

Chapter 1

A Dispute in Donggo: Fieldwork and Ethnography

As has often been said, if you want to understand what anthropology is, look at what anthropologists do. Above all else, what anthropologists do is ethnography. Ethnography is to the cultural or social anthropologist what lab research is to the biologist, what archival research is to the historian, or what survey research is to the sociologist. Often called - not altogether accurately - 'participant observation', ethnography is based on the apparently simple idea that in order to understand what people are up to, it is best to observe them by interacting with them intimately and over an extended period. That is why anthropologists have tended traditionally to spend long periods - sometimes years at a stretch - living in the communities they study, sharing the lives of the people to as great an extent as they can. It is this approach that has defined our discipline and distinguished it from other social sciences. Now, we certainly do not dismiss the methods more characteristic of other disciplines, such as the use of questionnaires or the collection of quantitative behavioural data. But anthropologists have long felt that approaching the study of human beings in those ways is likely to produce an incomplete - even misleading - understanding of the people studied, especially when those people are members of foreign or unfamiliar societies.

It might also be said that fieldwork is what gives the enterprise of anthropology a good deal of its romance. It was certainly one of the

things that attracted the two of us to the discipline. Today anthropologists conduct fieldwork in settings that are as unexotic as television stations in city centres, magistrate's courts in small towns. corporate boardrooms, and church congregations in middle-class suburbs. But in its infancy as a profession, anthropology was distinguished by its concentration on so-called 'primitive' societies: relatively small, non-Western communities in which social institutions appeared to be fairly limited and simple (not so, as it turned out!) and social interaction was conducted almost entirely face-to-face. Such societies, it was felt, provided anthropologists with a simplified view of the 'elementary' workings of society, one that contrasted with the complexities of 'modern' (that is, Western) society. There was also a sense among anthropologists that the ways of life represented by these smaller societies were rapidly disappearing, and since many of them had no writing, it was an urgent task to preserve a record for posterity. This orientation of the discipline, and an early commitment to the first-hand collection of ethnographic data by means of participant observation, led anthropologists to some of the most remote and exotic places on earth. Most often working alone and in isolation from other Westerners, the ethnographer cut a bold figure indeed. Often this isolation created a sense of alienation and loneliness, especially in the early stages of fieldwork. But almost all anthropologists find themselves assimilating to the culture of their host communities to a greater or lesser degree - a few, it is said, even to the point of 'going native', completely adopting the lifestyle of their hosts and never returning home. All together, the process of immersing oneself in fieldwork can be a challenging and unique experience, one that continues to attract men and women to anthropology. It also so happens that participant observation seems to be the most effective way of understanding in depth the ways in which other people see the world and interact with it, and often provides a check on our own preconceptions and beliefs.

Let us begin with a story, a story that shows you not only how anthropologists work, but what is distinctive about anthropology as a



ama Tife. Ama Tife was the elder who assured ina Mone's kin that they
ld receive justice from a panel of village elders. In addition to his
nts as a judge, ama Tife was a skilled healer and ritual specialist.

ipline. This is a story about Peter's fieldwork with the Dou Donggo
how he came to be interested in the anthropology of law.

One night I was sitting in the house of a friend in Doro Ntika, the village
where I was conducting fieldwork. One of my friend's relatives burst into
the room, shouting that his sister-in-law, a woman named ina Mone, had
been assaulted by a young man, la Ninde. We rushed over to ina Mone's
house to see what had happened. Ina Mone sat on the floor of the room,
one side of her face painted with a medicinal paste, where she said la
Ninde had struck her. She also showed us the shirt she had been wearing
and that had been torn in the assault. Her male relatives were angry, and
talked of 'taking down the spears and sharpening the bushknives',
anxious to exact an immediate revenge on la Ninde. But everyone
became calmer when ama Tife, one of the principal elders of the village,
came by to assure us that he and the other elders of Doro Ntika would
convene a court and exact justice according to tradition. The next

morning they did just that. La Ninde was brought before a group of elders with most of the village looking on. Ina Mone showed her medicated face and torn shirt as evidence. La Ninde admitted to having shouted at her, but denied having laid hands upon her. A spirited and tumultuous drama ensued, as members of the court, led by ama Panci, berated la Ninde and finally extracted a confession. He was assessed a minor fine and was made to kneel before ina Mone begging forgiveness. She gave him a symbolic slap on the head, and he was let go.

Later that afternoon. I chatted with a friend. I said, 'Wasn't that terrible, what la Ninde did, assaulting ina Mone like that?' He answered, 'Yes, it was. But you know he never really hit her.' I was surprised. 'What about the torn shirt and her face?', I asked. 'Well', he said. 'anyone can tear a shirt, and who knows what's under the medicine.' I was deeply shocked. 'But that means la Ninde is innocent. Isn't this terribly unfair?' 'Not at all', he replied. 'What la Ninde was convicted of was more true than what really happened.' He then proceeded to fill me in on what everyone else in the village knew, indeed, what they had known all along. Ina Mone had seen la Ninde hanging around la Fia, a young woman who was betrothed to another young man, absent from Doro Ntika. Ina Mone had complained to la Ninde's mother, who in turn had admonished la Ninde. Furious at having been ratted out, la Ninde had gone to ina Mone's house and threatened her - a serious breach of etiquette - but had not in fact assaulted her.

This story is an account of a real event in the real world, as witnessed by an ethnographer. How would this event have been recorded and analysed by a historian or a sociologist? To begin with, to a historian who works primarily with archives or court records, the case of la Ninde's assault on ina Mone would be completely invisible. The Dou Donggo do not keep written records of disputes settled by village elders, so this case and the great majority of cases would not appear in a form accessible to the historian working in an archive. Even a historian who adopts the ethnographic methods of an anthropologist and takes

down oral histories might have difficulty in accessing this case, for among the Dou Donggo it is an accepted practice that one never discusses a dispute after it has been settled. Only because he was on the scene at the time the dispute erupted, was Peter able to record it and explore its meaning.

How would this case have appeared to a sociologist or a criminologist? Although some sociologists and criminologists are adept at using ethnographic methods, it is far more common for them to rely on surveys, questionnaires, and the analysis of official statistics. Again, to those relying on official statistics, this case would have been completely invisible. La Ninde's 'assault' might have appeared as a 'data point' in a survey of disputes in the community undertaken by a sociologist. But it seems unlikely that a survey would be so artfully constructed as to see beyond the superficial evidence of the case, or, more importantly, to uncover the notion that la Ninde's conviction of a crime he did not commit was 'more true than what really happened'. If the case *had* been recorded officially, researchers (including anthropologists) who rely on such data would probably assume the case of la Ninde was one of simple assault, leading to conclusions about Dou Donggo society that would be seriously incomplete, if not misleading.

Very well, then, what might the case described mean to an ethnographer? How might an anthropologist analyse this event to learn more about what the Dou Donggo believe and how they behave? First, after considerable questioning, it became clear to Peter that the case had little to do with assault and a very great deal to do with respect for the institution of marriage. Why had ina Mone complained to la Ninde's mother about his flirtations with la Fia? Because ina Mone had a real and vested interest in protecting the integrity of betrothals, particularly betrothals contracted by the family of ama Panci. Why? Because ina Mone's daughter was betrothed to ama Panci's son and another of ama Panci's sons was betrothed to la Fia!



4. A Dou Donggo Judge. This is ama Balo, another prominent Dou Donggo elder, as he is engaged in settling a dispute. Dou Donggo dispute settlement, like law in many small-scale societies, stresses consensus and the restoration of ruptured social relationships rather than a winner-take-all decision of guilt or innocence.

One lesson, then, that Peter learned was that in disputes (at least among the Dou Doriggo) things are often other than what they appear to be. A case of 'assault' may really be a case about 'alienation of affection'. What made this sort of realization possible? First of all, Peter was there to witness the event to begin with, something that would not have been possible had he not spent almost two years in this village. The ability to observe unusual, unique events is one of the principal advantages of the ethnographic method. It is important to recognize, as well, that Peter was able to observe the case in question from the outset not only because he lived in Doro Ntika for a long time, but because he lived there around the clock and as a member of the community. The case came to his attention not because he was seeking out information on disputes or even on betrothals, but because he just happened to be chatting with friends in a nearby house, long after a conventional 'working day' was over. It is this openness to the serendipitous discovery that gives the ethnographic method strength and flexibility not generally available to highly deductive social science methods, such as survey or statistical research. Indeed, anthropologists often find themselves doing significant research on unanticipated subjects. While there are those research topics we take with us to the field, there are also topics imposed upon us by the actual circumstances and events of people's everyday lives. Peter had not intended to study dispute settlement when he set off for Indonesia, but neither could he ignore the research opportunity he encountered that evening. The randomness of ethnographic serendipity is compensated for by the length of time a good ethnographer spends in the field; eventually, one hopes, one will accidentally encounter most social phenomena of significance.

Prolonged exposure to daily life in Doro Ntika also made Peter aware that it was necessary to look beyond the superficial events of the case, made him aware that issues like the fidelity of fiances was a sensitive, even explosive, topic in this community. In other words, after more than a year living in this community, Peter had a rich and nuanced context

into which the events of this case could be placed. The discrepancy between what a social event is apparently about and what it might 'really' be about is almost impossible to discern without the experiential context ethnographic fieldwork makes available. That is one of the advantages that anthropologists have traditionally relied upon for the insights they derive from their research and it is why traditional ethnographic fieldwork has placed a premium on long duration – often as much as two years for an initial study. Moreover, Peter was able to discover what the case was 'really about' because his long residence in the village had allowed him to build up relations of trust with people who were willing to confide in him and to explain events and motivations beyond superficial appearances. Having long-term cordial relations with people in the village – having friends, if you like – also enabled Peter to persist in his questions beyond the superficial and to evaluate the content of the answers he received.

What implications might an anthropologist see in the lessons of this case? Every ethnographic description at least implicitly participates in the cross-cultural comparisons that also engage anthropologists. Anthropology has long been engaged in relating the description of local beliefs and practices to categories of universal, pan-human significance. The case of la Ninde compelled Peter to bring into question his understanding of legal categories like 'evidence' and 'liability', to question the universality of the idea of 'justice' itself. What does it mean that virtually everyone in the village knew the physical evidence presented by ina Mone was false, yet was nonetheless accepted? What might it mean for our understanding of liability and responsibility if la Ninde could be convicted for what he *might have done*, rather than for what he actually did, without producing a sense among the villagers that he was a victim of trumped-up or injustice? If evidence and liability could be handled in this way, what does that mean if we are to try to construct a sense of what justice means to human beings at large? It is this interplay between the specific and the general, between the local and the universal that gives anthropology much of its value as a social

science. For not only are we engaged in recording the 'customs and manners*' of people around the world, we are constantly bringing our appreciation of local knowledge to bear on a more general understanding of what it means to be a human being. We will have more to say about this process in the next chapter.

Fieldwork: Strategies and Practices

It should be obvious that a truly comprehensive description of any society's culture is far beyond the capacity of even a hundred researchers. An ethnographer goes to the field with the intention of studying some particular aspect of social life, which might range from ecological adaptation to indigenous theology, to relations between the genders, to grassroots political mobilization, and so on (see text box). The ethnographer does not enter into the enterprise unprepared. What ethnographers need to know is as diverse and varied as the studies they undertake. Most anthropologists begin their preparation with several years of study in the history and previous ethnographic literature of the region in which they propose to do fieldwork. Because anthropologists have felt it imperative that they conduct their fieldwork in the language of the people they study without using translators, an ethnographer may need to acquire at least passable fluency in several languages. In addition to such general preparation, ethnographers are usually trained in more specialized fields concerning the kind of problem they intend to investigate. A researcher who intends to study the medicinal use of plants among an Amazonian people, for example, needs to learn not only a good deal of conventional botany, but also needs to be familiar with how various of the world's peoples have categorized and used plants. Anthropologists are always anthropologists of something and somewhere: John is an anthropologist of religion and a Mesoamericanist; Peter is an anthropologist of law and a Southeast Asianist.

An ethnographer's first task is to become established in the community.

Half a Dozen Ethnographies

To give an idea of the range of people and places anthropologists study, here, in no particular order, are the titles of half a dozen ethnographies, each with a very brief description.

We Eat the Mines and the Mines Eat Us June Nash's description of Bolivian tin miners and the ways in which transnational economic processes affect their lives (1979).

Beamtimes and Lifetimes Sharon Traweek's account of the world of high-energy physicists (1988).

Tuhami: Portrait of a Moroccan Vincent Crapanzano's ethnographic biography describes his encounter with 'an illiterate Moroccan tilemaker who believes himself married to a camel-footed she-demon' (1980).

In the Realm of the Diamond Queen Anna Tsing's study of political and cultural marginality, linking a Borneo people to the Indonesian nation and the global politics of 'modernization' (1993).

The Channeling Zone: American Spirituality in an Anxious Age Michael F. Brown presents a fascinating look at the lives and experiences of New Age 'channellers' and their place in contemporary American spiritual life (1997).

Medusa's Hair Gananath Obeyesekere brings insights from psychoanalysis to bear on 'personal symbols and religious experience' among ecstatic priests and priestesses in Sri Lanka (1981).

This is often a protracted and difficult process, during which more than a few projects have foundered. Once the ethnographer has found a source of funding for the project, it is often necessary to secure a variety of permits from various levels of government, local research institutions, and the host community. This can consume more than a year of the ethnographer's time, before he or she even sets foot in the field site. One colleague carrying out a research project at the

headquarters of a major industrial concern needed to have his proposal reviewed by the company's lawyers before he could even enter the building to talk with anyone. Once they have arrived, ethnographers face many of the same problems anyone would encounter when moving into a new community, problems complicated by unfamiliarity with the language and the challenges of daily life in places lacking many of the amenities they may have been used to at home: electricity, indoor plumbing, or easy access to healthcare, news, or entertainment. Many anthropologists work in cities and suburbs in Europe and North America, where the challenges are of a different nature. Neither of us would be eager to trade places with colleagues we know who have worked with drug addicts in Spanish Harlem or the top executives of a multinational corporation in Philadelphia.

The ethnographer faces more subtle difficulties, too. Locally powerful individuals may try to use the ethnographer as a prize or a pawn in their rivalries. Members of the community may have an exaggerated idea of what the ethnographer can do for them, and make persistent demands that cannot be met. At the same time, the ethnographer often experiences the great joy of making new friends and the thrill of seeing and doing things he or she would never otherwise have been able to see or do. As a day-to-day experience, fieldwork can be filled with abruptly alternating emotional highs and lows. At its heart the process of doing ethnography really is participant observation. By living among the people of the community as they themselves live, the ethnographer stands the best chance of becoming established.

Dialogue is the backbone of ethnography. While anthropologists make use of a variety of techniques to elicit and record data, the interview is by far the most important. Interviews can range in formality from highly structured question-and-answer sessions with indigenous specialists, to the recording of life histories, to informal conversations, or to a chance exchange during an unanticipated encounter. Ultimately, the key to ethnographic success is being there, available to observe, available to

Half a Dozen More Ethnographies

After Nature Marilyn Strathern, whose work on gender and exchange in Papua New Guinea is much admired, turns to English kinship in the twentieth century (1992).

Corn Is Our Blood Alan Sandstrom's examination of Nahua (Aztec) theology and its relation to ethnic identity (1991).

The Golden Yoke Rebecca French, who is both a lawyer and an anthropologist, gives a masterful account of Buddhist cosmology and its relationship to the traditional legal system of Tibet (1995).

Geisha Liza Dalby trained as a geisha in Kyoto and provides a fascinating look at the 'willow world' (1983).

Persuasions of the Witch's Craft Tanya Lührman delves into the world of witches and magicians in contemporary Britain exploring the implications of their beliefs in the context of modern society (1989).

Javanese Shadow Plays, Javanese Selves Ward Keeler lived with a Javanese puppeteer for several years and wrote this fascinating account of an ancient art form, its practitioners, and its place in modern culture (1987).

follow up, available to take advantage of the chance event. Beyond the apparently simple techniques of interview and dialogue, ethnographers also employ a variety of more specialized techniques. Audio recording of speech and music, photography, film, drawing, genealogy, mapping, census-taking, archival research, collecting material culture, collecting botanical or other natural samples, all have their ethnographic uses, depending on the ethnographer's specific research project.

Leaving the field can be almost as difficult as entering it: considering the effort required to establish oneself in a community, parting company with friends and now-familiar ways of life can be a wrenching

experience. On an intellectual level, there are often nagging worries about whether one has really completed the research topic – a concern that is often justified. In a sense, no ethnographic research project is ever truly complete; it is always possible to learn more, to expand the temporal or spatial scope of one's understanding, or deepen the subtlety of that understanding. Epistemological misgivings, such as those discussed more fully in the next section, often bother the departing ethnographer. Nonetheless, a kind of closure is sometimes possible. Peter recalls relating his analysis of a particularly complex dispute to a friend who was one of the principal elders of Doro Ntika. His friend laughed, and slapping this thigh said, 'You really *do* understand the way things work around here! Looks like you weren't wasting the past two years after all!'

Critiques of Ethnographic Fieldwork

For all its virtues, we would not want to give the impression that ethnographic fieldwork is the best method for all kinds of social science research, nor that participant observation is the only method employed by anthropologists. Fieldwork brings with it a substantial set of methodological and epistemological problems. Fieldwork also carries with it a unique set of ethical dilemmas.

The very strengths of classical ethnographic research have sometimes also proved to be weaknesses. One problem with participant observation has been a temptation for the ethnographer to present the community in a kind of temporal and spatial isolation. Many ethnographers, particularly in the 'classic' accounts of the 1930s and 1940s, employed what came to be called the *ethnographic present* in which communities were presented as frozen in time, outside any historical context, and without reference to neighbouring societies or encapsulating states. For example, one of the most admired classics, Raymond Firth's *We the Tikopia*, described the social organization and traditional religion of the Tikopia without reference to the fact that half

the population had recently converted to Christianity. Indeed, anthropologists may sometimes be carried away by the romance of their own enterprise and value the 'unspoiled' traditions of a society far more than the people themselves do. A friend of ours visited Tikopia some twenty years after Firth had lived there, and was taken to a grotto by the sea where offerings to the gods of the old religion had been made. Seeing a single old offering, he asked his guide who had left it there, and was told 'Fossi left it there'. 'Fossi', of course, is the Tikopia pronunciation of Firth's name. Ethnographers are not always successful in guarding against a temptation to romanticize the 'otherness' of the people they study. Another criticism of the 'ethnographic present' has concerned the tendency of ethnographers to write in an omniscient third-person voice, as if they had not been actively involved in eliciting the information they present. For better or worse, the past ten years has seen the emergence of a genre of ethnography that seems as intent on conveying the ethnographer's personal experiences in collecting the data as in presenting the data themselves.

Participant observation - characterized by long-term intense interaction with relatively small groups of people - may allow the ethnographer to dig deeply into the complexities and subtleties of a community's social life. But how representative of larger social and cultural wholes can this be? Based on participant observation alone, it would be impossible for Peter to say to what extent the beliefs and values uncovered in the case of Ia Ninde are typical of the Dou Donggo in general, or of the regency of Bima, or of Indonesia, or of Southeast Asia. In approaching these problems we recall once again that ethnography is incomplete without the cross-cultural comparisons which allow the uniqueness of ethnographic description to find a comparative spatial and temporal context. Moreover, when it comes to matters of historicity and generalization, anthropologists often make use of the methods of allied disciplines such as history, psychology, and sociology.

There are also persistent questions about the 'objectivity' of the data

collected by means of participant observation. When a chemist sets out to analyse a sample, she might use a spectroscope. Like any scientific instrument, a spectroscope can be *calibrated* so that the scientist can be reasonably sure that data collected with one spectroscope will be comparable to data collected with a spectroscope calibrated in another time or place. But what – or, more appropriately, who – is the instrument of data collection in anthropology? Obviously, it is the ethnographer, and calibrating a human being is a far more daunting prospect than calibrating a spectroscope. Each ethnographer is a unique individual, the product of a unique upbringing and education, replete with all the psychological predispositions – hidden as well as obvious – that constitute any human being. There have been notorious instances in which two anthropologists have studied the same community but come to very different conclusions about them. How, then, can we reconcile the inevitable subjectivity of participant observation with our desire for a calibrated uniformity of data collection? The short answer is that we can't, and it is this, more than anything else, that distinguishes social sciences such as anthropology from natural sciences such as chemistry, whatever their own problems of observer bias.

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Can the problem of ethnographic subjectivity be overcome? The origins of participant observation as the hallmark method of anthropology began at the end of the last century as an attempt to compensate for the variable reliability of descriptions of non-Western peoples. Not content to rely on travellers' tales, missionary accounts, and official colonial reports of 'customs and manners', W. H. R. Rivers, Bronislaw Malinowski, Franz Boas, and others among the founders of modern professional anthropology insisted on the first-hand collection of ethnographic data by trained observers. It was their hope that training would suffice to compensate for the prejudices of the observer. In the 1951 edition of the Royal Anthropological Institute's *Notes and Queries on Anthropology* the uninitiated were told that 'amateurs untrained in anthropology' are apt to assume that they are free from bias. This,

however, is far from the case; without a scientific training their observation will certainly be hampered by preconceived attitudes of mind.' Standardized categories for data collection, such as those presented in Notes and Queries and the Human Relations Area Files' Outline of Cultural Materials, had been created in an attempt to overcome observer bias and ensure the comparability of data collected by different ethnographers. In the 1930s some American anthropologists even went so far as to undergo psychoanalysis before fieldwork in an attempt to 'calibrate' the instrument of data collection, a practice quickly abandoned.

Other notable attempts to overcome these epistemological problems have included re-studies and studies undertaken by teams of ethnographers. One would think that a scientific approach to gathering ethnographic data would encourage anthropologists to re-study communities that had been studied before by other ethnographers as a check against subjectivity or bias. But this is far from common. To some extent this has been due to a sense of urgency among anthropologists to conduct 'salvage ethnography'. Many have been concerned that most of the world's smaller societies and traditional ways of life are fast disappearing and that it is more important to record those that have never been studied than to confirm results already collected. It must also be admitted that many anthropologists were first attracted to the field by the romantic image of the lone, intrepid explorer, and that an unspoken ethnographic 'machismo' has attached itself to those who have studied the previously unstudied. There has been, altogether, an understandable if misguided sense of proprietorship on the part of an ethnographer for 'his' or 'her people' which has made it very difficult for one ethnographer to 'poach' on the 'territory' of another. Finally, it has been rare for ethnographers working in communities that have been studied before to approach those communities interested in precisely the same theoretical or ethnographic issues as their predecessors. And because societies can change rapidly, separation in time of even a few years between an initial study and the next study

also makes it difficult for re-studies to provide a check on ethnographic objectivity.

On occasion, anthropologists have engaged in the study of a particular community by a team of researchers, partly to provide greater comprehensiveness and partly to compensate for individual observer bias. The 'Modjokuto Project' engaged seven social scientists (mostly anthropologists) in the study of a small town in central Java in the early 1950s; while the Mexican town of Zinacantan was serially the focus of scores of ethnographic studies in the 1960s and 1970s under the general supervision of Evon Vogt. Problems of funding and logistics make such projects difficult to organize and so they have been rare. Nonetheless, in some countries, notably Mexico and Japan, ethnographers are institutionally inclined to engage in team efforts, usually consisting of a group of advanced graduate students led by their professor. For all of this, it is not clear that the data collected by teams of ethnographers are significantly less subjective than those collected by groups.

More recently, some anthropologists have argued that 'objectivity' is a false issue. Our bias – that is, our social and historical situation – is what gives us a point of view, and hence constitutes a resource we should openly draw upon in our interpretations. Others contend that any form of representation is an exercise in power and control. To these critics, the whole enterprise of ethnographic description is suspect so long as asymmetries of power persist between the observer and the observed. These critiques have occasioned new styles of ethnographic writing. In contrast to the language of omniscient objectivity that characterized earlier ethnography, some now favour the presentation of relatively unedited texts representing a variety of 'voices' other than the ethnographer's. Other ethnographers have adopted the inclusion of a more autobiographical style of presentation, in which the ethnographer's background and relations with his or her subjects become a central topic of the ethnography. In a way, we may have come

full circle back to travellers' tales. Unfortunately, few ethnographers have proved to be as interesting as the people they study.

All the same, isn't it an act of extraordinary hubris for someone to propose to present a definitive account of another people, even when it is based on long-term 'participant observation'? And isn't it problematic that the vast majority of ethnographers are Westerners when the vast majority of their subjects have been non-Western? To some extent this is a self-correcting problem: more and more non-Western students are trained as anthropologists and more and more nations are developing their own traditions and styles of anthropological research. For example, most of the ethnography of Mexican communities is today written by Mexicans, in Spanish, which was not the case twenty years ago. The same can be said to be true of gender: women, who now constitute a majority of recent doctorates in American anthropology, are frequently engaged in the study of women, both at home and elsewhere. By the same token, a number of non-Western ethnographers have begun to turn their attention to the study of Western societies. The discipline as a whole can only benefit from additional perspectives. After all, Alexis de Toqueville's description of American society remains unsurpassed by any observation made by an American. In the same way, anthropologists have long regarded the 'outsider's perspective' they bring to their subjects as one of the principal advantages of ethnographic method. A person studying his or her own culture can be likened to a fish trying to describe water. While the insider is capable of noticing subtle local variations, the outsider is far more likely to notice the tacit understandings that local people take for granted as 'common sense' or 'natural' categories of thought. The outsider status of the ethnographer, then, can be regarded as a strength as well as a weakness, even as a strength crucial to the success of the enterprise.

The Ethics of Ethnography

The nature of ethnographic work is such that the researcher develops a unique set of relationships with the people he or she studies, with host institutions and governments, and with colleagues. As anthropology has matured, the moral issues raised by these relationships have become matters of concern. Various professional associations have debated the issues and framed codes of ethical conduct. For fieldworkers the first imperative is to ensure that one's research does not harm the people one studies. For example, John and a colleague wrote a history of a Maya town in Guatemala. In a book review, a geographer questioned their expertise and political commitment by noting that the book failed to mention and criticize the establishment of an army garrison in the town in the 1980s. John and his colleague had certainly been aware of the army's presence (in 1979 a drunken soldier fired a machine gun into the house where John was sleeping). But John and his colleague declined to discuss the army in their book because, given the political situation in Guatemala at the time, and their close work with certain individuals and families in the town, critical mention of the army could have led to retaliation against their friends. Similarly, Peter's account of the case of la Ninde makes use of pseudonyms to protect the anonymity of the parties concerned - a fairly standard practice among anthropologists. Like other anthropologists, he also uses pseudonyms to refer to the places where he has worked.

A Dispute in Donggo

A persistent source of ethical dilemma for ethnographers is to be found in the extent to which it is appropriate for ethnographers actively to influence the social, religious, or political life of the communities in which they work. In one celebrated case, for example, an ethnographer was presented with a situation in which members of her host community held the traditional belief that twins are inhuman and should be allowed to die of neglect. When twins were born to a village woman during her stay, she faced the dilemma of whether to intervene and if so, in what way. Should she try to persuade the mother **not to**

abandon her new-born babies? Should she offer to adopt them herself? Should she inform village or government officials who disapproved of the traditional practice? Or, out of respect for the beliefs of her hosts, should she do nothing? For all our efforts to frame codes of professional behaviour, there is no consensus among anthropologists as to how such dilemmas are to be resolved. Admittedly, most of the dilemmas anthropologists face are not matters of life and death, but the degree to which the participant observer should really participate in the affairs of the community remains a persistent and vexing problem. In a similar vein, John has frequently been asked by Mixtecs to aid them in entering the United States without a visa. How should he respond? On the one hand he feels a deep sense of obligation to people who have been his friends and hosts in Mexico. On the other hand, helping them in this way violates the laws of his own country.

At the same time, ethnographers have often felt compelled to become advocates for the people they study. The peoples anthropologists study have often been among those most vulnerable to colonial and neo-colonial oppression, genocide, displacement, poverty, and general powerlessness in the face of governments and other institutions. Anthropologists sometimes (although hardly always) have access to media and other means of publicizing the plight of the people they study and many have made use of this access. Advocacy has not been without risk to these anthropologists, who have suffered deportation, imprisonment, and even assassination in retaliation for their actions.

One ethical issue that has received increasing attention concerns intellectual property rights. Anthropologists have been criticized for 'profiting' from the 'expropriation' of indigenous cultural knowledge. Are indigenous peoples entitled to copyright knowledge that has traditionally been in the public domain? Should communities be able to exercise control over the publication of cultural knowledge? Should they be entitled to pass binding editorial judgement on the interpretations ethnographers make? Are ethnographers obliged to

share what profits, if any, they make from the sale of ethnographic accounts with the subjects of their accounts?

Ultimately, we have to confront more general ethical issues. To whom does an ethnographer owe his or her greatest allegiance? Is it to the people studied, to the sovereign government of the country where research takes place, to the agency or foundation that funds the ethnographer's research, to the academic or research institution that employs the ethnographer, or to the community of scholars to which the ethnographer belongs? Should ethnographers be expected only to add to humanity's knowledge of itself or should they be expected to provide more tangible benefits to the people they study or to the world at large? Should ethnographers be held to a higher standard than the one applied to journalists, filmmakers, or photographers who also report on their fellow human beings? These, too, are unresolved questions, subject to lively debate.

What can we expect of ethnography and the ethnographer? For all of the claims made for and against the products of participant observation, anthropology has always relied on what amounts to a good-faith effort on the part of ethnographers to tell their stories as fully and honestly as possible. Similarly, we have relied on the common decency of ethnographers to act with due regard for the integrity of their profession. We all recognize that complete descriptive objectivity is impossible, that a comprehensive understanding of any society or culture is unattainable, and that ethical problems are more easily posed than resolved. That we continue to pose these questions is perhaps the best indication of the fundamental health of anthropology as both an academic discipline and a humanistic enterprise.