the thatch, roused me from my absorption and sent me to bed for warmth. There I told myself that it was only the thunder that kept me awake. Only the thunder, not the memory of raucous shouting, of smothered air, of people thrusting, handling, forcing screaming children at me. I tossed myself into bad dreams.

The pleasant rustle of rain in the thatch greeted my waking. I wanted coffee, but I lay on in bed listening for voices and wondering how many people were watching outside. The rain leaking through my thatch tapped against my canvas ceiling. It must be raining very hard. I sat up. No one would be out in this weather. I peered cautiously round the edge of my mat door. The rain-swept yard was empty. I went out.

Sunday brought papaw and coffee. I ate and drank, lit a cigarette and stared absently at the rain-pocked earth outside, tiny mud craters lifted and shifted by the pelting rain. Sunday appeared at the kitchen door, holding my umbrella over a precarious structure of dishes. I eyed him with foreboding while he tenderly unwrapped the dishes and set before me a repulsive plate of steaming sardines.

"Take it away!" I yelped. The missionary's word list had some very practical phrases in it. "Take it away!" Sunday started off with the coffee. In this language the pronouns depend on the class of objects; I had indicated a liquid "it." "Return with it," I countermanded. With almost aggressive resignation, Sunday brought back the coffee. I pointed at the sardines and repeated "take"; I did not dare repeat the liquid "it." Sunday first provided the correct pronoun. Then he gave advice. I resorted to the word list and discovered that I would get very thin and something else not listed.

Again I summoned the cook. As he so righteously pointed out, I had said only: "Eggs bad, porridge bad." With a sigh, I began a painstaking condemnation of every breakfast dish I could imagine. This time he seemed to understand. Sardines in hand, he turned to go. At the very door he turned around with the light of last hope in his eyes. "Beans on toast?" he suggested. I rejected his offer vehemently. The cook departed sadly.

I picked up the word list. Clearly I could not even influence my destiny until I could talk. The crowd had listened to Atakpa. But how could I hope to imitate her, when I could not make my own cook understand about my breakfast? I sat down to my task. The rain dripped from the trees, onto the thatch, through the thatch and onto the canvas. Against its tapping I mumbled words and phrases, over and over. Yet I was not really aware of what I uttered, for my ears were attentive to the rain, dreading any sign of its lessening, dreading the sunshine that would bring people forth.

The word list was dead. I tossed it aside impatiently. To learn I needed to hear people speak; to do my job I had to know at least a few of them intimately. Whatever else had been done yesterday, it had been made clear that I was prepared to see everyone. That could be turned to advantage. To set a guard at my gates now would destroy anything I had gained. In any case, no matter whom I might choose, I would give that person the power of selecting with whom I talked and thus what I came to know. That wouldn't work. What, then? I stretched out on my bed to think.

It was hot when I awoke, and clear. The yard was full of women's chatter punctuated with laughter and the teasing, off-duty voices of my boys. I peered out. About a dozen women were sitting under my tree, knee deep in calabashes and children. I hid behind my mat, wrestling with a sensation I came to know all too well: I wanted to talk to people! I couldn't bear to meet them. Even months later, when I could converse with ease and had my friends among these people, there remained that momentary pause, that same need to compel myself into their midst. It was very like the instinctive hesitation of the most ardent swimmer just before he plunges into the sea. I liked to be among them, just as I liked to be in the sea, but neither ever became my natural element.

I forced myself out into the yard. The women clustered about me, chattering gaily, thrusting small gifts of corn and eggs upon me. I accepted and smiled and wrote down names. I recognized Atakpa and greeted her. I sat down with them on the roots of the tree, but I could not give them my full attention. My eye was on the path. Two girls passed, saw us, turned back and came in. My heart sank. Atakpa was saying something. I paid no heed, afraid of another crowd, not knowing what to do to avoid it. Atakpa was shouting and pointing to something behind me. I shrugged my incomprehension; my eyes were fixed on the path.

Something soft and wet struck at me, below and behind. I scrambled away. It was a small, shiny tree frog. Atakpa was laughing, repeating the word she had screamed at me. If it had been a scorpion . . . I tried to grin, and resolved hereafter to pay attention to what was told me. The frog leaped at Atakpa. She jumped. I too began to laugh. Soon the frog, like a mouse at a bridge party, had all the women on their feet, giggling and screeching. Monday disposed of the frog, and we settled down again.

Atakpa said something about Yabo. I couldn't understand, but I remembered how he had chased the crowd from his homestead and wished I knew the secret. Several of the women began to chorus Atakpa's words, more and more loudly. Atakpa turned on them, silenced them, and repeated her speech softly. Gradually, with the help of my cook and the word list, I understood: so much shouting was bad; many people were a bad thing; I should talk to two or three at a time. I agreed heartily. Atakpa said I should come live with her at Yabo's homestead; there I could learn the language and no one would bother me.

For a moment I toyed with the idea. Then I remembered how small, how dilapidated and dirty Yabo's homestead was. Besides my boys seemed genuinely concerned: if I were to live any place but the resthouse, it must be in the chief's homestead. I suddenly recalled that Yabo had not been among Kako's collection of notables. Atakpa was still coaxing: it was not good to live in the bush alone. She threatened me with snakes, ants and witches. The boys were in complete agreement about the dangers she listed; there were few of us; we were some distance from the closest homestead, and again the whole group chorused: "It's not good to sit alone in the bush."

It took a long time to frame my questions and to understand their answers. At Kako's homestead would I see anyone besides his own people? "All the world comes to a chief's homestead, and they would all greet you – a European." Would they come as they had on market day? They were impatient: no crowd would push like that, uninvited (the boys were being tactful), into the homestead of anyone of importance, let alone into that of a chief.

I began to hope. True, I had planned to live separately at first, to avoid identifying myself with any part of the community till I knew something of the factions within it. By now, however, I was quite willing to shelter behind the chief's toga, if he would let me do so. But where would I live in his homestead and would he want me always underfoot?

The swift parting of the women and a flurry of greetings interrupted my vacillation. Kako himself stood before me, presenting a large and active ram prancing stiff-legged at the end of a rope. As soon as I had thanked him, he confided that his heart was troubled. With paternal concern he informed me that in my present location I was exposed to snakes and ants (he omitted the witches). It was not good to sit alone in the bush. Would we move down to his homestead? He would build me a hut for myself, one for the boys, and a kitchen and reception hut right next to his own huts in his homestead. As quickly as I could, almost before I was sure that I understood him correctly, I accepted.

As we started off for his homestead to select the exact site then and there, I felt I was fortunate indeed to be among such kind, hospitable folk. Much later, when Kako and I knew each other from months of living within whispering distance, he confessed that he had been quite ready to insist on my moving down where he could keep an eye on me. He had not thought me able to take care of myself, and Sackerton held him responsible. I had, it seemed, been an embarrassment to the Native Administration. That afternoon, however, I felt only gratitude for his kindness. I walked back to the resthouse praying that Kako would build as promptly as he had promised, that I could move soon, and that until then it would rain a little every day and all day on market day. I vowed to pay better attention to words and frogs and leaves. I was not, as yet, concerned by snakes and ants and witches.

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# The Teachings of Don Juan: A Yaqui Way of Knowledge

*Carlos Castaneda*

## Introduction

In the summer of 1960, while I was an anthropology student at the University of California, Los Angeles, I made several trips to the Southwest to collect information on the medicinal plants used by the Indians of the area. The events I describe here began during one of my trips. I was waiting in a border town for a Greyhound bus, talking with a friend who had been my guide and helper in the survey. Suddenly he leaned toward me and whispered that the man, a white-haired old Indian, who was sitting in front of the window was very learned about plants, especially peyote. I asked my friend to introduce me to this man.

My friend greeted him, then went over and shook his hand. After they had talked for a while, my friend signaled me to join them, but immediately left me alone with the old man, not even bothering to introduce us. He was not in the least embarrassed. I told him my name and he said that he was called Juan and that he was at my service. He used the Spanish polite form of address. We shook hands at my initiative and then remained silent for some time. It was not a strained silence, but a quietness, natural and relaxed on both sides. Though his dark face and neck were wrinkled, showing his age, it struck me that his body was agile and muscular.

I then told him that I was interested in obtaining information about medicinal plants. Although in truth I was almost totally ignorant about peyote, I found myself pretending that I knew a great deal, and even suggesting that it might be to his advantage to talk with me. As I rattled on, he nodded slowly and looked at me, but said nothing. I avoided his eyes and we finished by standing, the two of us, in dead silence. Finally, after what seemed a very long time, don Juan got up and looked out of the window. His bus had come. He said goodbye and left the station.

I was annoyed at having talked nonsense to him, and at being seen through by those remarkable eyes. When my friend returned he tried to console me for my failure to learn anything from don Juan. He explained that the old man was often silent or noncommittal, but the disturbing effect of this first encounter was not so easily dispelled.

I made a point of finding out where don Juan lived, and later visited him several times. On each visit I tried to lead him to discuss peyote, but without success. We became, nonetheless, very good friends, and my scientific investigation was forgotten or was at least redirected into channels that were worlds apart from my original intention.

The friend who had introduced me to don Juan explained later that the old man was not

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a native of Arizona, where we met, but was a Yaqui Indian from Sonora, Mexico.

At first I saw don Juan simply as a rather peculiar man who knew a great deal about peyote and who spoke Spanish remarkably well. But the people with whom he lived believed that he had some sort of "secret knowledge," that he was a "brujo." The Spanish word *brujo* means, in English, medicine man, curer, witch, sorcerer. It connotes essentially a person who has extraordinary, and usually evil, powers.

I had known don Juan for a whole year before he took me into his confidence. One day he explained that he possessed a certain knowledge that he had learned from a teacher, a "benefactor" as he called him, who had directed him in a kind of apprenticeship. Don Juan had, in turn, chosen me to serve as his apprentice, but he warned me that I would have to make a very deep commitment and that the training was long and arduous.

In describing his teacher, don Juan used the word "diablero." Later I learned that diablero is a term used only by the Sonoran Indians. It refers to an evil person who practices black sorcery and is capable of transforming himself into an animal – bird, a dog, a coyote, or any other creature. On one of my visits to Sonora I had a peculiar experience that illustrated the Indians' feeling about diableros. I was driving at night in the company of two Indian friends when I saw an animal that seemed to be a dog crossing the highway. One of my companions said it was not a dog, but a huge coyote. I slowed down and pulled to the side of the road to get a good look at the animal. It stayed within range of the headlights a few seconds longer and then ran into the chaparral. It was unmistakably a coyote, but it was twice the ordinary size. Talking excitedly, my friends agreed that it was a very unusual animal, and one of them suggested that it might be a diablero. I decided to use an account of the experience to question the Indians of that area about their beliefs in the existence of diableros. I talked with many people, telling them the story and asking them questions. The three conversations that follow indicate what they felt.

"Do you think it was a coyote, Choy?" I asked a young man after he had heard the story.

"Who knows? A dog, no doubt. Too large for a coyote."

"Do you think it may have been a diablero?"

"That's a lot of bull. There are no such things."

"Why do you say that, Choy?"

"People imagine things. I bet if you had caught that animal you would have seen that it was a dog. Once I had some business in another town and got up before daybreak and saddled up a horse. As I was leaving I came upon a dark shadow on the road which looked like a huge animal. My horse reared, throwing me off the saddle. I was pretty scared too, but it turned out that the shadow was a woman who was walking to town."

"Do you mean, Choy, that you don't believe there are diableros?"

"Diableros! What's a diablero? Tell me what a diablero is!"

"I don't know, Choy. Manuel, who was riding with me that night, said the coyote could have been a diablero. Maybe you could tell me what a diablero is?"

"A diablero, they say, is a brujo who changes into any form he wants to adopt. But everybody knows that is pure bull. The old people here are full of stories about diableros. You won't find that among us younger people."

"What kind of animal do you think it was, doña Luz?" I asked a middle-aged woman.

"Only God knows for sure, but I think it was not a coyote. There are things that appear to be coyotes, but are not. Was the coyote running, or was it eating?"

"It was standing most of the time, but when I first saw it, I think it was eating something."

"Are you sure it was not carrying something in its mouth?"

"Perhaps it was. But tell me, would that make any difference?"

"Yes, it would. If it was carrying something in its mouth it was not a coyote."

"What was it then?"

"It was a man or a woman."

"What do you call such people, doña Luz?"

She did not answer. I questioned her for a while longer, but without success. Finally she said she did not know. I asked her if such people were called diableros, and she answered that "diablero" was one of the names given to them.

"Do you know any diableros?" I asked.

"I knew one woman," she replied. "She was killed. It happened when I was a little girl. The woman, they said, used to turn into a female dog. And one night a dog went into the house of a white man to steal cheese. The white man killed the dog with a shotgun, and at the very moment the dog died in the house of the white man the woman died in her own hut. Her kin got together and went to the white man and demanded payment. The white man paid good money for having killed her."

"How could they demand payment if it was only a dog he killed?"

"They said that the white man knew it was not a dog, because other people were with him, and they all saw the dog stood up on its legs like a man and reached for the cheese, which was on a tray hanging from the roof. The men were waiting for the thief because the white man's cheese was being stolen every night. So the man killed the thief knowing it was not a dog."

"Are there any diableros nowadays, doña Luz?"

"Such things are very secret. They say there are no more diableros, but I doubt it, because one member of a diablero's family has to learn what the diablero knows. Diableros have their own laws, and one of them is that a diablero has to teach his secret to one of his kin."

"What do you think the animal was, Genaro?" I asked a very old man.

"A dog from one of the ranches of that area. What else?"

"It could have been a diablero!"

"A diablero? You are crazy! There are no diableros."

"Do you mean that there are none today, or that there never were any?"

"At one time there were, yes. It is common knowledge. Everybody knows that. But the people were very afraid of them and had them all killed."

"Who killed them, Genaro?"

"All the people of the tribe. The last diablero I knew about was S——. He killed dozens, maybe even hundreds, of people with his sorcery. We couldn't put up with that and the people got together and took him by surprise one night and burned him alive."

"How long ago was that, Genaro?"

"In nineteen forty-two."

"Did you see it yourself?"

"No, but people still talk about it. They say that there were no ashes left, even though the stake was made of fresh wood. All that was left at the end was a huge pool of grease."

Although don Juan categorized his benefactor as a diablero, he never mentioned the place where he had acquired his knowledge, nor did he identify his teacher. In fact, don Juan disclosed very little about his personal life. All he said was that he had been born in the Southwest in 1891; that he had spent nearly all his life in Mexico; that in 1900 his family was exiled by the Mexican government to central Mexico along with thousands of other Sonoran Indians; and that he had lived in central and southern Mexico until 1940. Thus, as don Juan had traveled a great deal, his knowledge may have been the product of many influences. And although he regarded himself as an Indian from Sonora, I was not sure whether to place the context of his knowledge totally in the culture of the Sonoran Indians. But it is not my intention here to determine his precise cultural milieu.

I began to serve my apprenticeship to don Juan in June, 1961. Prior to that time I had seen him on various occasions, but always in the capacity of an anthropological observer. During these early conversations I took notes in a covert manner. Later, relying on my memory, I reconstructed the entire conversation. When I began to participate as an apprentice, however, that method of taking notes became very difficult, because our conversations touched on many different topics. Then don Juan allowed me – under strong protest, however – to record openly anything that was said. I would also have liked to take photographs and make tape recordings, but he would not permit me to do so.

I carried out the apprenticeship first in Arizona and then in Sonora, because don Juan moved to Mexico during the course of my training. The procedure I employed was to see him for a few days every so often. My visits became more frequent and lasted longer during the summer months of 1961, 1962, 1963, and 1964. In retrospect, I believe this method of conducting the apprenticeship prevented the training from being successful, because it retarded the advent of the full commitment I needed to become a sorcerer. Yet the method was beneficial from my personal standpoint in that it allowed me a modicum of detachment, and that in turn fostered a sense of critical examination which would have been impossible to attain had I participated continuously, without interruption. In September, 1965, I voluntarily discontinued the apprenticeship.

Several months after my withdrawal, I considered for the first time the idea of arranging my field notes in a systematic way. As the data I had collected were quite voluminous, and included much miscellaneous information, I began by trying to establish a classification system. I divided the data into areas of related concepts and procedures and arranged the areas hierarchically according to subjective importance – that is, in terms of the impact that each of them had had on me. In that way I arrived at the following classification: uses of hallucinogenic plants; procedures and formulas used in sorcery; acquisition and manipulation of power objects; uses of medicinal plants; songs and legends.

Reflecting upon the phenomena I had experienced, I realized that my attempt at classification had produced nothing more than an inventory of categories; any attempt to refine my scheme would therefore yield only a more complex inventory. That was not what I wanted. During the months following my withdrawal from the apprenticeship, I needed to understand what I had experienced, and what I had experienced was the teaching of a coherent system of beliefs by means of a pragmatic and experimental method. It had been evident to me from the very first session in which I had participated that don Juan's teachings possessed an internal cohesion. Once he had definitely decided to communicate his knowledge to me, he proceeded to present his explanations in orderly steps. To discover that order and to understand it proved to be a most difficult task for me.

My inability to arrive at an understanding seems to have been traceable to the fact that, after four years of apprenticeship, I was still a beginner. It was clear that don Juan's knowledge and his method of conveying it were those of his benefactor; thus my difficulties in understanding his teachings must have been analogous to those he himself had encountered. Don Juan alluded to our similarity as beginners through incidental comments about his incapacity to understand his teacher during his own apprenticeship. Such remarks led me to believe that to any beginner, Indian or non-Indian, the knowledge of sorcery was rendered incomprehensible by the outlandish characteristics of the phenomena he experienced. Personally, as a Western man, I found these characteristics so bizarre that it was virtually impossible to explain them in terms of my own everyday life, and I was forced to the conclusion that any attempt to classify my field data in my own terms would be futile.

Thus it became obvious to me that don Juan's knowledge had to be examined in terms of how he himself understood it; only in such terms could it be made evident and convincing. In trying to reconcile my own views with don Juan's, however, I realized that whenever he tried to explain his knowledge to me, he used concepts that would render it "intelligible" to him. As those concepts were alien to me, trying to understand his knowledge in the way he did placed me in another untenable position. Therefore, my first task was to determine his order of conceptualization. While working in that direction, I saw that don Juan himself had placed particular emphasis on a certain area of his teachings – specifically, the uses of hallucinogenic plants. On the basis of this realization, I revised my own scheme of categories.

Don Juan used, separately and on different occasions, three hallucinogenic plants: peyote (*Lophophora williamsii*), Jimson weed (*Datura inoxia* syn. D. *meteloides*), and a mushroom (possibly *Psilocybe mexicana*). Since before their contact with Europeans, American Indians have known the hallucinogenic properties of these three plants. Because of their properties, the plants have been widely employed for pleasure, for curing, for witchcraft, and for attaining a state of ecstasy. In the specific context of his teachings, don Juan related the use of *Datura inoxia* and *Psilocybe mexicana* to the acquisition of power, a power he called an "ally." He related the use of *Lophophora williamsii* to the acquisition of wisdom, or the knowledge of the right way to live.

The importance of the plants was, for don Juan, their capacity to produce stages of peculiar perception in a human being. Thus he guided me into experiencing a sequence of these stages for the purpose of unfolding and validating his knowledge. I have called them "states of nonordinary reality," meaning unusual reality as opposed to the ordinary reality of everyday life. The distinction is based on the inherent meaning of the states of nonordinary reality. In the context of don Juan's knowledge they were considered as real, although their reality was differentiated from ordinary reality.

Don Juan believed the states of nonordinary reality to be the only form of pragmatic learning and the only means of acquiring power. He conveyed the impression that other parts of his teachings were incidental to the acquisition of power. This point of view permeated don Juan's attitude toward everything not directly connected with the states of nonordinary reality. Throughout my field notes there are scattered references to the way don Juan felt. For example, in one conversation he suggested that some objects have a certain amount of power in themselves. Although he himself had no respect for power objects, he said they were frequently used as aids by lesser brujos. I often asked him about such objects, but he seemed totally uninterested in discussing them. When the topic was raised again on another occasion, however, he reluctantly consented to talk about them.

"There are certain objects that are permeated with power," he said. "There are scores of such objects which are fostered by powerful men with the aid of friendly spirits. These objects are tools – not ordinary tools, but tools of death. Yet they are only instruments; they have no power to teach. Properly speaking, they are in the realm of war objects designed for strife; they are made to kill, to be hurled."

"What kind of objects are they, don Juan?"

"They are not really objects; rather, they are types of power."

"How can one get those types of power, don Juan?"

"That depends on the kind of object you want."

"How many kinds are there?"

"As I have already said, there are scores of them. Anything can be a power object."

"Well, which are the most powerful, then?"

"The power of an object depends on its owner, on the kind of man he is. A power object fostered by a lesser brujo is almost a joke; on the other hand, a strong, powerful brujo gives his strength to his tools."

"Which power objects are most common, then? Which ones do most brujos prefer?"

"There are no preferences. They are all powerful objects, all just the same."

"Do you have any yourself, don Juan?"

He did not answer; he just looked at me and laughed. He remained quiet for a long time, and I thought my questions were annoying him.

"There are limitations on those types of powers," he went on. "But such a point is, I am sure, incomprehensible to you. It has taken me nearly a lifetime to understand that, by itself, an ally can reveal all the secrets of these lesser powers, rendering them rather childish. I had tools like that at one time, when I was very young."

"What power objects did you have?"

"*Maíz-pinto*, crystals, and feathers."

"What is maíz-pinto, don Juan?"

"It is a small kernel of corn which has a streak of red color in its middle."

"Is it a single kernel?"

"No. A brujo owns forty-eight kernels."

"What do the kernels do, don Juan?"

"Each one of them can kill a man by entering into his body."

"How does a kernel enter into a human body?"

"It is a power object and its power consists, among other things, in entering into the body."

"What does it do when it enters into the body?"

"It immerses itself in the body; it settles on the chest, or on the intestines. The man becomes ill, and unless the brujo who is tending him is stronger than the bewitcher, he will die within three months from the moment the kernel entered into his body."

"Is there any way of curing him?"

"The only way is to suck the kernel out, but very few brujos would dare to do that. A brujo may succeed in sucking the kernel out, but unless he is powerful enough to repel it, it will get inside him and will kill him instead."

"But how does a kernel manage to enter into someone's body?"

"To explain that I must tell you about corn witchcraft, which is one of the most powerful witchcrafts I know. The witchcraft is done by two kernels. One of them is put inside a fresh bud of a yellow flower. The flower is then set on a spot where it will come into contact with the victim: the road on which he walks every day, or any place where he is habitually present. As soon as the victim steps on the kernel, or touches it in any way, the witchcraft is done. The kernel immerses itself in the body."

"What happens to the kernel after the man has touched it?"

"All its power goes inside the man, and the kernel is free. It becomes just another kernel. It may be left at the site of the witchcraft, or it may be swept away; it does not matter. It is better to sweep it away into the underbrush, where a bird will eat it."

"Can a bird eat it before the man touches it?"

"No. No bird is that stupid, I assure you. The birds stay away from it."

Don Juan then described a very complex procedure by which such power kernels can be obtained.

"You must bear in mind that maíz-pinto is merely an instrument, not an ally," he said. "Once you make that distinction you will have no problem. But if you consider such tools to be supreme, you will be a fool."

"Are the power objects as powerful as an ally?" I asked.

Don Juan laughed scornfully before answering. It seemed that he was trying hard to be patient with me.

"Maíz-pinto, crystals, and feathers are mere toys in comparison with an ally," he said. "These power objects are necessary only when a man does not have an ally. It is a waste of time to pursue them, especially for you. You should be trying to get an ally; when you succeed, you will understand what I am telling you now. Power objects are like a game for children."

"Don't get me wrong, don Juan," I protested. "I want to have an ally, but I also want to know everything I can. You yourself have said that knowledge is power."

"No!" he said emphatically. "Power rests on the kind of knowledge one holds. What is the sense of knowing things that are useless?"

In don Juan's system of beliefs, the acquisition of an ally meant exclusively the exploitation of the states of nonordinary reality he produced in me through the use of hallucinogenic plants. He believed that by focusing on these states and omitting other aspects of the knowledge he taught I would arrive at a coherent view of the phenomena I had experienced.

I have therefore divided this book into two parts. In the first part I present selections from my field notes dealing with the states of nonordinary reality I underwent during my apprenticeship. As I have arranged my notes to fit the continuity of the narrative, they are not always in proper chronological sequence. I never wrote my description of a state of nonordinary reality until several days after I had experienced it, waiting until I was able to treat it calmly and objectively. My conversations with don Juan, however, were taken down as they occurred, immediately after each state of nonordinary reality. My reports of these conversations, therefore, sometimes antedate the full description of an experience.

My field notes disclose the subjective version of what I perceived while undergoing the experience. That version is presented here just as I narrated it to don Juan, who demanded a complete and faithful recollection of every detail and a full recounting of each experience. At the time of recording these experiences, I added incidental details in an attempt to recapture the total setting of each state of nonordinary reality. I wanted to describe the emotional impact I had experienced as completely as possible.

My field notes also reveal the content of don Juan's system of beliefs. I have condensed long papers of questions and answers between don Juan and myself in order to avoid reproducing the repetitiveness of conversation. But as I also want to reflect accurately the overall mood of our exchanges, I have deleted only dialogue that contributed nothing to my understanding of his way of knowledge. The information don Juan gave me about his way of knowledge was always sporadic, and for every spurt on his part there were hours of probing on mine. Nevertheless, there were innumerable occasions on which he freely expounded his knowledge.

In the second part of this book I present a structural analysis drawn exclusively from the data reported in the first part. Through my analysis I seek to support the following contentions: (1) don Juan presented his teachings as a system of logical thought; (2) the system made sense only if examined in the light of its structural units; and (3) the system was devised to guide an apprentice to a level of conceptualization which explained the order of the phenomena he had experienced.

# 38

# Shabono: A True Adventure in the Remote and Magical Heart of the South American Jungle

*Florinda Donner*

## 6

"When do you think you'll be back?" I asked Milagros six months later, handing him the letter I had written to Father Coriolano at the mission. In it I briefly notified him that I intended to stay for at least two more months with the Iticoteri. I asked him to inform my friends in Caracas; and most important of all, I begged him to send with Milagros as many writing pads and pencils as he could spare. "When will you be back?" I asked again.

"In two weeks or so," Milagros said casually, fitting the letter into his bamboo quiver. He must have detected the anxiousness in my face for he added, "There is no way to tell, but I'll be back."

I watched as he started down the path leading to the river. He adjusted the quiver on his back, then turned to me briefly, his movements momentarily arrested as though there were something he wished to say. Instead he lifted his hand to wave good-bye.

Slowly I headed back to the *shabono*, passing several men felling trees next to the gardens. Carefully I stepped around the logs cluttered all over the cleared patch, making sure not to cut my feet on the pieces of bark, chips, and slivers of wood buried amidst the dead leaves on the ground.

"He'll be back as soon as the plantains are ripe," Etewa shouted, waving his hand the way Milagros had just done. "He won't miss the feast."

Smiling, I waved back, wanting to ask when the feast would take place. I did not need to; he had already given me the answer: When the plantains were ripe.

The brush and logs that were scattered each night in front of the main entrance of the *shabono* to keep out intruders had already been moved aside. It was still early, yet the huts facing the round, open clearing were mostly empty. Women and men were working in the nearby gardens or had gone into the forest to gather wild fruits, honey, and firewood.

Armed with miniature bows and arrows, a group of little boys gathered around me. "See the lizard I killed," Sisiwe said, holding the dead animal by the tail.

Florinda Donner, Chapter 6, pp. 61–73 from Florinda Donner, *Shabono: A True Adventure in the Remote and Magical Heart of the South American Jungle* (London: Triad/Paladin, 1984). Copyright © Florinda Donner 1982.

"That's all he can do – shoot lizards," a boy in the group said mockingly, scratching his ankle with the toes of his other foot. "And most of the time he misses."

"I don't," Sisiwe shouted, his face turning red with rage.

I caressed the stubbles on the crown of his head. In the sunlight his hair was not black but a reddish brown. Searching for the right words from my limited vocabulary, I hoped to assure him that one day he would be the best hunter in the settlement.

Sisiwe, Ritimi's and Etewa's son, was six, at the most seven, years old for he did not yet wear a pubic waist string. Ritimi, believing that the sooner a boy tied his penis against his abdomen the faster he would grow, had repeatedly forced the child to do so. But Sisiwe had refused, arguing that it hurt. Etewa had not insisted. His son was growing healthy and strong. Soon, the father had argued, Sisiwe would realize that it was improper for a man to be seen without a waist string. Like most children, Sisiwe wore a piece of fragrant root tied around his neck, a charm against disease, and as soon as the designs on his body faded, he was painted anew with *onoto*.

Smiling, his anger forgotten, Sisiwe held on to my hand and in one swift motion climbed up on me as if I were a tree. He wrapped his legs around my waist. He swung backward and, stretching his arms toward the sky, shouted, "Look how blue it is – the color of your eyes."

From the middle of the clearing the sky seemed immense. There were no trees, lianas, or leaves to mar its splendor. The dense vegetation loomed outside the *shabono*, beyond the palisades of logs protecting the settlement. The trees appeared to bide their time, as if they knew they were only provisionally held in check.

Tugging at my arm, the children pulled me together with Sisiwe to the ground. At first I had not been able to associate them with any particular parent for they wandered in and out of the huts, eating and sleeping wherever it was convenient. I only knew where the babies belonged, for they were perennially hanging around their mother's bodies. Whether it was day or night, the infants never seemed disturbed, regardless of what activity their mothers were engaged in.

I wondered how I would do without Milagros. Each day he had spent several hours teaching me the language, customs, and beliefs of his people, which I eagerly recorded in my notepads.

Learning who was who among the Iticoteri proved to be most confusing. They never called each other by name, except when someone was to be insulted. Ritimi and Etewa were known as Mother and Father of Sisiwe and Texoma. (It was permissible to use children's names, but as soon as they reached puberty everyone refrained from it.) Matters were further complicated in that males and females from a given lineage called each other brother and sister; males and females from another lineage were referred to as brother-in-law and sister-in-law. A male who married a woman from an eligible lineage called all the women of that lineage wives, but did not have sexual contact with them.

Milagros often pointed out that it was not only I who had to adapt. The Iticoteri were just as baffled by my odd behavior; to them I was neither woman, man, or child, and as such they did not quite know what to think of me or where they could fit me in.

Old Hayama emerged from her hut. In a high-pitched voice she told the children to leave me alone. "Her stomach is still empty," she said. Putting her arm around my waist, she led me to the hearth in her hut.

Making sure not to step on or collide with any of the aluminum and enamel cooking pots (acquired through trade with other settlements), the tortoise shells, gourds, and baskets scattered on the ground, I sat across from Hayama. I extended my legs fully, in the way of the Iticoteri women, and scratching the head of her pet parrot, I waited for the food.

"Eat," she said, handing me a baked plantain on a broken calabash. Attentively the old woman watched as I chewed with my mouth open, smacking my lips repeatedly. She smiled, content that I was fully appreciating the soft sweet plantain.

Hayama had been introduced to me by Milagros as Angelica's sister. Every time I looked at her I tried to find some resemblance to the frail old woman I had lost in the forest. About five feet four, Hayama was tall for an Iticoteri woman. Not only was she physically different from Angelica, but she did not have her sister's lightness of spirit. There was a harshness to Hayama's voice and manner that often made me feel uncomfortable. And her heavy, drooping eyelids gave her face a peculiarly sinister expression.

"You stay here with me until Milagros returns," the old woman said, serving me another baked plantain.

I stuffed the hot fruit in my mouth so I would not have to answer. Milagros had introduced me to his brother-in-law Arasuwe, who was the headman of the Iticoteri, as well as to the other members of the settlement. However, it was Ritimi who, by hanging my hammock in the hut she shared with Etewa and their two children, had made it known that I belonged to her. "The white girl sleeps here," she had said to Milagros, explaining that little Texoma and Sisiwe would have their hammocks hung around Tutemi's hearth in the adjoining hut.

No one had interfered with Ritimi's scheme. Silently, a smile of gentle mockery on his face, Etewa had watched as Ritimi rushed between their hut and Tutemi's, rearranging the hammocks in the customary triangle around the fire. On a small loft built between the back poles supporting the dwelling, she placed my knapsack, amidst bark boxes, an assortment of baskets, an ax, and gourds with *onoto*, seeds, and roots.

Ritimi's self-assuredness stemmed not only from the fact that she was the headman Arasuwe's oldest daughter – by his first wife, a daughter of old Hayama, now dead – and that she was Etewa's first and favorite wife, but also because Ritimi knew that in spite of her quick temper everyone in the *shabono* respected and liked her.

"No more," I pleaded with Hayama as she took another plantain from the fire. "My belly is full." Pulling up my T-shirt, I pushed out my stomach so she could see how filled it looked.

"You need to grow fat around your bones," the old woman said, mashing up the banana with her fingers. "Your breasts are as small as a child's." Giggling, she pulled my T-shirt up further. "No man will ever want you – he'll be afraid to hurt himself on the bones."

Opening my eyes wide in mock horror, I pretended to gobble down the mush. "I'll surely get fat and beautiful eating your food," I said with my mouth full.

Still wet from her river bath, Ritimi came into the hut combing her hair with a densely thistled pod. Sitting next to me, she put her arms around my neck and planted resounding kisses on my face. I had to restrain myself from laughing. The Iticoteri's kisses tickled me. They kissed differently; each time they put their mouth against my cheek and neck they vibrated their lips while sonorously ejecting air.

"You are not moving the white girl's hammock in here," Ritimi said, looking at her grandmother. The certainty of her tone was not matched by the inquiring softness of her dark eyes.

Not wanting to be the cause of an argument, I made it clear that it did not make much difference where my hammock hung. Since there were no walls between the huts, we practically lived together. Hayama's hut stood on Tutemi's left, and on our right was Arasuwe the headman's, which he shared with his oldest wife and three of his smallest children. His other two wives and their respective offspring occupied adjacent huts.

Ritimi fixed her gaze on me, a pleading expression in her eyes. "Milagros asked me to take care of you," she said, running the thistled pod through my hair, softly, so as not to scratch my scalp.

After what seemed an interminable silence, Hayama finally said, "You can leave your hammock where it is, but you will eat here with me."

It was a good arrangement, I thought. Etewa already had four mouths to feed. Hayama, on the other hand, was taken good care of by her youngest son. Judging by the amount of animal skulls and plantains hanging from the thatched palm roof, her son was a good hunter and cultivator. Other than the baked plantains eaten in the morning, there was only one meal, in the late afternoon, when families gathered together to eat. People snacked throughout the day on whatever was available – fruit, nuts, or such delicacies as roasted ants and grubs.

Ritimi also seemed pleased with the eating arrangement. Smiling, she walked over to our hut, pulled down the basket she had given me, which was hanging above my hammock, then took out my notepad and pencil. "Now let us work," she said in a commanding tone.

In the days that followed Ritimi taught me about her people as Milagros had done for the past six months. He had set up a few hours each day for what I referred to as formal instruction.

At first I had great difficulty in learning the language. Not only did I find it to be heavily nasal, but it was extremely difficult to understand people when they talked with wads of tobacco in their mouths. I tried to devise some sort of a comparative grammar but give it up when I realized that not only did I not have the proper linguistic training, but the more I tried to be rational about learning their language, the less I could speak.

My best teachers were the children. Although they pointed things out to me and greatly enjoyed giving me words to repeat, they made no conscious effort to explain anything. With them I was able to rattle on, totally uninhibited about making mistakes. After Milagros's departure, there was still much I did not comprehend, yet I was astonished by how well I managed to communicate with others, reading correctly the inflection of their voices, the expression on their faces, and the eloquent movements of their hands and bodies.

During those hours of formal instruction, Ritimi took me to visit the women in the different huts and I was allowed to ask questions to my heart's content. Baffled by my curiosity, the women talked freely, as if they were playing a game. They patiently explained again and again whatever I did not understand.

I was grateful Milagros had set that precedent. Not only was curiosity regarded as bad manners, but it went against their will to be questioned. Yet Milagros had lavishly indulged me in what he called my eccentric whim, stating that the more I knew about the language and customs of the Iticoteri, the quicker I would feel at home with them.

It soon became apparent that I did not need to ask too many direct questions. Often the most casual remark on my part was reciprocated by a flow of information I would not have dreamed of eliciting.

Each day, just before nightfall, aided by Ritimi and Tutemi, I would go over the data gathered during the day and try to order it under some kind of classificatory scheme such as social structure, cultural values, subsistence techniques, and other universal categories of human social behavior.

However, to my great disappointment, there was one subject Milagros had not touched upon: shamanism. I had observed from my hammock two curing sessions, of which I had written detailed accounts.

"Arasuwe is a great *shapori*," Milagros had said to me as I watched my first curing ritual.

"Does he invoke the help of the spirits when he chants?" I asked as I watched Milagros's brother-in-law massage, suck, and rub the prostrate body of a child.

Milagros had given me an outraged look. "There are things one doesn't talk about." He had gotten up abruptly and before walking out of the hut had added, "Don't ask about these things. If you do, you will run into serious trouble."

I had not been surprised by his response, but I had been unprepared for his outright anger. I wondered if his refusal to talk about the subject was because I was a woman or rather that shamanism was a taboo topic. I did not dare to find out at the time. Being a woman, white, and alone was precarious enough.

I was aware that in most societies knowledge regarding shamanistic and curing practices is never revealed except to the initiates. During Milagros's absence I did not mention the word shamanism once but spent hours deliberating over what would be the best way to learn about it without arousing any anger and suspicion.

From my notes on the two sessions it became evident that the Iticoteri believed the *shapori*'s body underwent a change when under the influence of the hallucinogenic snuff *epena*. That is, the shaman acted under the assumption that his human body transformed itself into a supernatural body. Thus he made contact with the spirits in the forest. My obvious approach would be to arrive at an understanding of shamanism via the body – not as an object determined by psychochemical laws, holistic forces in nature, the environment, or the psyche itself, but through an understanding of the body as lived experience, the body as an expressive unity known through performance.

Most studies on shamanism, including mine, have focused on the psychotherapeutic and social aspects of healing. I thought that my approach would not only provide a novel explanation but would furnish me with a way of learning about curing without becoming suspect. Questions concerning the body need not necessarily be associated with shamanism. I had no doubt that little by little I would retrieve the necessary data without the Iticoteri ever being aware of what I was really after.

Any pangs of conscience I felt regarding the dishonesty of my task were quickly stilled by repeating to myself that my work was important for the understanding of non-Western healing practices. The strange, often bizarre customs of shamanism would become understandable in the light of a different interpretational context, thus furthering anthropological knowledge in general.

"You haven't worked for two days," Ritimi said to me one afternoon. "You haven't asked about last night's songs and dances. Don't you know they are important? If we don't sing and dance the hunters will return without meat for the feast." Scowling, she threw the notepad into my lap. "You haven't even painted in your book."

"I'm resting for a few days," I said, clutching the notepad against my breast as if it were the dearest thing I possessed. I had no intention of letting her know that every precious page was to be filled exclusively with data on shamanism.

Ritimi took my hands in hers, examined them intently, then, assuming a very serious expression, commented, "They look very tired – they need rest."

We burst out laughing. Ritimi had always been baffled that I considered decorating my book to be work. To her work meant digging weeds in the garden, collecting firewood, and repairing the roof of the *shabono.*

"I liked the dances and songs very much," I said. "I recognized your voice – it was beautiful."

Ritimi beamed at me. "I sing very well." There was a charming candor and assurance in her statement; she was not boasting but only stating a fact. "I'm sure the hunters will return with plenty of game to feed the guests at the feast."

Nodding in agreement, I looked for a twig, then began to sketch a human figure on the soft dirt. "This is the body of a white person," I said as I sketched the main organs and bones. "I wonder how the body of an Iticoteri looks?"

"You must be very tired to ask such a stupid question," Ritimi said, staring at me as if I were dim-witted. She stood up and began to dance, chanting in a loud melodious voice: "This is my head, this is my arm, this is my breast, this is my stomach, this is my . . ."

In no time at all, attracted by Ritimi's antics, a group of women and men gathered around us. Squealing and laughing, they made obscene remarks about each other's bodies. Some of the adolescent boys were laughing so hard, they rolled on the ground, holding their penises.

"Can anyone draw a body the way I drew mine?" I asked.

Several responded to this challenge. Grabbing a piece of wood, a twig, or a broken bow, they began to draw on the dirt. Their drawings differed markedly from each other's, not only because of the obvious sexual differences, which they made sure to emphasize, but because all the men's bodies were depicted with tiny figures inside the chest.

I could hardly hide my delight. I thought these must be the spirits I had heard Arasuwe summon with his chant before he began the curing session. "What are these?" I asked casually.

"The *hekuras* of the forest who live in a man's chest," one of the men said.

"Are all men *shapori*?"

"All men have *hekuras* in their chests," the man said. "But only a real *shapori* can make use of them. Only a great *shapori* can command his *hekuras* to aid the sick and counteract the spells of enemy *shapori.*" Studying my sketch, he asked, "Why does your picture have *hekuras*, even in the legs? Women don't have *hekuras.*"

I explained that these were not spirits, but organs and bones, and they promptly added them to their own drawings. Content with what I had learned, I willingly accompanied Ritimi to gather firewood in the forest – the women's most arduous and unwelcome task.

They could never get enough wood, for the fires were never allowed to die.

That evening, as she had done every night since I arrived at the settlement, Ritimi examined my feet for thorns and splinters. Satisfied that there were none, she rubbed them clean with her hands.

"I wonder if the bodies of the *shapori* go through some kind of transformation when they are under the influence of *epena*," I said. It was important to have it confirmed in their own words, since the original premise of my theoretical scheme was that the shaman operated under certain assumptions concerning the body. I needed to know if these assumptions were shared by the group and if they were of a conscious or unconscious nature.

"Did you see Iramamowe yesterday?" Ritimi asked. "Did you see him walk? His feet didn't touch the ground. He is a powerful *shapori*. He became the great jaguar."

"He didn't cure anyone," I said glumly. It disappointed me that Arasuwe's brother was considered a great shaman. I had seen him beat his wife on two occasions.

No longer interested in pursuing the conversation, Ritimi turned away from me and began to get ready for our evening ritual. Lifting the basket that held my belongings from the small loft at the back of the hut, she placed it on the ground. One by one she took out each item and held it above her head, waiting for me to identify it. As soon as I did she repeated the name in Spanish, then in English, starting a nocturnal chorus as the headman's wives and several other women who each night gathered in our hut echoed the foreign words.

I relaxed in my hammock as Tutemi's fingers parted my hair searching for imaginary lice; I was certain I did not have any – not yet. Tutemi appeared to be five or six years younger than Ritimi, whom I believed to be twenty. She was taller and heavier, her stomach round with her first pregnancy. She was shy and retiring. Often I had discovered a sad, faraway look in her dark eyes, and at times she talked to herself as if she were thinking aloud.

"Lice! Lice!" Tutemi shouted, interrupting the women's Spanish-English chant.

"Let me see," I said, convinced that she was joking. "Are lice white?" I asked, examining the tiny white bugs on her finger. I had always believed they were dark.

"White girl, white lice," Tutemi said mischievously. With gleeful delight she crunched them one by one between her teeth and swallowed them. "All lice are white."

# Appendix 1: Key Ethnographic, Sociological, Qualitative, and Multidisciplinary Fieldwork Methods Texts (selected for relevance to anthropology; chronological, newest first)

- *The SAGE Handbook of Qualitative Research* (2005), edited by Norman Denzin and Yvonna Lincoln. 3rd edn.
- *Fieldwork* (2005), edited by Christopher Pole. 4 vols.
- *The SAGE Handbook of Fieldwork* (2005), edited by Dick Hobbs and Richard Wright.
- *Recording Oral History: A Guide for the Humanities and Social Sciences* (2005), by Valerie Raleigh Yow. 2nd edn.
- *Dialogue with the Past: Engaging Students and Meeting Standards through Oral History* (2004), by Glen Whitman.
- *Qualitative Research Methods for the Social Sciences* (2003), by Bruce Berg. 5th edn.
- *Participant Observation: A Guide for Fieldworkers* (2002), by Kathleen and Billie DeWalt.
- *Qualitative Research Methods* (2002), edited by Darin Weinberg.
- *The American Tradition in Qualitative Research* (2001), edited by Norman Denzin and Yvonna Lincoln. 4 vols.
- *Handbook of Ethnography* (2001), edited by Paul Atkinson et al.
- *Ethnography* (2001), edited by Alan Bryman. 4 vols.
- *Reflexive Methodology: New Vistas for Qualitative Research* (2000), by Mats Alvesson and Kaj Sköldberg.
- *Ethnographer's Toolkit* (1999), edited by Jean Schensul and Margaret LeCompte. 7 vols.
- *Qualitative Research* (1999), edited by Alan Bryman and Robert Burgess. 4 vols.

- *Basics of Qualitative Research: Techniques and Procedures for Developing Grounded Theory* (1998), by Anselm Strauss and Juliet Corbin. 2nd edn.
- *The World Observed: Reflections on the Fieldwork Process* (1996), edited by Bruce Jackson and Edward Ives.
- *Journeys Through Ethnography: Realistic Accounts of Fieldwork* (1996), edited by Annette Lareau and Jeffrey Shultz.
- *Co-operative Inquiry: Research Into the Human Condition* (1996), by John Heron.
- *Ethnography: Principles in Practice* (1995), by Martyn Hammersley and Paul Atkinson. 2nd edn.
- *The Active Interview* (1995), by James Holstein and Jaber Gubrium.
- *Interpreting the Field: Accounts of Ethnography* (1993), edited by Dick Hobbs and Tim May.
- *Ethnography: Step-by-Step* (1992), by David Fetterman.
- *Participant Observation: A Methodology for Human Studies* (1989), by Danny Jorgensen.
- *Systematic Fieldwork* (1987), by Oswald Werner and G. Mark Schoepfle. 2 vols.
- *The Politics and Ethics of Fieldwork* (1986), by Maurice Punch.
- *Interpreting Life Histories: An Anthropological Inquiry* (1985), by Lawrence C. Watson and Maria-Barbara Watson-Franke.
- *Learning From the Field: A Guide From Experience* (1984), by William Foote Whyte.
- *Social Researching: Politics, Problems, Practice* (1984), edited by Colin Bell and Helen Roberts.
- *Qualitative Methodology* (1983), edited by John Van Maanen.
- *Ethnographic Research* (1982), by Marion Dobbert.
- *People Studying People: The Human Element in Fieldwork* (1980), by Robert Georges and Michael Jones.
- *Fieldwork Experience: Qualitative Approaches to Social Research* (1980), edited by William Shaffir, Robert Stebbins, and Allan Turowetz.
- *The Fieldworker and the Field: Problems and Challenges in Sociological Investigation* (1979), edited by M. Srinivas, A. Shah, and E. Ramaswamy.
- *Doing Sociological Research* (1977), edited by Colin Bell and Howard Newby.
- *Participant Observation: Theory and Practice* (1975), by Jürgen Friedrichs and Hartmut Lüdtke.
- *Introduction to Qualitative Research Methods: A Phenomenological Approach to the Social Sciences* (1975), by Robert Bogden and Steven Taylor.
- *The Research Adventure: Promise and Problems in Fieldwork* (1972), by Myron Glazer.
- *Research on Deviance* (1972), edited by Jack Douglas.
- *Issues in Participant Observation: A Text and Reader* (1969), edited by George McCall and Jerry Simmons.
- *Hustlers, Beats, and Others* (1967), by Ned Polsky.
- *Reflections on Community Studies* (1964), edited by Arthur Vidich, Joseph Bensman, and Maurice Stein.
- *Sociologists at Work: Essays on the Craft of Social Research* (1964), edited by Phillip Hammond.

# Appendix 2: Edited Cultural Anthropology Volumes on Fieldwork Experiences (chronological; oldest first)

- *In the Company of Man: Twenty Portraits of Anthropological Informants* (1960), edited by Joseph Casagrande.
- *Anthropologists in the Field* (1967), edited by D. Jongmans and P. Gutkind.
- *Stress and Response in Fieldwork* (1969), edited by Frances Henry and Satish Saberwal.
- *Women in the Field: Anthropological Experiences* (1970), edited by Peggy Golde.
- *Being an Anthropologist: Fieldwork in Eleven Cultures* (1970), edited by George Spindler.
- *Marginal Natives: Anthropologists at Work* (1970), edited by Morris Freilich.
- *Crossing Cultural Boundaries: The Anthropological Experience* (1972), edited by Solon Kimball and James Watson.
- *Encounter and Experience: Personal Accounts of Fieldwork* (1975), edited by André Béteille and T. N. Madan.
- *Anthropologists at Home In North America: Methods and Issues in the Study of One's Own Society* (1981), edited by Donald Messerschmidt.
- *Observers Observed: Essays on Ethnographic Fieldwork* (1983), edited by George Stocking.
- *Doing Fieldwork: Eight Personal Accounts of Social Research* (1989), edited by John Perry.
- *The Humbled Anthropologist: Tales From the Pacific* (1990), edited by Philip DeVita.
- *Experiencing Fieldwork: An Inside View of Qualitative Research* (1991), edited by William Shaffir and Robert Stebbins.
- *The Naked Anthropologist: Tales From Around the World* (1992), edited by Philip DeVita.
- *Fieldwork Under Fire: Contemporary Studies of Violence and Survival* (1995), edited by Carolyn Nordstrom and Antonius Robben.
- *In the Field: Readings on the Field Research Experience* (1996), edited by Carolyn Smith and William Kornblum.

- *Out in the Field: Reflections of Lesbian and Gay Anthropologists* (1996), edited by Ellen Lewin and William Leap.
- *Fieldwork Revisited: Changing Contexts of Ethnographic Practice in the Era of Globalization* (1997), special issue of *Anthropology and Humanism* 22:1, edited by Joel Robbins and Sandra Bamford.
- *Being There: Fieldwork in Anthropology* (1999), edited by C. W. Watson.
- *Anthropological Journeys: Reflections on Fieldwork* (1998), edited by Meenakshi Thapan.
- *Stumbling Toward Truth: Anthropologists at Work* (2000), edited by Philip DeVita.
- *Fieldwork Dilemmas: Anthropologists in Postsocialist States* (2000), edited by Hermine De Soto and Nora Dudwick.
- *Constructing the Field: Ethnographic Fieldwork in the Contemporary World* (2000), edited by Vered Amit.
- *Anthropologists in the Field: Cases in Participant Observation* (2004), edited by Lynne Hume and Jane Mulcock.

# Appendix 3: Reflexive Accounts of Fieldwork and Ethnographies Which Include Accounts of Fieldwork (chronological; oldest first)

- *Greener Fields: Experiences among the American Indians* (1953), by Alice Marriott.
- *Hindus of the Himalayas* (1963), by Gerald Berreman.
- *The Savage and the Innocent* (1965), by David Maybury-Lewis.
- *The High Valley* (1966), by Kenneth Read.
- *Never in Anger: Portrait of an Eskimo Family* (1970), by Jean Briggs.
- *Doing Fieldwork: Warnings and Advice* (1971), by Rosalie Wax.
- *Down among the Wild Men: The Narrative Journal of Fifteen Years Pursuing the Old Stone Age Aborigines of Australia's Western Desert* (1972), by John Greenway.
- *Studying the Yanomamö* (1974), by Napoleon Chagnon.
- *When the Spider Danced: Notes from an African Village* (1975), by Alexander Alland, Jr.
- *African Odyssey: An Anthropological Adventure* (1976), by Mariam Slater.
- *Reflections on Fieldwork in Morocco* (1977), by Paul Rabinow.
- *An Asian Anthropologist in the South: Field Experiences with Blacks, Indians and Whites* (1977), by Choong Soon Kim.
- *The Headman and I: Ambiguity and Ambivalence in the Fieldworking Experience* (1978), by Jean-Paul Dumont.
- *The Bamboo Fire: An Anthropologist in New Guinea* (1978), by William Mitchell.
- *Tuhami: Portrait of a Moroccan* (1980), by Vincent Crapanzano.
- *Assault on Paradise: Social Change in a Brazilian Village* (1983), by Conrad Kottak.
- *Return to the High Valley: Coming Full Circle* (1986), by Kenneth Read.

- *In Sorcery's Shadow: A Memoir of Apprenticeship Among the Songhay of Niger* (1987), by Paul Stoller and Cheryl Olkes.
- *Nest in the Wind: Adventures in Anthropology on a Tropical Island* (1989), by Martha Ward.
- *Hearts and Minds, Water and Fish: Popular Support for the IRA and INLA in a Northern Irish Ghetto* (1989), by Jeffrey Sluka.
- *Inis Beag Revisited: The Anthropologist as Observant Participator* (1989), by John Messenger.
- *First Fieldwork: The Misadventures of an Anthropologist* (1990), by Barbara Anderson.
- *Road Through the Rain Forest: Living Anthropology in Highland Papua New Guinea* (1990), by David Hayano.
- *Fighting for Faith and Nation: Dialogues With Sikh Militants* (1996), by Cynthia Mahmood.
- *Mad Dogs, Englishmen, and the Errant Anthropologist: Fieldwork in Malaysia* (1996), by Douglas Raybeck.
- *Being There: The Necessity of Fieldwork* (1998), by Daniel Bradburd.
- *An Anthropologist in Japan: Glimpses of Life in the Field* (1999), by Joy Hendry.
- *Around the World in 30 Years: Life as a Cultural Anthropologist* (1999), by Barbara Anderson.
- *The Politics of Fieldwork: Research in an American Concentration Camp* (2001), by Lane Hirabayashi.
- *Doing Fieldwork in Japan* (2003), edited by Theodore Bestor et al.
- *The Ethnographic I: A Methodological Novel about Autoethnography* (2004), by Carolyn Ellis.

# Appendix 4: Leading Cultural Anthropology Fieldwork Methods Texts (chronological; oldest first)

- *Notes and Queries on Anthropology* (1951), Royal Anthropological Institute of Great Britain and Ireland. 6th edn.
- *Method and Perspective in Anthropology: Essays in Honor of Wilson D. Wallis* (1954), by Robert F. Spencer.
- *Methods in Social Anthropology: Selected Essays* (1958), by A. R. Radcliffe-Brown.
- *Human Organization Research: Field Relations and Techniques* (1960), edited by Richard Adams and Jack Preiss.
- *The Anthropology of Music* (1964), by Alan Merriam.
- *Field Methods in the Study of Culture* (1967), by Thomas Williams.
- *The Craft of Social Anthropology* (1967), edited by A. L. Epstein.
- *A Handbook of Method in Cultural Anthropology* (1970), edited by Raoul Narroll and Ronald Cohen.
- *Crossing Cultural Boundaries: The Anthropological Experience* (1972), edited by Solon Kimball and James Watson.
- *Handbook of Social and Cultural Anthropology* (1973), edited by John Honigmann.
- *Methods and Styles in the Study of Culture* (1974), by Robert Edgerton and L. Langness.
- *Ethnographic Film* (1976), by Karl Heider.
- *Anthropological Research: The Structure of Inquiry* (1978), by Pertti Pelto and Gretel Pelto. 2nd edn.
- *The Craft of Community Study: Fieldwork Dialogues* (1979), by Solon Kimball and William Partridge.
- *The Ethnographic Interview* (1979), by James Spradley.
- *Participant Observation* (1980), by James Spradley.
- *The Professional Stranger: An Informal Introduction to Ethnography* (1980), by Michael Agar.
- *Speaking of Ethnography* (1986), by Michael Agar.

- *Systematic Fieldwork* (1987), by Oswald Werner and G. Mark Schoepfle. 2 vols.
- *The Varieties of Sensory Experience: A Sourcebook in the Anthropology of the Senses* (1991), edited by David Howes.
- *Ethnomusicology: An Introduction* (1992), edited by Helen Myers.
- *Field Projects in Anthropology: A Student Handbook* (1992), edited by Julia Crane and Michael Angrosino.
- *Cross-Cultural Filmmaking: A Handbook for Making Documentary and Ethnographic Films and Videos* (1997), by Ilisa Barbash and Lucien Taylor.
- *Handbook of Methods in Cultural Anthropology* (1998), edited by H. Russell Bernard.
- *Doing Cultural Anthropology: Projects for Ethnographic Data Collection* (2002), edited by Michael Angrosino.
- *Projects in Ethnographic Research* (2005), by Michael Angrosino.
- *Research Methods in Anthropology: Qualitative and Quantitative Approaches* (2005), by H. Russell Bernard. 4th edn.

# Appendix 5: Early and Classic Anthropological Writings on Fieldwork, including Diaries and Letters (chronological; oldest first)

- Bronislaw Malinowski, "Introduction: The Subject, Method and Scope of This Inquiry," in *Argonauts of the Western Pacific* (1922), and his posthumously published *A Diary in the Strict Sense of the Term* (1967).
- On Franz Boas' diary see Rohner 1969.
- Paul Radin, *The Method and Theory of Ethnology: An Essay in Criticism* (1933).
- Audrey Richards, "The Development of Field Work Methods in Social Anthropology" (1939).
- Ralph Piddington, "Methods of Field Work" (1950).
- Claude Lévi-Strauss, *Tristes Tropiques* (1955).
- On Lowie's fieldwork see his *Robert H. Lowie, Ethnologist: A Personal Record* (1959).
- E. E. Evans-Pritchard, "Some Reminiscences and Reflections on Fieldwork" (1973).
- On Ruth Benedict's fieldwork see Mead 1959 and 1977.
- On the fieldwork correspondence of Robert Redfield and Sol Tax, see Rubinstein 1991.

# Notes

## NOTE TO CHAPTER 1

1 It is unnecessary to give warning that the critical reflections that we are making on the accounts of explorers are levelled at the usual run of these accounts, and consequently admit notable exceptions. Far be it from us to wish a lessening of the admiration due to men like Cook, Bougainville and others. In this respect, you will have preceded us: it has been your first concern to study their writings.

## NOTES TO CHAPTER 3

1 I may note at once that there were a few delightful exceptions to that, to mention only my friends Billy Hancock in the Trobriands; M. Raffael Brudo, another pearl trader; and the missionary, Mr M. K. Gilmour.

2 According to a useful habit of the terminology of science, I use the word Ethnography for the empirical and descriptive results of the science of Man, and the word Ethnology for speculative and comparative theories.

3 The legendary "early authority" who found the natives only beastly and without customs is left behind by a modern writer, who, speaking about the Southern Massim with whom he lived and worked "in close contact" for many years, says: – ". . . We teach lawless men to become obedient, inhuman men to love, and savage men to change." And again: – "Guided in his conduct by nothing but his instincts and propensities, and governed by his unchecked passions. . . ." "Lawless, inhuman and savage!" A grosser misstatement of the real state of things could not be invented by anyone wishing to parody the Missionary point of view. Quoted from the Rev. C. W. Abel, of the London Missionary Society, "Savage Life in New Guinea," no date.

4 It was soon after I had adopted this course that I received a letter from Dr A. H. Gardiner, the well-known Egyptologist, urging me to do this very thing. From his point of view as archæologist, he naturally saw the enormous possibilities for an Ethnographer of obtaining a similar body of written sources as have been preserved to us from ancient cultures, plus the possibility of illuminating them by personal knowledge of the full life of that culture.

## NOTE TO CHAPTER 4

1 Unfortunately, I neglected to get its technical name and in my notes used the pidgin English, "big sickness." It seemed similar to polio.

## NOTES TO CHAPTER 5

1 For a discussion of the continuing significance of van Gennep to cultural anthropology, see Zumwalt (1982).
2 I prefer the phrase "ethnographic research" to the more customary reference to "fieldwork." Though the Boasians suggested that the rest of the world outside of one's own culture/subculture (Anglo-American middle/upper middle-class academic) ought to be considered a laboratory for the comparative examination of sociocultural specimens, I still wince when I hear the term "fieldwork." The politics of travel and access surround the phrase. Benjamin Whorf reminded us that the words we use affect us in subtle ways. I think if one uses the term "fieldwork" often enough and long enough, one will think and behave in the world as one might similarly behave in any other type of "laboratory."
3 A mystique has been built up surrounding the transformational nature of ethnographic research. See, for example, Braroe and Hicks (1967) and Sontag (1966).
4 On early ethnography, see Hodgen (1964).
5 For Bequia, the initial pilot study described here was funded by grant No. 1-0-101-3284-VC851 from the University Research Council, University of North Carolina at Chapel Hill. Ethnographic research in West Haven was funded by grant No. MH58496-01 from the National Institute of Mental Health. While writing this article, I held a grant from the Spencer Foundation.
6 These phases in the sequence are marked by publicly recognized secular ceremonial events and activities closer to the idea of rites than to the strictly religious idea of ritual.
7 One explanation for the comparative lack of attention to maritime traditions in the Caribbean is that the impact of colonial and neocolonial cultures on the islands has focused ethnographic attention on matters pertaining to economics, social organization, and politics.
8 In exchange for my help I was invited to carve my name in the keel of his schooner.
9 I did not pretend that I had "gone native." The point here is that I was offered an opportunity for a closer degree of participation in the group I came to study. I did not feel as if I was a Bequian. I felt I was being recognized as a "man of the sea," as it is termed on the island. The identification is emotional and is based on the recognition of shared knowledge with respect to particular experiences. I am reminded of a passage from Powdermaker's (1966:112) account of her stay on the island of New Ireland:

> . . . the drums began; I danced. Something happened, I forgot myself and was one with the dancers. Under the full moon and for the brief time of dance, I ceased to be an anthropologist from a modern society. I danced. When it was over I realized that, for this short period, I had been emotionally part of the rite. Then out came my notebook.

Also see Shostak (1981:1–43) and Wax (1971:44–6) on recognizing the dangers of overidentification.
10 Transitional events and activities are variable. Not everyone trying to access Bequia culture would be asked to sail a boat. The demonstration of knowledge and skill is related to the subject of the ethnographic inquiry.
11 For Steward (1972:50), education in the form of mass schooling is important to the maintenance of sociocultural integration at the national level:

> The effects of nationally shared practices of child-training and family patterns, of *common participation in national institutions*, and of mass communications all serve to develop national uniformities of individual behavior (my italics).

12 On the politics of anthropology, see Aberle's (1967) thoughts on anthropology and imperialism, Diamond (1974) on anthropology and the colonial encounter, and Long (1980) and Stocking (1968) on the relationship between the "primitive"/"civilized" opposition and the color and sex of the ethnographer.

## NOTES TO CHAPTER 6

1 See, for instance, Malinowski (1961) [1922]; Lévi-Strauss (1955) [1984]; Turnbull (1968); Chernoff (1979); and, in more recent years, Trawick (1990).
2 Notable exceptions include Cesara (1982); Read (1972); and Schneebaum (1969, 1979, 1988).
3 A pseudonym. It is unfortunate that her fascinating, reflexive account of her fieldwork experiences in 'Lenda' is so difficult to obtain. It has long been out of print, but can sometimes be found tucked away in secondhand bookstores in university towns.
4 A pseudonym.
5 See, for example, Whitehead and Conaway (1986).
6 See, for example, Myerhoff (1978); Fernandez (1982); and, in more recent years, Rose (1987); Stoller and Olkes (1987); Brown (1991); Behar (1993).
7 Her book A *Natural History of the Senses* (Ackerman 1990) is an extraordinary exploration of the pleasures of sensory experiences and should be required reading for anthropologists embarking upon journeys into the field. A poet and professor of literature, she has much to offer to social scientists wishing to expand and develop their sensibilities, and the written expression of those sensibilities, in any given locale.
8 This *Diary* (first published in 1967), and its impact on the anthropological community, have been written about extensively. For an excellent analysis of the *Diary* in regard to sexuality and Malinowski's sometimes drastic attempts to 'contain' his feelings and urges, see Torgovnick (1990).
9 As Raymond Firth points out, in the introduction to Malinowski's *Diary*, Malinowski's sincere love and respect for the woman he would marry has been well documented (Firth 1989: xviii). However, it is also noted that he had failed to break off his emotional link with another woman with whom he had formerly been involved, which would certainly have added to his self-recriminations, remorse, and abject discomfort with himself during his stay in New Guinea (p. xix).
10 Anthropologists Whitehead and Conaway (1986) write about the traditional ethnographic endeavor, in which the anthropologist attempts to achieve objectivity. They note accurately that the more personalized accounts of field experiences were often considered to be 'an unmitigated self-indulgence'. They claim that the prevailing sentiment in the profession held that 'confessional narratives' were considered to be appropriate for travel writers, but not for them (p. 2). This is fascinating, especially when we consider that the anthropologist invariably travels to other places. In some respects, we *are* travel writers. This rejection of the personal response to the field began to yield a bit with the advent of more reflexive accounts, exemplified by the work of Myerhoff and Ruby (1982) and, more recently, Behar (1993) and Brown (1991).
11 Torgovnick offers an excellent example of this approach in practice when she discusses the writings of anthropologist Tobias Schneebaum, who entered into the rituals of a men's group which went on a mission involving the killing of enemies, male homosexual rituals, and the cannibalizing of dead enemies. Torgovnick writes that Schneebaum was stunned by his encounter with 'the primitive within himself' (1990: 182). She notes that his book *The Wild Man* (Schneebaum 1979) has been out of print since its first edition was released, due to what Torgovnick describes as its 'aggressive endorsement of homosexuality among primitives as the "natural" thing to do' (1990: 182).

Schneebaum himself writes freely and openly about his experiences, according to Torgovnick. One can't help but wonder if his writings would have caused more of an uproar if they had not been homosexually contextualized, i.e. if he had had passionate affairs with women 'natives' and written about it as candidly as he wrote about his homoerotic experiences. It's almost as though he could be written off – the marginal writing about the marginalized – since his behavior didn't involve a cross-gender interaction. This is a sad indictment of one of the ways in which our culture compartmentalizes and privileges certain kinds of experience as being more meaningful and, perhaps, more worthy of being entered into the academic discourse.

12 I do not mean to imply that women do not read the sports page, although I did not witness any women doing so during my stay in the fire camp. Rather, I point to the casual act of sharing the sports page as an example of a way in which the camp took on many of the characteristics of a family setting, where members might typically pass a section of the paper on to one another after reading it.

13 Several women in the camp informed me that the women tend to stick together when it comes to sleeping arrangements, either putting their one-person tents up in close proximity to one another or sharing a large tent. They do this in order to 'feel safer', as one woman put it, as well as to 'be with our own kind' after long days of working in a situation where they are greatly outnumbered by males.

14 A volume of essays written by anthropologists on the anthropology of place, edited by Margaret Rodman and Terri Aihoshi, is currently under review for publication. That volume, entitled *Spacializing Narratives: The Anthropology of Place*, stands on the cutting edge of the scholarly discourse on the meanings of place. For other writings by anthropologists, one can turn to Altman and Low (1992); Rodman (1992); Basso (1988); Appadurai (1988). However, writings on place have traditionally been undertaken by those working in other disciplines, including architects and landscape architects, historians, geographers, and sociologists. Some of the best include Agnew and Duncan (1989); Appleton (1975); Entrikin (1991); Fitchen (1991); Hiss (1990); Hough (1990); Jackson (1984); Pred (1990); Tuan (1974, 1977); Walter (1988).

My doctoral work was done through the Union Graduate School in Cincinnati. Dedicated to interdisciplinary enquiry and a student-structured program of study, the university values work that marries rigorous, scientific research with the humanistic and creative. The publication rate for Union graduates is high, a tribute to scholarly work that tends to be both academically solid and pleasurable to read.

15 In fairness, it must be pointed out that I did not enquire into the topic of same-sex sexual relationships while in the camp, the subject being outside of the scope of my original project. Therefore, such relationships may well exist, just as 'fireline romances' exist between men and women. Clearly, this indicates a direction for needed future research.

16 Other 'instant communities' often have this erotic dimension and dynamic operative within them, including those in which racing-car drivers and racing sailors ply their trades. These activities, highly collaborative by nature, seem to foster an atmosphere of sensuality amidst a tense and gritty environment of competition.

17 I would point out here that this behavior, while considered socially appropriate between 'equals' (in terms of rank and power), does not seem to go on between, for instance, members of the management team and the firefighters themselves. The acts of playful flirting that I witnessed took place inevitably between social equals within that particular system, when they took place at all. Further, they were contextualized as 'fooling around' and not typically construed to be 'sexual' in connotation.

18 I am indebted to Mary Catherine Bateson for reminding me of the erotic element of heterosexual male bonding in certain settings, and the importance of that notion in this context.

19 See Newton's commentary (1993a: 7) for an elaboration on this.

20 There are sometimes problems with experimental writing forms. Birth (1990: 556), for instance, cautions against the simplistic discarding of the frame of referentiality, to be replaced by an ethereal poetics which can be indecipherable to a reading audience. Other critics of such

writings (e.g. Caplan 1988; Mascia-Lees *et al.* 1989; Sangren 1988) remind us of what should be obvious; namely, that we have a responsibility to the reading audience. As Margery Wolf points out (1992: 138), 'The message of exclusion that attaches to some of these [experimental] texts contradicts the ostensible purpose of experimental ethnography, [which is] to find better ways of conveying some aspect of the experiences of another community.'

21 This acutely pointed phrase was used by two well-respected professionals I have spoken to, one a writer of fiction and the other a corporate executive working in a setting where a premium of respect is paid to those who can get rapidly to 'the bottom line' without going around the linguistic merry-go-round.

22 The humanists in the field of anthropology are a welcome exception here, dedicated to the exploration of different writing genres and styles in their writings. Represented in the discipline by the Society for Humanistic Anthropology (a unit of the American Anthropological Association), they continue to advocate the inclusion of poetics, fiction, and other forms of evocative expression in ethnographic writings. Many wonderfully written books, filled with sensual imagery and reflexivity, have been (and continue to be) the fruit of these efforts.

## NOTES TO CHAPTER 7

This is the second of a three-part project which examines anthropological constructions of self and other. The first paper compared some uses of these categories in the British and French traditions (Cohen 1989). The present chapter explores ethnographic implications of 'the Self': how we, as anthropologists, conceptualise self-hood among those whom we study, and how our concepts relating to 'the self' derive from, and/or contribute to our own self-knowledge. The final instalment will relate consciousness of self to the idea of personal identity, and will argue that this sense of personhood must be acknowledged as the fundamental human right in order that it may be protected from subversion and abuse by political, economic and other sources of power (Cohen 1993).

1 During the 1960s, Newfoundland generally, and rural Newfoundland in particular, was stigmatised in mainland Canada by the cult of 'humour' known as the 'Newfie joke'. This was a vicious and racist depiction of a backwardness, an exaggerated form of the Polish and Irish jokes which flourish elsewhere. Newfoundland only joined the Confederation of Canada in 1949. For many years thereafter it retained the characteristic features of underdevelopment: unemployment and underemployment; high rates of outmigration, infant mortality, and tuberculosis; a rudimentary infrastructure, shortage of capital, meagre educational provision, intense sectarianism, and so forth.

2 An eminent Jewish scholar, Rabbi Jonathan Sacks [now chief Rabbi of the UK], recently echoed this sentiment: 'If Judaism no longer unites Jews, overachieving does' (Sacks 1989).

3 Interestingly, Dumont's own taxonomy of 'individualism' as a cultural mode in India has been challenged recently (Mines 1988).

4 See, e.g. Paul Spencer's sensitive illustration (1989) and Okely and Callaway (1992).

5 Lock notes that self-awareness is necessarily anchored in time and place (1981: 24).

## NOTES TO CHAPTER 10

1 See, for example, Edgerton 1990, Feldman 1991, Lan 1985, Nordstrom and Martin 1992, Ranger 1985, Sluka 1989, Stoll 1993, and Zulaika 1988.

2 The expression *bombardeo de amor* (love bombardment) is also often heard. It means the heaping of praise on prominent public figures, only to defame and even slander them when they refuse to accommodate their flatterers. The love bombardment is comparable to seduction

in the sense that both are political practices that try to make people lose their critical distance and independent judgment.

3 This is not the place to discuss whether transference can be attributed to displacement or projection, but the distinction deserves at least some attention. Displacement implies that transference is an object relation, an expression of repressed desires displaced on the analyst, comparable to their displacement on dreams, jokes, and slips of the tongue. When transference is regarded as a projection of those desires, then the analyst is not just the object but also a substitute, namely a substitute for another person for whom those desires are actually intended. For an extensive discussion, see Jordan (1992) and Zetzel (1956).

4 Waelder (1956) suggests in his opening address to the 1955 International Psycho-Analytical Congress that an analyst might consider using positive transference to correct behavior or to strengthen the analysand's superego by utilizing the identification with the analyst.

5 See the polemic between Lewin (1993) and Stein (1993) about whether or not the restoration of a damaged ego into an integrated whole is possible and even desirable.

6 Little has been written on the analysis of perpetrators of violence (notable exceptions are several articles in Bergmann and Jucovy 1982). The reported defense reactions are comparable to those evoked in the analysis of holocaust survivors.

7 Lothane (1987:99–100) mentions two transference triads in which Freud himself deliberately manipulated the analysand away from a proper interpretation of compromising affairs, namely those between Breuer and Anna O., and between Jung and Sabina Spielrein.

8 Baudrillard (1990) criticizes Freud for reducing seduction to sex and desire. Freud gives a purely sexual interpretation of gender relations and does not realize that seduction is a female counterhegemonic strategy against male sexual dominance. Men obtain power through sexual and economic domination, while women have power by manipulating the symbolic universe of appearance through seduction, according to Baudrillard.

9 Obeysekere's (1990:232) term *cultural transference* has a counterpart in *cultural seduction* in the case where the ethnographer deliberately employs his or her status as an outsider to provoke transferences among the informants in order to establish a "good rapport."

10 The circumstances of the interview also added to the success of the general's strategy. This unusual interview took place while the general was about to go on trial for ordering the disappearance of Argentine citizens, and for carrying the hierarchical responsibility for their rape and torture by his men, in the provinces that had stood under his military command. We were both aware of the importance of this interview for my research, and we both realized that a break in our rapport might be sufficient reason to end it. Later I realized that his interest in the interview might have been to use me as an intellectual sparring partner for his upcoming trial; a trial that was eventually dismissed by a presidential decree (*indulto*). Below, I shall elaborate on the importance of ethnographic seduction in the researcher's desire to gain access to data that are hard to get.

11 One common indication of countertransference is that one is tongue-tied upon hearing a rhetorical argument and is only able to formulate convincing rebuttals after the interview has ended. For a detailed discussion of the rhetorical dimension of seduction see Robben (1995).

12 At the risk of sounding repetitive I must of course again mention Devereux as an exception. Repression makes the ethnographer "protect himself against anxiety by the omission, softpedalling, non-exploitation, misunderstanding, ambiguous description, over-exploitation or rearrangement of certain parts of his material" (Devereux 1967:44). Unfortunately, Devereux's important work on countertransference has failed to enter

mainstream theoretical thought on fieldwork.

13 Part of the dialogue quoted here can be found in almost the exact same words in Cohen Salama (1992:230).

14 I owe several of these points to a personal communication with Vincent Crapanzano.

15 The filmmaker Claude Lanzmann tested these limits when he virtually coerced Abraham Bomba, a survivor of Treblinka, to recall his experiences for the documentary *Shoah*: "*AB:* A friend of mine worked as a barber – he was a good barber in my hometown – when his wife and his sister came into the gas chamber. . . . I can't. It's too horrible. Please. *CL:* We have to do it. You know it. *AB:* I won't be able to do it. *CL:* You have to do it. I know it's very hard. I know and I apologize. *AB:* Don't make me go on please. *CL:* Please. We must go on" (Lanzmann 1985:117).

## NOTES TO CHAPTER 13

1 It would be possible to expand the discussion of the responses by including readers from outside the community, such as friends and fellow professionals. Here I focus primarily on the community in which the research was conducted and its variety of responses.

2 Criticism of me as a stranger and an outsider, although I am an indigenous anthropologist with shared citizenship, religion, and language, was very powerful. I was defined as an outsider because it was then easier to oppose my analysis, and to define it as emanating from ulterior motives for personal advancement. Evans-Pritchard (1968:173–4) describes the role of the mediator among the Nuer. This role is performed by a person belonging to one certain lineage who derives his authority from being an "outsider" (belonging to a specific lineage). To the outside observer, the mediator seems hardly to differ from other Nuer people, but from within he is an outsider. I find my position as "stranger" to be quite similar.

3 It is interesting to note that none of the town's inhabitants wrote to the national newspaper, although several had promised to do so. The sense of social distance proved decisive; the townfolk did not feel sufficiently at ease or confident to contact the strange, "far-away" newspaper.

4 I had been apprehensive about objections that readers might voice with regard to my presentation of this topic, but I never imagined the intensity of the reactions. It must, however, be emphasized once more that the majority of those reacting in this manner had read only the commentary mediated by the press.

I admit to having had some misgivings about writing on the subject of a lack of family commitment, knowing that this would be a sensitive issue in Jewish culture in general, and among oriental Jews in particular. I was naive enough to believe that anyone reading the sociological explanations of this process would not regard my presentation of the phenomenon as an allocation of blame. Whereas the press's distortions came as a complete surprise, I could have foreseen that most of the local people would not read the book, but would merely seek a short summary of its contents, and would therefore have taken offense in any case. I was consciously thinking of a specific readership, made up of professionals and a slightly wider circle of interested general readers.

5 As an aside, I must admit to harboring some faint hope that the contents of the book would gain the attention of public authorities, who would then amend their policies in such a way as to bring about changes in phenomena such as apathy and lack of commitment. Unfortunately, I can report no such change of policy. What remains is the sense of humiliation and betrayal among residents.

6 Unless a local politician had learned of the book's publication, bothered to read it, and then used it to further his aims, I consider this an unlikely possibility.

7 I find a good deal of similarity between my experience and that of Wrobel, beginning with the quotes taken out of context, which lead the innocent reader to believe that the author was "blaming the victim," and continuing with the way in which other communications media took up and broadcast the issue. His reactions to the publication are also familiar, including the use of personal contacts to get friends from the community he researched to read the book, thereby gaining their understanding and moral support.

## NOTES TO CHAPTER 14

1 Michael Hout's (1989) excellent quantitative study of social mobility and industrialization in Ireland between 1959 and 1973 indicated that the 'excess' sons of rural farm families did well and better in the Irish cities to which they migrated than the urban-born children of the Irish working classes.

2 The debate swirled around the following: Sidney Callahan, 'An Anthropologist in Ireland', *Commonweal*, 25 May 1979, 310-11; Michael Viney, 'Geared for a Gale', *The Irish Times*, 24 September 1980; Nancy Scheper-Hughes, 'Reply to Viney and to Ballybran', *The Irish Times*, 21 February 1981; Eileen Kane, 'Cui Bono? Do Aon Duine?', *RAIN* 51, August 1982; Nancy Scheper-Hughes, 'Ballybran – Reply to Eileen Kane', *RAIN*, no. 51, August 1982; John Messenger, 'Reply to Kane', *RAIN*, No. 52, October 1982; Eileen Kane, 'Reply to Scheper-Hughes', *RAIN*, no. 52, October 1982; P. Nixon and P. Buckley, 'Reply to Kane', *RAIN*, no. 54, February 1983; Eileen Kane, John Blacking, M. McCann and G. McFarlane, 'Social Anthropology in Ireland – A Response', *RAIN*, No. 54, February 1983; Michael Viney, 'The Yank in the Corner: Why the Ethics of Anthropology are a Concern for Rural Ireland', *The Irish Times*, 6 August 1983; Nancy Scheper-Hughes, 'From Anxiety to Analysis: Rethinking Irish Sexuality and Sex Roles', *Journal of Women's Studies* 10, 1983: 147–60.

3 This section was inspired by a discussion between Seamus Heaney and Robert Haas on 'the art of translating poetry' at the University of California, Berkeley, on 9 February 1999.

4 According to tradition in West Kerry, the 'old ones' are expected to sense the approach of death, which was often personified as in the saying, 'Death hasn't left Cork on its way to meet me yet!', or 'He has struck me. I feel his blow in my heart.' Many an older villager would tell with great satisfaction of the moment his old mother or father took to bed and sent for the priest with the words, 'Today is my dying day' or 'Sure, I won't last the night'. A more discreet way of signaling that death was near was to ask for the final meal, what the old ones called the *Lon na Bais*. 'Auntie' Anne explained it as follows:

> One morning, about two weeks after I had returned from America, my father called me to his bedside and he asked me to bring him a large bowl of tea and two thick slices of fresh baked bread. 'Father', says I, 'you must be mistaken. Our people haven't used bowls for more than a century. You must mean a large cup of tea.' 'It's a bowl I want', he replied. I offered him some cognac to ease the pain, but he stopped me saying, 'No, my daughter, I have no more use for that – I had plenty enough when I was a boy. But today I am going to see my God.' So I did bring him the tea and the toast and I laid it next to his bed, but he never touched any of it. He just sat up in bed, smiling at it, anxiously waiting. He died that night. ... Wasn't that a beautiful death? It was what the old folks called the *Lon na Bais*, the death meal.'

## NOTES TO CHAPTER 17

1 Munapeo is a fictitious name and, in fact, is the name of an illness whose primary symptoms are that one "hurts all over – everything feels bad."

2 Gersony's (1988) interviews with Mozambican refugees who have fled the

war recorded that 90% of the severe human rights abuses in the war were attributed to Renamo.

3 All of the conversations with Mozambicans in this article were conducted in Portuguese, Mozambique's national language. The translations are my own.

4 At first glance, it might appear strange to apply a concept like "the absurd" that was formulated as an alienated response to Western techno-urban-industrial society to a bush war in Africa. The application holds for three reasons. First, contemporary dirty war is a product of modern state institutional society. Second, I resist the tendency to differentiate postmodern technological society from non-Western nonindustrialized, and by implication, (pre)modern, society. Mozambicans have long been embroiled in a transnational political economy: centuries ago no remote bush village was safe from the incursions of merchants, slavers, colonialists, and profiteers. Many Africans I know can speak eloquently on the ramifications of living in a postmodern reality and did so well before Western intellectuals gave the perspective a word. Finally, the absurd applies to the experience of human existence, something we all share. The term "absurd" was honed by philosophers and writers who had been affected by wars they themselves had lived through and whose primary focus was on the lived experience of self as it confronts violence and senselessness. An irony of violence, one that gives it an existentially absurd quality, is that it "exists" as an experiential negation of existence.

I concur with Hanna (1969:191) in his use of the term *absurdity*:

> In declaring my own understanding of the term "absurd," I want to insist that it not be taken as some exclusive philosophical concept which stands sovereignly aloof from certain obviously similar terms in the existentialists' vocabulary. With only slight qualifications in each case, I would be quite content to use Nietzsche's "pathos of distance," Sartre's "nausea," Camus' "revolt," Heidegger's "dread," and even the journalistically popular word "meaninglessness" as just as useful as the word "absurd." This extends equally to Kierkegaard's "despair."

5 Excellent comprehensive books on Mozambique include Casimiro, Loforte, and Pessoa 1990; Finnegan 1992; Geffray 1990; Hanlon 1984, 1991; Issacman and Issacman 1983; Jeichande 1990; Legum 1988; Magaia 1988, 1989; Ministerio da Saude/UNICEF 1988; Munslow 1983; UNICEF 1989, 1990; UNICEF/Ministry of Cooperation 1990; Urdang 1989; Vail and White 1980; Vines 1991; World Health Organization 1990.

6 To existential philosophers, angst of this realization provides the pivot where death, negation, and the slippages of reality can confront being and existence. This process, initiated only by individual choice, is viewed by the theorists as the font for creative change and redefinition – for the realization of being and self. In painful comparison, death, negation, and slippages of reality are not lurking possibilities on a cognitive horizon but brutally inescapable facts in the center of Mozambican life. They inhabit being and existence. Far from the self-actualizing function the philosophers impart to the reunion of being/negation, their unbridled penetrations are fundamentally destructive.

7 See Masolo 1983; Oruka 1983; Jackson 1989; p'Bitek 1983 for similar analyses of African epistemology.

8 I choose the word *dislocation* here as in Mozambique, displaced peoples are referred to as *deslocados* – or dislocated by the war and its effects.

9 I use the Portuguese word for healer here. This is intended to cover the range of healers available, including herbalists, diviners, trance performers, and spirit mediums. There are a dozen major languages in Mozambique, each with its own terms for healers, and, as I studied with people from many of these languages, I will use the national language of Portuguese rather than one of the African language groups.

10 For the early definitive works on the social construction of reality, see James

(1976, 1978); Schutz (1962, 1964); Berger and Luckman (1966).

## NOTES TO CHAPTER 18

1 At least sixty anthropologists have died of "fieldwork mishaps" in the past decade (Howell 1990), and at least three have been killed "on the job" as a result of political violence. In 1982, Ruth First, a South African-born anthropologist and professor, was killed by a mail bomb in her office at Maputo University in Mozambique. "It is suspected that the bomb was sent by the South African secret service to end her effective political protests against apartheid" (ibid., 100). In 1984, the Melanesian anthropologist Arnold Ap was tortured and killed by the Indonesian army in West Papua, allegedly because of his association with the Free Papua Movement (OPM). In 1990, Myrna Mack, a Guatemalan anthropologist, was brutally assassinated as she was leaving the research center where she worked in that country. She had been studying the effects of the civil war on indigenous peoples. In February 1993, a former Guatemalan soldier was sentenced to twenty-five years in prison for her murder.

2 It should also be noted that Nancy Howell's *Surviving Fieldwork* is the first comprehensive study of "the risks that are taken, and the prices that are paid for doing fieldwork in the ways we do" (1990:1). It is intended to help fieldworkers anticipate the dangers they will face and prepare for preventing and responding to them. She shows that anthropology can be dangerous and that hundreds of anthropologists have failed to protect themselves from dangers and have been victims of fieldwork. She devotes a chapter specifically to human hazards of fieldwork, which includes descriptions and discussion of incidents involving arrest, military attack, suspicion of spying, living through political turmoil, factional conflict, and the taking of anthropologists as hostages in the field.

3 The risks and dangers of participant observation-based research in Belfast are described in detail in my article on managing danger in fieldwork (Sluka 1990).

4 Seven months after completing this research, in August 1992, Jimmy Brown was shot dead in a feud within the IPLO. This feud ultimately led to the dissolution of the organization by the IRA in November of that year.

5 In 1989, three members of Ulster Resistance – a quasi-paramilitary loyalist group set up in 1985 – were charged in Paris with arms trafficking, receiving stolen goods, and conspiracy for the purpose of terrorism. They had been found in a hotel room with a mock-up of a Blowpipe shoulder-fired missile launcher, built by Protestant workers at the Shorts factory in East Belfast. With them were an American arms dealer with CIA links and a South African diplomat. Later, South Africa issued a statement rejecting allegations of links with Loyalist paramilitaries and denying they had supplied them with weapons. However, it is thought South Africans supplied Loyalists with their biggest-ever arms shipment in January 1988.

6 Conducting fieldwork in dangerous contexts raises very important ethical issues. I have struggled with these issues both personally and professionally for many years. I have chosen not to discuss the ethics of conducting research in dangerous contexts here because, in my opinion, that issue is more important than the issue of managing danger in fieldwork and therefore deserves a paper (or better yet, a book) devoted exclusively to it. Others may be of the opinion that it is inappropriate to discuss managing danger without discussing the larger issue of the ethics of conducting research in dangerous contexts. However, for the reason stated above, I have chosen to stick specifically to the topic of managing danger. The question, therefore, is, what is the relationship between ethics and managing danger?

7 The best contemporary example of drastic partisan anthropology is the case of the Dutch anthropologist Klaas de Jonge,

who was involved in smuggling weapons and explosives for guerrillas of the African National Congress. To avoid arrest, he sought asylum in a Dutch embassy office in Pretoria, where he spent two years before being allowed to leave South Africa as part of a prisoner exchange in September 1987.

## NOTES TO CHAPTER 20

1 By the time anthropologists mobilized effectively as a discipline to censure intelligence work, the US Defense Department had already successfully tapped anthropological expertise to refine counterinsurgency strategies in Indochina. The US military even started an "Ethnographic Study Series" and published a volume *Minority Groups in North Vietnam* which was "designed to be useful to military and other personnel who need a convenient compilation of basic facts about the social, economic, and political institutions and practices of minority groups in North Vietnam" (Kensington Office of the American Institutes for Research 1972). Incidentally, the chapter on the "Meo" [Hmong] in this volume specifically notes that the "Meo . . . make excellent guides" and "reliable porters, who can carry heavy loads (up to 50 kilograms) while maintaining a rapid gait" (Ibid.:239).

2 A church-based director of a human rights organization rebuked me when I explained to him the anthropological ethics which prevented me from showing members of US Congress photographs of peasant victims during my testimony on the military invasion: "For God's sake; what are you talking about! Testify as a human being then – not as an anthropologist."

## NOTES TO CHAPTER 21

1 The *Newsletter* of the American Anthropological Association (known at various times also as the *Fellow Newsletter* and *Anthropology Newsletter*), from its inception in 1960 until the present (and especially from 1967 when the "Correspondence" columns became a regular feature), and before that the *American Anthropologist*, are the best, and generally the only, sources for resolutions, votes, and debates. Some of these concerning the issues discussed in this essay have been brought together in Berreman 1973.

2 I can no longer refrain from noting that one of the two candidates who withdrew astounded me a couple of years later, when we were both on the Executive Board, by spontaneously announcing one evening, "I owe you an apology!" He went on to explain that this was because of his role in that maneuver when he acceded to an unexpected late-evening telephone request by the then president of the association to withdraw in favor of the strongest of the three candidates nominated by the Nominations Committee; he was asked, further, that he call the third candidate to persuade him to do likewise, in order to save the presidency from the challenge of "the radicals." He professed having felt guilty ever since.

3 The Executive Board at the time consisted, I believe, of George M. Foster, president (University of California, Berkeley), Charles Wagley, president-elect (Columbia University), Dell Hymes (University of Pennsylvania), David Schneider (University of Chicago), David Aberle (University of British Columbia), Eugene Hammel (University of California, Berkeley), Cyril Belshaw (University of British Columbia), and James Gibbs (Stanford University).

4 Wolf and Jorgensen's observation that in condemning them, "the Board evidently hoped to avert a threat to the internal harmony [and, I would add, the reputation] of the Association," is astute. The action was in this respect reminiscent of the council's action in 1920, hastily condemning and expelling Boas without concerning itself at all with the situation he reported. Both of these instances anticipated the Ad Hoc Committee to Evaluate the Controversy Concerning Anthropo-

logical Activities in Relation to Thailand, which concluded its deliberations in 1971 by simply denouncing those who sounded the alarm and thereby threatened the internal harmony and public reputation of the association. The tendency in all three was to close the wagons in a circle to blindly fend off the attackers, principle be damned.

5 " 'Free enterprise scholarship' . . . is scholarship which uses whatever resources are available by whoever has access to them, for immediate payoff without thought to consequences for others. This is a selfish, short-sighted and destructive posture" (my definition: Berreman 1971c: 398).

6 The Society for Applied Anthropology has made mention of secrecy-related issues in the 1963, 1973, and 1983 versions of its brief "Statement of Professional and Ethical Responsibilities," although only in 1973 was secrecy explicitly addressed: "2. We should not consent to employment in which our activities and/or scientific data remain permanently secret and inaccessible." That seems to have disappeared in the 1983 version, even as the dCoE of the AAA has jettisoned the issue as well (see: Society for Applied Anthropology 1963; 1973; 1983).

7 The quality of the prose in the draft code fell, relative to that in the PPR, as precipitously as its idealism. To confirm this, one need only compare the preambles to each of the two documents. Alternatively, one might read again the sentences quoted in the discussion above . . . or any pair of paragraphs at random. Better yet, read the documents in their entirety with this in mind. I would recommend that the draft code be dropped for the dreadful precedent it provides for dull, turgid anthropological prose, if for no other reason.

8 COPHEAR first met in October 1972, and it existed through June 1974. Its members were: Stephen A. Barnett, chair (Princeton University), Norman A. Chance (University of Connecticut), Shepard Forman (University of Michigan), Sally Falk Moore (University of Southern California), Robert J. Smith (Cornell University), and I, liaison member of the Executive Board (University of California, Berkeley) (AAA 1973:23, 62; 1974:72).

Other commitments prevented Sally Falk Moore from serving during the second year of the committee's existence.

9 These three most prominent organizations and their publications are: (1) Anthropology Resource Center (ARC; currently inactive, but publications available through Cultural Survival, Inc.); publications: *ARC Newsletter; The Global Reporter*; a series of occasional special reports and monographs. (2) Cultural Survival, Inc., 11 Divinity Ave., Cambridge, MA 02138; publications: *Cultural Survival Quarterly* (newsletter); occasional papers and special reports. (Also distributor for other publications including those of Anthropology Resource Center.) (3) Although not an American publication, this one is so central to its American audience that I include it here also: International Work Group for Indigenous Affairs (IWGIA), Fiolstraeda 10, DK-1171 Copenhagen K, Denmark; publications: *IWGIA Newsletter; IWGIA Yearbook; IWGIA Documents* (monograph series).

10 For a broader spectrum of points of view than those described here, consult the quarterly journal *Practicing Anthropology: A Career Oriented Publication of the Society for Applied Anthropology* (Box 24083, Oklahoma City, OK 73214). Now in its eleventh year and volume, it began publication in October 1978.

## NOTES TO CHAPTER 22

1 The African experience of western medicine has not always been problematic. The Chagga of Tanzania, for example, have long been aggressive consumers of European medicine, turning Moshi Hospital into one of the largest and most active in Tanzania, and becoming public

health officials in the western model. See Setel (1994).

2 I am grateful to my colleague, Dr Lee Dryden of the SUNY/Buffalo Department of Philosophy, for this interesting suggestion; naturally, he is not responsible for my failure to develop it thoroughly or insightfully.

3 For opposing views on the ethical obligation of physicians to treat AIDS patients, see, for example, Jonsen (1990) and Tegtmeier (1990).

4 In this regard, Janzen's (1978) study of "therapy management groups" among the BaKongo is an instructive illustration of the importance of the "popular sector" in the treatment of illness.

5 I also assume that the preferred form of medications is also related to body image in any culture. This hunch was reinforced in the field when Maronaua received a shipment of antibiotics donated by an Italian missionary organization. These Italian antibiotics, for adult use, were all in suppository form. When I described how one used these suppositories, the Kulina were at first amused by what they took to be my joke, and then became incredulous; they may still have a rather "inverted" conception of Italians.

6 I am unaware of any discussion of the ethics of selling medical services in such indigenous communities in the critical literature on the Summer Institute of Linguistics. However, a missiologist commenting on my paper on conversion (Pollock 1993) cited this approvingly among a group of strategies that were judged to be effective in converting indigenous groups.

## NOTES TO CHAPTER 24

1 This is obviously not true of the "new ethnicity" literature, of texts such as Anzaldúa (1987) and Radhakrishnan (1987).

2 See also Robertson (1988, 1991) on the politics of nostalgia and "native place-making" in Japan.

3 We are, of course, aware that a considerable amount of recent work in anthropology has centered on immigration. However, it seems to us that too much of this work remains at the level of describing and documenting patterns and trends of migration, often with a policy science focus. Such work is undoubtedly important, and often strategically effective in the formal political arena. Yet there remains the challenge of taking up the specifically *cultural* issues surrounding the mapping of otherness onto space, as we have suggested is necessary. One area where at least some anthropologists have taken such issues seriously is that of Mexican immigration to the United States (e.g., Alvarez 1987; Bustamente 1987; Chavez 1991; Kearney 1986, 1990; Kearney and Nagengast 1989; and Rouse 1991). Another example is Borneman (1986), which is noteworthy for showing the specific links between immigration law and homophobia, nationalism and sexuality, in the case of the Cuban "Marielito" immigrants to the United States.

## NOTES TO CHAPTER 26

1 'Being there' is, for one thing, the title of the first chapter in Clifford Geertz's (1988) study of anthropological writing, where another chapter is indeed devoted to Evans-Pritchard. It is also the title of another British anthropologist, C.W. Watson's (1999) collection of accounts of fieldwork, half a century after Evans-Pritchard's statement. Paul Willis reminds me, moreover, that it is the title of a Peter Sellers movie.

2 The project has had the support of the Bank of Sweden Tercentenary Foundation. Previous writings resulting from it include Hannerz (1998a, 1998b, 1999, 2001b, 2002). The project was discussed in the Lewis Henry Morgan Lectures at the University of Rochester in November 2000, and a book will result from these lectures (Hannerz, forthcoming). I will

also draw to a certain extent here on my discussion of multi-site ethnography in a more general handbook chapter on transnational research (Hannerz, 1998c).

3 As I soon learned, that was not self-evident – foreign correspondents have recently been inclined to think that international news reporting is under great pressure, perhaps particularly in the United States. As I write this, I come upon an item in what amounts to the gossip column of the *International Herald Tribune* (28 August 2002), according to which Dan Rather, CBS anchorman, tells *TV Guide* in an interview that less than a year after 11 September 2001, there is a new lack of emphasis on such reporting. 'The public has lost interest', Rather says. 'They'd much rather hear about the Robert Blake murder case or what is happening on Wall Street. A feeling is creeping back in that if you lead foreign, you die.'

4 Marcus (1995), in his discussion of this matter, has seen it in large part as a matter of choosing between, or making some combination among, six strategies: follow the people; follow the thing; follow the metaphor; follow the plot, story, or allegory; follow the life or biography; or follow the conflict.

## NOTE TO CHAPTER 27

1 The data on which this paper is based were collected from 1988 to 1989 at the European Space Agency's main research and technology institute (ESTEC), located in Noordwijk, the Netherlands. See Zabusky 1995 for a full treatment of these data.

## NOTES TO CHAPTER 28

1 Mead, Margaret, William Morrow, 1928, William Morrow, 1930, William Morrow, 1935, respectively.

2 Bateson, Gregory. Cambridge, England, Cambridge University Press, 1936.

## NOTES TO CHAPTER 29

1 Montaigne, [1580–8] 1943: 343.

2 The Songhay are a people of some 800,000 who live along the banks of the Niger River from as far north as Timbucktu, Mali, to as far south as Sansane-Hausa in the Republic of Niger. There are also some 2.5 million first-language Songhay speakers living in Mali, Niger, and northern Benin. These Songhay speakers, however, are members of other ethnic groups (Wogo, Kurtey, Zerma, Dendi) which have distinct social histories. Djebo's family is from Say, a town on the west bank of the Niger some 200 kilometers south of Tillaberi; it was the center of Fulan power in the nineteenth century.

3 Seneca, [63–5 ACE] 1962, Book 2: 281.

4 Kahn, 1980: 1271.

5 *Ibid.*: 1269.

6 Kant, [1790] 1966: 32.

7 Williams, 1976.

8 *Ibid.*: 264.

9 Montaigne, [1580–8] 1943: 320.

10 *Ibid.*: 345.

11 Ulmer, 1985: 52.

12 Derrida, 1974: 161.

13 Derrida, 1974: 109 as cited in Ulmer, 1985: 55.

14 Ulmer, 1985: 55.

15 Marcus and Cushman, 1982: 29.

16 Stoller 1984c: 102–3.

17 Marcus and Cushman, 1982: 31–6.

18 Some of the well-known contributions include Clifford, 1988; Crapanzano, 1980, 1985, 1987; Dumont, 1978; Dwyer, 1982; Marcus and Fischer, 1985; Rabinow, 1977; Stoller, 1984a, 1984b, 1986; Stoller and Olkes, 1987; Rose, 1987; Tyler, 1984, 1988.

19 Fabian, 1983: 164.

20 American Anthropological Association, 1984.

21 *Ibid.*: 2.

22 Jarvie, 1975.

23 Dryfus and Rabinow, 1982: 107.

24 Agee, 1941: 139–40.

25 Chernoff, 1979: 39.

26 Geertz, 1973: 347.

27 Lévi-Strauss, [1955] 1973: 362.
28 Merleau-Ponty, 1964: 159.

## NOTES TO CHAPTER 30

1 Some of the recent literature that I have found stimulating in this regard includes Tedlock 1979, Clifford 1983, Marcus and Cushman 1982, and the essays in Clifford and Marcus 1986. My thoughts about the value of reflexive follow-up accounts and the dialectics of cultural invention were stimulated during fieldwork in 1982 by immediately prior readings of Dumont 1978, Rabinow 1977, and Wagner 1981.
2 "Dialogue is the fashionable metaphor for modernist concerns. The metaphor can illegitimately be taken too literally or hypostatized into philosophical abstraction. It can, however, also refer to the practical efforts to present multiple voices within a text, and to encourage readings from diverse perspectives. This is the sense in which we use dialogue" (Marcus and Fischer 1986:680). This is also the sense in which the notion of dialogue is employed in the present article.
3 The missionary-linguist was Murray Rule of the Unevangelized Field Museum (today, Asia Pacific Christian Mission); Rule 1964 is a first description of the Kaluli language. During an interview in November 1984, Rule told me that of all the people in Papua New Guinea he had worked with in his years of translation, he was most impressed with the intelligence and quickness of his Kaluli linguistic informants. He attributed this to "a definite gift for language that the Kaluli tribe must have received from the time of Babel." Minus biblical rationale, Rule's perception is not unique among both long- and short-term visitors to Bosavi.
The Kaluli *are* energetically verbal; the cultural focus on language skill as a social resource is a significant feature of Kaluli everyday life, and this is readily manifest in the adaptation of new words, lexical expansion and coinage, and interest in other languages, not to mention the more typical arenas (metalanguage, poetics, conversation, registers and styles, socialization, etc.). This verbal "high profile" is described in B. B. Schieffelin 1979, Feld and Schieffelin 1982.
4 What is clear is that in the last ten years a number of literal back-translations have come into Kaluli everyday use, sometimes standing alongside a Kaluli equivalent, sometimes introducing a concept and coining a label. In 1984 Bambi Schieffelin and I came upon several of these back translation uses, for example, *tok pisin, stretim tok* → Kaluli *di-galema:no: to.* Here the term for "settle a complaint" or "solve a discussion," literally "straighten (-ed, -ing) talk" is back-translated by the direct Kaluli terms for "straighten" and "talk."
5 Kaluli also use this "show" term as a metalinguistic label to indicate what mothers do when teaching language to their children (B. B. Schieffelin 1979:105–6).
6 While the Kaluli generally interpret the Bible as an elaborate compendium of Christian "turned over words," my readers volunteered that missionary translation work is different from ours and is not "turned around" and "turned over" Kaluli.
7 *Voices in the Forest* was funded by the National Endowment for the Arts and the Satellite Program Development Fund of National Public Radio. The tape was coproduced with Scott Sinkler, who was also the chief studio engineer. We thank the above agencies and Magnetik Recording Studios, Philadelphia, for their assistance. *Voices in the Forest* was aired in 1984 and 1985 on NPR, Pacifica, and independent stations in the USA, Europe, and the Pacific.
8 Stereo recordings were done with omnidirectional AKG condenser microphones in an X–Y configuration; monaural recordings, mostly of bird sounds, were made using a Gibson parabolic reflector. All were originally recorded on a Nagra IV-S at 7-1/2 or 15 ips. Sound pressure

level readings in dB A, B, and C were taken at the time of each recording to insure proper volume level continuity throughout the studio rerecording and mix.

## NOTES TO CHAPTER 31

*Author's note*: In crafting this chapter, the author has drawn extensively on the work of Constance Classen, and especially on her article "Foundations for an Anthropology of the Senses" (*International Social Science Journal* 153 [1997]: 401–12), which was originally written for the UNESCO project, directed by the author, that culminated in the book from which the chapter is taken.

1 Within sociology Anthony Synnott, among others, has been concerned with examining the sensory codes of the contemporary West, from the symbolism of perfumes to the tactile intricacies of child-care (Synnott 1993; Classen, Howes, and Synnott 1994). A sensuous geography has been elaborated by Yi-Fu Tuan (1995) and Paul Rodaway (1994). Historians such as Alain Corbin and Roy Porter have delved into the cultural shifts in sensory values which have taken place at different periods of Western history (Corbin 1986; Porter 1993). These parallel investigations help to supplement and inform the anthropology of the senses, placing it within a multi-disciplinary movement to explore the life of the senses in society.

## NOTES TO CHAPTER 32

1 *Nicomachean Ethics*, Book VIII, Chapter 2, p. 1060 in *The Basic Works of Aristotle*, edited by Richard McKeon (Random House, New York, 1941).
2 *Nicomachean Ethics*, Book VIII, Chapter 13, p. 1075 in McKeon.
3 Frederic Jameson, *The Prison House of Language* (Princeton University Press, Princeton, 1972), p. 13.

## NOTES TO CHAPTER 33

1 With respect to the "Orient" – and this would include "Morocco" – these categories or typifications have, as Edward Said (1978) has argued, a "discursive consistency" that is embedded in a distinct European and American ideology. The "Oriental," the "Moroccan," "Tuhami" as a type, are all representations in an inevitably distorting language. They are – and here I part company with Said – subject to some partial reformulation in the negotiations that transpire in "lived encounters" with the Oriental, with the Moroccan, with Tuhami. These reformulations are still deformations, to use the jargon: but they do raise the problematic of traditional representations and even give the mutually satisfying illusion of a corrective to these representations. What is sadly lacking in Said's work (and is its pathetic irony) is an encounter with the Oriental in person or text. (There is, after all, a long scholarly tradition in the "Orient" that has profoundly affected the Orientalist's perception and scholarship.) Said's critical and even self-critical stance serves only to relocate him, inevitably, he would argue, in the Orientalist discourse. He, his text, is frozen in the "chosen" moment of an arrested dialectic.
2 Or, perhaps more accurately, "fore-understanding" (Gadamer 1960).
3 My wife had met Tuhami on several occasions, once when Tuhami took us on a tour of the sanctuaries in and around Meknes and again when we came upon him on his way to the shrines of Sidi 'Ali and Moulay Idriss. She did not come to our meetings, but the fact of her existence must have influenced Tuhami's relationship to me. At the time, we were in our late twenties and still childless.
4 I should point out that in Paris, several weeks before my arrival in Morocco. I dreamed anxiously that I was trapped in a saint's tomb – it was white and damp like clay – and that I was rescued by a woman's brown hand that pulled me

through a slit-like window. When I first came to Meknes, several Hamadsha, who were still suspicious of me, asked if I had ever dreamed about Morocco. I told them the dream, and they said that it meant that 'A'isha Qandisha had sent for me. One of them a *muqaddim*, began to call me "Tahush" – the name of an important *jinn*, he explained, laughing. My relations with the Hamadsha improved immensely thereafter. Tuhami was not among the Hamadsha and probably never heard of my dream or of my *jinn*-name.

5 I have spoken here as though only the change in *my* attitude was responsible for the change in our relationship. I have thus preempted the initiative, have declared Tuhami passive and myself active and free of influence, and have falsified the dynamics of our relationship. Even the most directed relationships involve a negotiation of reality by both parties.

6 I am not alone among anthropologists and the readers of anthropology in equating the informant with the informer. The confusion results from a guilt-inspiring voyeuristic intention that can be rationalized away no more in the anthropological endeavor (by science) than in the psychoanalytic endeavor (by cure).

7 The reflective attitude is always alienating. It demands an interlocutor, a stranger of sorts, who questions the taken-for-granted. The "endopsychic" stranger is of course constrained by the same idiom that articulates the unreflected world. The "real" stranger, inevitably understood through that idiom, is nevertheless occasionally able, through insistence, perspectival difference, projection, and misunderstanding, to break through the idiomatic constraints that fashion the taken-for-granted world and the (conventional) reflections upon it.

8 I am fully aware of the alienating quality of this effacement both in terms of the inner dynamics of the triad, Tuhami, Lhacen, and me, and in terms of the work I set Lhacen. It is a given in the triadic relationship and has its implications for all three members, both alone and in various alliances. Lhacen as a *tertius gaudens*, to use Simmel's (1964b) expression, was not, however, without benefit from the arrangement.

9 Strictly speaking, all my statements here about Tuhami's (or, for that matter, Lhacen's) subjective experience, either in itself or as a component of a we-relationship, must be understood hypothetically, however compelling my basis for inference. The experience of the we-subject, Sartre (1956) notes, "in no way implies a similar and correlative experience in others." The experience of the "we" remains a simple symbol of the longed-for unity of transcendence – a kind of psychological mask for the original conflicts of transcendence, at least in Sartre's Hegelian vision of the relations between consciousnesses.

10 See Parin et al. (1971) for examples of the importance of departure in their psychoanalytic interviews with the Anyi.

11 Did my "therapeutic" interest in Tuhami's marriage reflect my own anxiety over departure – over abandoning Tuhami? Was I seeking to get myself off the hook by providing him with the possibility of a substitute for me?

12 I do not wish to suggest here that Lhacen became the "father" for Tuhami and me. Rather, Lhacen represented one of the functions of the "father": the controller of the word. He was not, however, the source of the word. The resolution of oedipal conflicts can be understood in terms of negotiating and stabilizing the triadic relationship between father, mother, and son. Father and son compete, so to speak, for the word; and, in the classical resolution of the conflict, the father wins out, and the son (and mother) surrenders to him as both source and controller of the word. In the triad formed by Lhacen, Tuhami, and me the functions are split. Lhacen represents the more abstract function of control. I am the initiator of the word that Tuhami gives. In the text I produce here, through my re-presentation of what transpired, I assume all three functions.

## NOTES TO CHAPTER 34

1 *Bocage*: countryside of Western France marked by intermingling patches of woodland and heath, small fields, tall hedgerows and orchards.

2 *Unwitcher*: The Bocage natives use the word *désorcelleur* rather than the more usual *désensorcelleur* [ensorceller = to bewitch]. I have translated it by *unwitcher* rather than *unbewitcher*. Similarly, *désorceller* is translated as *to unwitch* and *désorcellage* or *désorcellement* as *unwitching* or *unwitchment*.

3 He only recounts one incident (p. 460) in which the Zande were able to say he was bewitched.

4 It is not surprising that Clausewitz (1968) was an important point of reference at the beginning of my work: war as a supremely serious game, trying to dictate its laws to the enemy; as an extension of a duel on a wider scale and over a longer span; as a continuation of politics through other means, and so on. It was not always easy to decide which one was speaking: the discourse of war or the discourse of witchcraft, at least until I realized that it was meaningless to think of witchcraft in terms of the categories of game theory.

5 Anyone who called himself bewitched on his own authority would simply be thought mad: a warning to apprentice sorcerers who try to make peasants talk by simply declaring themselves '*caught*'.

6 A dodge which an Azande would never have imagined, since he can only choose between *witchcraft* and *sorcery*.

7 To consult without asking the seer to 'see' is futile, since the latter sees nothing and there is nothing for the ethnographer to understand.

8 I might just as well express it as 'can't be thought' or 'can't be said'; in talking of what 'can't be coped with', I am trying to point to an element of reality, that at some point escapes the grasp of language or symbolization.

## NOTES TO CHAPTER 35

1 Only English, American, and French examples are discussed. If it is likely that the modes of authority analyzed here are able to be generalized widely, no attempt has been made to extend them to other national traditions. It is assumed also, in the antipositivist tradition of Wilhelm Dilthey, that ethnography is a process of interpretation, not of explanation. Modes of authority based on natural-scientific epistemologies are not discussed. In its focus on participant observation as an intersubjective process at the heart of twentieth-century ethnography, this discussion scants a number of contributing sources of authority: for example the weight of accumulated "archival" knowledge about particular groups, of a cross-cultural comparative perspective, and of statistical survey work.

2 "Heteroglossia" assumes that "languages do not *exclude* each other, but rather intersect with each other in many different ways (the Ukrainian language, the language of the epic poem, of early Symbolism, of the student, of a particular generation of children, of the run-of-the-mill intellectual, of the Nietzschean, and so on). It might even seem that the very word 'language' loses all meaning in this process – for apparently there is no single plane on which all these 'languages' might be juxtaposed to one another" (291). What is said of languages applies equally to "cultures" and "subcultures." See also Volosinov (Bakhtin?) 1953:291, esp. chaps. 1–3; and Todorov 1981: 88–93.

3 I have not attempted to survey new styles of ethnographic writing that may be originating outside the West. As Edward Said, Paulin Hountondji, and others have shown, a considerable work of ideological "clearing," oppositional critical work, remains; and it is to this that non-Western intellectuals have been devoting a great part of their energies. My discussion remains inside, but at the experimental boundaries of, a realist cultural science elaborated in the Occident. Moreover, it

does not consider as areas of innovation the "para-ethnographic" genres of oral history, the nonfiction novel, the "new journalism," travel literature, and the documentary film.

4 In the present crisis of authority, ethnography has emerged as a subject of historical scrutiny. For new critical approaches see Hartog 1971; Asad 1973; Burridge 1973:chap. 1; Duchet 1971; Boon 1982; De Certeau 1980; Said 1978; Stocking 1983; and Rupp-Eisenreich 1984.

5 On the suppression of dialogue in Lafitau's frontispiece and the constitution of a textualized, ahistorical, and visually oriented "anthropology" see Michel de Certeau's detailed analysis (1980).

6 Favret-Saada's book is translated as *Deadly Words* (1981); see esp. chap. 2. Her experience has been rewritten at another fictional level in Favret-Saada and Contreras 1981.

7 It would be wrong to gloss over the differences between Dwyer's and Crapanzano's theoretical positions. Dwyer, following Georg Lukács, translates dialogic into Marxian-Hegelian dialectic, thus holding out the possibility of a restoration of the human subject, a kind of completion in and through the other. Crapanzano refuses any anchor in an englobing theory, his only authority being that of the dialogue's writer, an authority undermined by an inconclusive narrative of encounter, rupture, and confusion. (It is worth noting that dialogic, as used by Bakhtin, is not reducible to dialectic.) For an early advocacy of dialogical anthropology see also Tedlock 1979.

8 On realist "types" see Lukács 1964, passim. The tendency to transform an individual into a cultural enunciator may be observed in Marcel Griaule's *Dieu d'eau* (1948). It occurs ambivalently in Shostak's *Nisa* (1981). For a discussion of this ambivalence and of the book's resulting discursive complexity see Clifford 1986:103–9.

9 For a study of this mode of textual production see Clifford 1980. See also in this context Fontana 1975, the introduction to Frank Russell, *The Pima Indians*, on the book's hidden coauthor, the Papago Indian José Lewis; Leiris 1948 discusses collaboration as coauthorship, as does Lewis 1973. For a forward-looking defense of Boas' emphasis on vernacular texts and his collaboration with Hunt see Goldman 1980.

10 James Fernandez' elaborate *Bwiti* (1985) is a self-conscious transgression of the tight, monographic form, returning to Malinowskian scale and reviving ethnography's "archival" functions.

11 Such a project is announced by Evans-Pritchard in his introduction to *Man and Woman among the Azande* (1974), a late work that may be seen as a reaction against the closed, analytical nature of his own earlier ethnographies. His acknowledged inspiration is Malinowski. (The notion of a book entirely composed of quotations is a modernist dream associated with Walter Benjamin.)

12 For a "group dynamics" approach to ethnography see Yannopoulos and Martin 1978. For an ethnography explicitly based on native "seminars" see Jones and Konner 1976.

13 For a very useful and complete survey of recent experimental ethnographies see Marcus and Cushman 1982; see also Webster 1982; Fahim 1982; and Clifford and Marcus 1986.

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### CHRONOLOGICAL LIST OF KULA EVENTS WITNESSED BY THE WRITER

FIRST EXPEDITION, August, 1914–March, 1915.

*March*, 1915. In the village of Dikoyas (Woodlark Island) a few ceremonial offerings seen. Preliminary information obtained.

SECOND EXPEDITION, May, 1915–May, 1916.

*June*, 1915. A Kabigidoya visit arrives from Vakuta to Kiriwina. Its anchoring at Kavataria witnessed and the men seen at Omarakana, where information collected.

*July*, 1915. Several parties from Kitava land on the beach of Kaulukuba. The men examined in Omarakana. Much information collected in that period.

*September*, 1915. Unsuccessful attempt to sail to Kitava with To'uluwa, the chief of Omarakana.

*October–November*, 1915. Departure noticed of three expeditions from Kiriwina to Kitava. Each time To'uluwa brings home a haul of *mwali* (armshells).

*November*, 1915–*March*, 1916. Preparations for a big overseas expedition from Kiriwina to the Marshall Bennett Islands. Construction of a canoe; renovating of another; sail making in Omarakana; launching; *tasasoria* on the beach of Kaulukuba. At the same time, information is being obtained about these and the associated subjects. Some magical texts of canoe building and Kula magic obtained.

THIRD EXPEDITION, October, 1917–October, 1918.

*November*, 1917–*December*, 1917. Inland Kula; some data obtained in Tukwaukwa.

*December–February*, 1918. Parties from Kitava arrive in Wawela. Collection of information about the *yoyova*. Magic and spells of Kaygau obtained.

*March*, 1918. Preparations in Sanaroa; preparations in the Amphletts; the Dobuan fleet arrives in the Amphletts. The ***uvalaku*** expedition from Dobu followed to Boyowa.

*April*, 1918. Their arrival; their reception in Sinaketa; the Kula transactions; the big intertribal gathering. Some magical formulæ obtained.

*May*, 1918. Party from Kitava seen in Vakuta.

*June*, *July*, 1918. Information about Kula magic and customs checked and amplified in Omarakana, especially with regard to its Eastern branches.

*August*, *September*, 1918. Magical texts obtained in Sinaketa.

*October*, 1918. Information obtained from a number of natives in Dobu and Southern Massim district (examined in Samarai).

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