UNPACKING "FIELDNOTES"


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Pretexts for Ethnography: On Reading Fieldnotes

Anthropologists do many things in the field and out, and while writing is one of those things, it is surely not the distinguishing characteristic of our work. Writing sets us apart neither from people in other disciplines and lines of work nor, always, from the people we seek to understand. Nevertheless, a focus on anthropological forms of writing can reveal something about the strengths and limits of anthropological knowledge.

Recent analyses of the conventions of ethnographic writing (e.g., Clifford 1983; Clifford and Marcus 1986; Marcus and Cohen 1982; Sperber 1982) are just part of a sustained exploration of the largely tacit dimensions of our work. During the past twenty years anthropologists have published detailed descriptions of the personal experience of fieldwork. While such accounts have not always been self-critical or analytical, they have been reflective in a particularly direct manner and have occasionally pursued epistemological and ethical or political issues merely named in manuals on research technique.

I thank Michael Morris, Harry Greer, Roger Sanjek, and J. J. Taylor for comments on an early version of this chapter, and also Jim Clifford, whose paper I read in 1986 as I was drafting this one and whose arguments helped to provoke mine. I do not take account of a number of important, recent works (e.g., Clifford 1988; Geertz 1988; Strathern 1988).
The “I” is generally not present in more recent considerations of ethnographic writing, as Rubinson (1986) notes. However, analyses of unspoken conventions—such as how an authoritative or persuasive voice is created in ethnography (Clifford 1986; Geertz 1988; Rosaldo 1986)—may pose a more systematically critical challenge to anthropological self-understanding than do descriptions of field experiences, a challenge akin to that raised by earlier exposés of the relationship between anthropology and colonialism or by the feminist critique of anthropological knowledge.1

This volume’s consideration of fieldnotes must be seen in the context of such reflexivity and critique. While fieldwork (the typification of anthropological practice in the popular mind) has been a focus of disciplinary attention, and while ethnography (anthropology’s official public medium) is now also an object of unsettling critical analysis, fieldnotes remain largely obscured from view, even among practitioners. They are a “muted” medium, seeming to be merely a means to an end, or an end to the day. One wonders whether fieldnotes constitute a topic worth writing about at all and casts about for a proper analogy: are they like historians’ archives, or like the notes historians take when they are in the archives?2 In view of the obvious centrality of fieldnotes to our work, professional silence on the matter ought at least to raise suspicions.

It is no wonder that fieldnotes are hard to think and write about: they are a bizarre genre. Simultaneously part of the “doing” of fieldwork and of the “writing” of ethnography, fieldnotes are shaped by two movements: a turning away from academic discourse to join conversations in unfamiliar settings, and a turning back again. As a kind of communication addressed primarily to oneself, they are unlike both the face-to-face but ephemeral sociability of fieldwork and the indirect but oddly enduring published exchanges at home. What is more, many (perhaps most) anthropologists have never actually read any before creating their own; they have well-established models neither for how fieldnotes are written nor for how they are used. Despite being created for oneself, fieldnotes are not meant simply as a diarylike record; however, neither are they a public archive. While they are supposed to be a reconstitutable record of field experiences—an anchor for the crafty frames of memory and possibly a resource for other researchers—their value as such is sometimes questioned: “The idea is that the best way to write a compelling ethnography is to lose your fieldnotes” (Schweder 1986). They are in fact ambiguous in form, content, and intention, neither here nor there (or, perhaps, both here and there). Smudged by gritty fingers and squashed bugs, any day’s sheaf of notes might include a series of chain and compass readings, jotted fragments of interrupted conversations, a typed-up interview transcript with marginal comments, a dense description of some event or person (suitable for publication), an outline for a dissertation or journal article, a comment on a book or letter recently read, an expression of personal feelings. Produced and still smelling of There—muddy, smoky, spicy evocations of people and places—fieldnotes, like ethnography, are simply a form of writing.

Discomfort with their personal side makes reading and writing about one’s own notes difficult (as this volume’s papers reveal). But reading fieldnotes is disconcerting not just because of their revelations about one’s personal anxieties and inadequacies or because of their ambiguity: fieldnotes are dangerous. Observations are not or written down in order to aid memory, but reading fieldnotes can challenge memory. It threatens to return one to uncertainty about what was what; it acts against the sense of the whole that one carries around in one’s head. Fieldnotes can contradict the single, anthropological voice we are all encouraged to adopt in our formal ethnographic writing at home by recording—however indirectly—the voices of the people we lived with when doing fieldwork. In this way, while fieldnotes mediate fieldwork and ethnographic writing and are shaped by both, they also subvert ethnography as surely as they are at odds with other aspects of the fieldwork experience.

In this essay I first describe my own fieldnotes to illustrate more concretely their particular form of fragmentation and their relations to the worlds of field and academy; the description is offered with the expectation (or hope) that my notes are typical—if not in details, then in function or sense. I go on to discuss some of the ways in which I have read and used fieldnotes. I conclude by considering the impact of different audiences and communities on the evaluation of fieldnotes. By the end, it ought to be clear that the dangers of fieldnotes are positive, even essential to critical cultural analysis.

1But see Rubinson 1986 and also Clifford’s (1986) self-critical remarks. Textual concerns may also lead one in a direction antithetical to feminism and anticolonialism.
Fieldnotes: Orientations and Disorientations

I agree with James Clifford (in this volume) that distinctions must be made among kinds of "field work"; the term is unwieldy and needs to be unpacked. While Clifford's scriptive categories are thought-provoking, I will need to unpack it differently because my focus is less on the contexts in which notes are written down than on how they are read and used.

I did field research in the Mendi Valley (Southern Highlands Province, Papua New Guinea) during 1977-79 and again for a few months in 1981; my research concerned sociopolitical aspects of the relationship between production and exchange and focused on understanding Highland pig festivals from the perspective of community history (see Lederman 1986). I also became interested in gender relations and in local economic and political innovation.

Over the course of my fieldwork in Mendi, I produced three main kinds of written fieldnotes: daily logs, typed files, and personal journals. Each kind is both orienting and disorienting for the reader in its own way. For example, an extended "description" in a logbook can be quite readable and provide apparently easy access to "what things were like"; it orients the reader by presenting an account that seems comprehensible in itself, or else by having traceable connections to other notes. At the same time it is disorienting insofar as it derives from heterogeneous, sometimes contradictory sources and documents a shifting perspective (move on this below).

More fragmentary notes such as my census data are relatively meaningless in themselves and need to be cross-referenced to be usable. They are disorienting insofar as they are so obviously incomplete. One needs to know more in order to interpret them, but they do not themselves point out a direction in which a reader must travel to complete them, and in fact, many directions are possible. At the same time, a census format does orient the reader to a single topical context.

I do not discuss tapes and photos here; their different mediums need separate consideration. I do not mean to imply that they are not "fieldnotes" too; both are quite relevant to any consideration of how fieldnotes preserve the "voices" of an anthropologist's research subjects, a central theme of this essay. But it would be more to the point and reify the notion of "voices" by asserting that they are necessarily preserved better on tape than in written notes. A process of selection of what is taped just as much as in written note-taking, and that process can be considered (consciously or not) to create a kind of invisibility in any record.

See Clifford, this volume, for a discussion of the inadequacies of this term.
sponses to my questions (concerning local events or linguistic points, for example). Whenever conversations became interviews—as they often did when talk turned to local history or to exchange practices—or whenever an event was so involved as to require an extended account, the log refers readers to my typed files. Like the census and interview material described below, these extended, log-style accounts were typed up and stored in ring binders, whereas entries in both the personal journals and the daily logs were handwritten in bound books.

As did the personal journals, daily log entries derived from various sources, although this is perhaps less obvious than in the journals, since these sources were more likely to be local (Mendi) ones—neither literary nor familiar to most potential readers—and, in any event, log entries contain little explicit information about them. For example, because I worked on a transition zone between two language areas, some of my frequent informants spoke a language with which I was not familiar; as a result, some log notes were based on direct discussions between me and my interlocutors, others on interpreter-mediated discussions. The logs do not always identify my interpreter (though that information may be recorded in my journal), and when they do, they rarely offer information about that person’s particular biases and active interventions.4

Another reflection of their diverse sources is that some of the log notes were written while people were talking, and others were written up afterward with the help of abbreviated jottings taken down in the steno pads I always carried with me. I did not distinguish between these two note-taking methods in the log.5 Rewritten notes usually contained more information than the original jottings, but the press of events or the limits of lighting—not to say my inadequate recognition of their importance—often led to uneven levels of detail concerning settings, my own and my assistants’ moods, and our respective relations with our interlocutors. My personal journals contain much of this missing background information, an indication that I did not then consider it to be of strictly “anthropological” interest.

In any case, even when I rewrote my abbreviated, nearly illegible

4 I agree with Obeyesekere (1984), who noted that much of interest could be written about the "interpreter effect."

5 An extended discussion could probably be written about what goes on when personal shorthand notes, written to the midst of a conversation or event, are transcribed for one’s permanent records. There are no doubt many ways of doing this when it is done at all.

steno-pad notes in the logbooks, expanding them in a legible script while I could still decipher them. I made no effort to compose and consolidate entries on a particular topic but rather transcribed them in the same order as I had recorded them. As a result, they contain interpolations and interjections: notes on so-and-so’s explanation for the fias he made at a public meeting; a list of other meetings planned; some Mendi terms; more notes about the funs. All the while, place names and personal names are explained only if I did not know them at the time of the note-taking.

Many a day’s log entries contain a series of unrelated items—a sentence reporting that a friend had gone off to his wife’s father’s place to repay a gift, a paragraph describing an interaction overheard on a village path that morning, a longer report summarizing several conversations bearing on a land dispute, a list of names of people who had contributed to a mortuary presentation a week or so before—all with only sporadic mention of where related items might be found. Very often there is no clear indication of why any particular item was deemed noteworthy at the time. Neither could a naive reader tell whether what is contained in an entry is complete in itself, as an item either of local concern or of anthropological interest. The giftrepid that day may have been controversial or might become so, the repayment might help to clarify an exchange rule previously (or soon to be) described.

To some extent, the log’s chronological organization is orienting, at least when what one is looking for is the story of a dispute or anything else that is played out over time, yet this mode of reading is inefficient. As the author of the log, with a reasonable memory of where things are and an index for each logbook, I nevertheless find myself reading over many items of no direct relevance to my immediate goals whenever I consult it. The eye wanders; unsought facts make their appearance, and unanticipated connections suggest themselves, leading the eye further astray. With all these juxtapositions, the daily log is the most disorienting of my notes.

But chronology is key in another way. These disorientations—collages of apparently unrelated items, ambiguities as to why certain items were included (or excluded) and whether (or on what basis) any item is complete—engender reading problems because log notes increasingly presuppose, and subordinate themselves to, the context of understandings created through long-term social exchanges with people in one’s field community. Over the course of fieldwork one
becomes party to conversations and situations defined not only by an interpreting observer's autonomous eye, or by external criteria of interest, but also by deepening relationships with some of the people with whom one is living. Happenings become notable (events) against a background of one's friends' and neighbors' not always convergent concerns.

These in-field matters have their own logics which, played out over time, may gradually shift the emphasis of one's notes away from preexisting, comparative frames of reference and toward diverse "colloquial" ones (Remenant 1985). Any item newly noted in a logbook may have many unnoted but significant antecedents that made its coming-into-(note-)consciousness possible. Just as one has limited control over the intertextual shaping of one's attention, one has only partial control over these changing colloquial influences. Yet while colloquial contexts for the interpretation of events are the special vantage points that fieldwork opens up, they probably cannot be fully recorded. Consequently, reading notes requires remembering (or discovering) the various local biases and partialities that formed an important but largely tacit rationale for inclusions and exclusions. This inevitable incompleteness is what makes reading one's own old notes, not to mention other people's, so difficult.

In addition to the handwritten journals and logs, I typed up notes taken during long interviews and complex events (my own observations and reports of what other observers and participants told me on the spot or afterward). Some interviews arose spontaneously out of informal conversations concerning events or topics of particular concern to me or to my hosts; these were the same, except for level of detail, as the sorts of items found in the log. Apart from these extended log-style accounts, my typed notes include the results of a community-wide household census, responses to systematic interviews concerning marriage and bridewealth, mortuary prestations, land tenure histories, exchange/partnership histories, daily "gift-debts" and "gift-credits" and other matters, and descriptions and measurements of the community's gardens, garden production, and pigs. The results of each of these investigations were typed up every day or so; back in the States, each was filed in its own ring binder.

My typed surveys are simultaneously the least readable and the most orienting and formal of my notes. While my personal journals are orienting on the level of the part but not of the whole, the reverse is true of the surveys. They are hard to "read" because they contain

decentralized responses to questions: the rationale for the questions is contained in the log and the journal, but the question-and-answer "situation"—the participants and their mutual relationships at the time of interviewing—is not described in the typed notes themselves. Nevertheless, any set of interview notes is composed of the responses of individuals to questions on a relatively coherent topic; it orients the reader to a single topic and involves few of the distractions that are rife in the journals and the logs.

Despite their apparent coherence, the survey notes are a precipitate of the dialectical relationship between intra-anthropological discourse and the interactions of fieldwork. For many of my interview projects I first defined topics and outlined questions with the anthropological literature on other Highlanders in mind; that is, with the desire to address topics with which other Highlands researchers were also concerned. But I worked out the boundaries of the topic, and the details and phrasing of the questions included in even the most general survey, with the help of my field assistants, my closest friends in the community, and the people I interviewed in each case.

For example, after talking and corresponding with a number of Highlanders researchers before I arrived in Mendi in 1977, and having talked with my husband during the preceding few years about his own historical research on the account books of eighteenth- and nineteenth-century American farmers, I decided to create a monthly gift exchange "accounts" for a sample of the female and male residents of my field community. The idea was to get a sense of the everyday gift exchanges of ordinary people in Mendi to complement my investigation of public "ceremonial" exchange. As a follow-up to that work, about a year into the research we interviewed all members of the "accounts" sample concerning the history of each of their partnerships. Considering that an average member of the sample might have a network of about forty exchange partners, we needed a way to organize the interviews meaningfully, so as to facilitate memory and to maintain interest.

The first people I interviewed were two of my closest friends in my field community: my village sponsor, Nare (a local leader), and Mel, my main field assistant. Both of them were comfortable enough with me and proprietary enough about the work I was doing to tell me how they thought I ought to conduct the interview. In separate conversations they each explained how they remember their own exchange obligations, and how those mnemonic devices might be employed in this unfamiliar context. My questions and what they each chose to
explain during their partner-by-partner histories helped me to develop explicit "prompts" in subsequent interviews. The interview format remained flexible as I spoke with the people I knew best (people most likely to speak without "prompting" questions and most likely to offer unsolicited advice and commentary) and gradually became more formal as I went along. Consequently, the results reflect both anthropological and local frames of reference. All the surveys my husband and I carried out in Mendi originated in this sort of interactive process and bear its traces—though it might be hard for anyone else to reconstruct, since the diachronic dimensions of the surveys are obscured by the way I have told them.

Using Fieldnotes

A written ethnography is not just a summary or selection of "what's in the notes." The point of ethnography is not, after all, to describe one's fieldnotes (as I am doing here) or to reconstruct the anthropologist's day through a chronological collation of notes but rather to enable one's audience to understand something of interest about a corner of the world they have not experienced directly themselves; to share that to which one's field experience has given one access. Something of interest to one's audience: what that is depends on the audience and how far one believes they are willing to travel.

I used my notes for self-clarification when I was still in the field. While my personal journal entries record reactions to field experiences, they are also the products of a critical reading of the log and other notes. In the field I used journal writing as a time for exploring connections between the various things I was learning about and for reciprocal translations of the terms of my anthropological and Mendi knowledge. This work invariably generated questions; the effort to orchestrate my knowledge clarified some of what was missing or discordant. Such frequent summarizing and rethinking was a check on the complacent sense of everyday competence and familiarity that long-term fieldwork can engender (Lederman 1986b). After all, frustrating or confusing interactions with informants, assistants, or friends and shifts in the sense of how things fit together are often repressed in the interest of carrying on.

In my case the journals became the place where these things were preserved for conscious reflection. My journals inform me, for ex-

ample, that I was not fully aware of the significance of exchange partnerships—a central component of my present understanding of Mendi social relations—until the last month of my first period of research, even though I had been focusing on them all along. This realization enables me to read my log and survey notes more critically and warns me of the need to compare my early reports of conversations and incidents with those written toward the end of the research.8

When I first returned from the field in 1979, I planned to index my notes but soon changed my mind. I was dissatisfied with the categories I was imposing on them and wanted to give myself more time to understand what I had learned in Mendi. For the same reason, I held off tabulating and summarizing the information contained in the surveys. In short, I was not at all sure how to read and use my own notes. Viewed as a whole—as shelves of ring binders and journals, and as stacks of paper on the floor near my desk—the notes were inaccessible. Journal writing, my in-field vehicle for exploring the other notes, no longer seemed appropriate; its rhythmic pacing had been too closely linked to the daily rhythm of fieldwork.

Another method of using the notes had begun to assert itself, however, even before I left the field. It was occasioned by the need to address audiences and contexts quite different from those that had shaped my journal writing and other fieldnote-taking. Several months before leaving Mendi I prepared an abstract and outline for a paper I hoped to read at the American Anthropological Association meeting later that year, and during my last week in Mendi in 1979 I presented a research report to the Southern Highlands Province Research Committee. These writing projects focused on issues defined for me by preexisting "conversations" among people who were not members of my field community. The research report addressed questions raised by provincial and national development planners about the rural political economy in Mendi; the meeting paper concerned the participation of Highlands women in gift exchange, a topic of general as well as

8Two points ought to be spelled out, though they may be obvious. First, as I have indicated, the notes themselves develop during fieldwork: one's use of terms shifts in subtle ways as one's understanding of local concepts and relations changes. Second, during any reading of the notes—in the midst of fieldwork or subsequently—one's current sense of the whole imposes certain consistencies on this heterogeneous source. As Ostendorp, Wolf, and others in this volume point out, one's changing sense of the whole is registered in the changing interests and perspectives expressed in the writings produced during an anthropological career. Clearly, this process may not be evident at any single moment in any particular writing.
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regional interest in ethnography. While the terms of these conversations shaped my participation in them, I joined in with the hope that introducing the Mendi case might shift the terms a bit.

Other events intervened to influence the ways I used my notes after I had returned from Mendi to New York. Like the two writing projects already mentioned, those events involved addressing specific audiences and entertaining conversations that already had histories. Reading the newsletter of the Association for Social Anthropology in Oceania in the fall of 1979, I found descriptions of two symposia to be held at the association's meeting the next spring. Since both symposia were still open to the inclusion of new papers, and I felt that I had observations relevant to the topics, I sat to work writing them up.

Not only did participation in these symposia help me to use my notes by orienting me to a specific audience and topic (just like the cases cited above), but it also suggested a particular place of entry into the notes. For both papers—one involving the political uses of language (Lederman 1986) and the other concerning the relationship between "society" and social change (Lederman 1986)—I planned to address the symposium topics by analyzing events I had studied in Mendi: a political meeting, a curing ceremony. As the work progressed, of course, I had to go far beyond simple description of the events themselves, tracing out connections to other events and collaring what many informants had told me about related matters.

In retrospect, it seems that "events" were good modes of entry into fieldnotes. Events happen at particular times, and can therefore be found readily in chronologically organized notes, whether one has a good index or not. They also have an apparent "wholeness"—a superficial sense of boundedness—that facilitates initial description. Event-based topics helped to orient me in my notes because they "made sense" in three ways: each occurrence had been a focus of local interest and discussion in my field community, but it also related to some domain of anthropological discourse, and from a practical standpoint it directed me first to my most readable notes.

Starting with events that had been of concern in my field community helped to preserve a local logic, but the integrating rationale for

"Each ASAO symposium is meant to be the last stage in a collective process that also (ideally) involves informal face-to-face discussion of ideas, followed by an exchange and discussion of working papers.

38. The question of what constitutes an "event" is thin or that culture (or cultural context) is complex, for a suggestion with regard to Mendi, see Lederman 1986a.

these inquiries was at least as much comparative as it was local. As I confronted ethnographic questions I had not explicitly thought about in the field, notes about events guided my search through the less immediately readable survey, bits of conversation and observations recorded in the fieldnotes, and so on. Unlike a project of indexing, of tabulating survey results, or of explicating concepts I knew to be important when I was in the midst of research, receding my record of events maximized the possibility of discovering relations and connections within the notes of which I had not previously been aware. This experience, which effectively turned the notes into an archive for me by suggesting questions different from those around which the notes were collected, finally enabled me to do the indexing and tabulating without which a longer writing project would not have been possible.

I will discuss one last use of fieldnotes here: their incorporation into ethnographic writing. Ethnography issues from an "argument" ("dialogue" may sometimes be too gentled a term) between comparative and local voices. While the comparative voice is usually the more influential (given the demands and capacities of ethnography's readership), the textual echo of local voices may be privileged in certain styles of ethnographic writing (as in life histories and transcripts of native texts). If my experience is at all typical, this argument has its clearest written expression in fieldnotes. It is there that the comparative attitude is humbled in the effort to understand an immediate but unfamiliar and confusing reality. That is not by any means to say that it disappears. But at least some balance is achieved, in the very course of fieldwork, between transcriptions, paraphrasings and reports of what some others are saying and doing, and the ethnographer's composed description and commentary.

One can bar this argument from one's formal ethnographic writing. Or one can choose to introduce it into the text by allowing fieldnotes to break through at critical points to advance the argument or even to

38 Transcriptions and paraphrases obviously involve interpretation, even "true" informants and ethnographer speak the same language; and changing contexts of memory, presentation, and of reflexivity may foreground as "interpretation" that which was previously unconcerned in such. It may be that fieldnotes provide more ready access than ethnographies do to the interpretive process, regardless of the ethnographer's commitment to "experimental" ethnographic writing. Description in the notes is more, clearly the product of a concrete social process involving particular people. Even one works to compose some of one's notes in the form of funded (publishable) descriptions, the balance is likely to convey a specific voice and perspective, the rough edges of uncertainty and questions with named others.
constitute it (as, for example, Clifford Geertz has done; see Clifford, this volume). Allowing fieldnotes to break through does not necessarily require direct quotation from the notes, but it does demand that some of the fragmentation of knowledge—some of the contradictions and polyvocality characteristic of fieldnotes—be represented to readers to consider, alongside the writer’s interpretive efforts of orchestration.

In other words, ethnographic writing is all about directing readers toward novel modes of seeing the world (an effect achieved by maintaining authorial control, one way or another). One claim to a right to write this way is based on bouts of successfully disorienting field research (and, presumably, on discovering a way of taking down and using equally disorienting notes). Bringing the field home is only fair to disoriented readers in sometimes an awkward way to encourage a rethinking of received categories and a reorientation of perspective.

I have tried several times to incorporate disorientations into my ethnographic writing. After composing a brief, univocal community history of a Mendi pig kill in a book mostly devoted to exploring the social-structural background of such events (Lederman 1986a), I wrote a paper (Lederman 1986a) that discusses some of the local sources of historical knowledge in Mendi, by way of arguing that while the Mendi have a dynamic past and present, they do not necessarily use “historical” arguments (as Europeans and Americans often do) to assert their agency in the world. That paper catalogued disparate observations I had made in the field concerning Mendi representations of the past, less to orchestrate an interpretive model than to create a sense of possibilities. The point of presenting the material in a relatively disjointed fashion was to encourage readers to rethink the meaning of “history” as applied to contexts like that of Mendi.

Similarly, in several places in my ethnography, What Gifts Engender (1986c: 40–41, 47–52), I present descriptions of what are essentially fieldnotes. They paraphrase or quote statements by my Mendi informants that either contradict one another or else do not fit existing ethnographic paradigms—or “gatekeeping concepts,” in Arjun Appadurai’s (1986) useful phrase. In this case the issue was the form and significance of male collectivities (“clans”) in Mendi. Using as my model a paper by Ray Wagner (1972), which questions whether there are “groups” in the New Guinea Highlands, I tried to clear a space for such a question about the Mendi by discussing my “sources” rather directly. In a later paper dealing with a related issue (Lederman 1986b), preserving the contradictory perspectives of Mendi men and women as I found them in my notes rather than giving them a unifying “glaze,” enabled me to raise questions about the relevance and implications of a general model of the social structure.

It would be interesting to discover how frequently fieldnotes are employed in this way in published ethnographies. Their use to bring the disorientations of fieldwork home to readers—the better to shift the terms of existing anthropological conversations—may be more common than it seems, although it may be more if we look only for deliberate and direct quotation. Because of the dangers and ambiguities of fieldnotes, and because of their privatization (which encourages each of us to interpret our confusions primarily in personal terms, as signs of inadequacy, rather than in terms of cultural disjunctions), the notes themselves may be disguised and detectable only indirectly as a force acting against received comparative categories.

Communities and Audiences

To historians who read anthropology, “being there” is anthropology’s distinct advantage insofar as it gives us a sense of the whole and the conviction that we have understood a place and a people. But that sense of the whole is treacherous; from this point of view, historians are lucky that their convictions clearly are conscious and hard-won acts of imagination. The case with which we can claim to know the worlds we invent—the fact that we can claim “to remember” them rather than having to admit always that we have fashioned them—is dangerous. We might do better to be suspicious of that ready familiarity, that implied factuality, even as we strive to convince readers, in authoritative and not so authoritative ways, of the plausibility of the worlds we write about.

Anthropological research practices do not automatically check the human tendency to familiarize strange circumstances, but they offer the possibility of doing so, and we can choose to emphasize it. To that end, it is interesting that fieldnotes can have the reverse effect “at home” from the one Clifford (this volume) describes for them in the field.

In the field, living with an alien reality, every new day offers us...
opportunities for a confrontation between our existing ways of understanding the world and those of our neighbors. In the midst of our research, many of us work to create contexts for long-term (post- research) dialogue with the people we live with and study—whether simply by building close personal friendships or by making practical or political commitments, of one sort or another—even when we do not write about these efforts. Whether personal or political, such involvement can help us in the tendency to interpret what we see and hear solely from our own perspective. But both the anthropologist and his or her informants continue to have their own interests as well, not all the projects of either are necessarily of moment to the other. For many, though certainly not all anthropologists see separation or, more strongly, this active disengagement is palpable in the everyday movement of writing in the field. Looking away to write something down while others continue to argue; turning one’s back to type something up while around the hearth the rest are still laughing. To understand the role of fieldnotes in the field, one has first to acknowledge that being in the field involves placing oneself deliberately in a context of commitment doubly different from the normal one. As we all know, this act need not involve any traveling at all: it sometimes involves simply a shifting of attention and of sociable connection within one’s own habitual milieu. From this perspective the “field” is not so much a place as it is a particular relation between oneself and others, involving a difficult combination of commitment and disengagement, relationship and separation. That one is writing about what one is simultaneously living is part of the separation and difficulty. But there is more to be said: the question is, for whom do we write? The point is that writing in the field is more often than not a very tangible sign of our double lives, of sociable connections in two

11Performing dual, apparently contradictory roles in the field as friend or engaged participant on the one hand and as note-taker, photographer, recorder, or translator on the other—as close and as distant—is a central experience for many anthropologists. The disquietengendered by that experience helps to motivate professional reflexivity. Engagement ought not to be thought of as a means to an end of better note-taking, nor ought note-taking to be thought of as a justification for being there or as something that gets in its way. As anthropological activities, these are two moments of the same process. Note-taking is not anthropological (field)note-taking without long-term participation in everyday life, and that participation is a less anthropological experience without the display of systematic comparison between alternative, inquiring realities which keeping notes encourages.

12The separation is quite clear when we write ethnographies for anthropologists and for the anthropologically trained. It is less so when those we write about will form a large part of our readership.

Directions. To the extent that our two worlds are distinct, our loyalties are divided, and we may feel compromised. But that is the price we pay for a unique voice.

Once we are home, however, the scales tilt overwhelmingly in one direction. The commitments we have made to people in our field community are subjected to intense if contradictory competition with commitments to our professional community, which for most of us exerts a more persistent influence. Our conversations, formal and informal—in seminars, conferences, and hallways, and indirectly on the pages of journals and books—are constrained by common anthropological idioms. As Appadurai (1986: 357) has emphasized, “gate-keeping concepts” (such as “honor and shame” in the Mediterranean) can virtually create ethnographic “places” and sustain our ways of talking about them; insofar as they frame our theorizing about the places where we do our research and “define the . . . dominant questions of interest in the region,” these concepts necessarily affect how we use our fieldnotes. How we read our notes is also affected by habits of thought that transcend approaches to particular ethnographic “places”: Western presuppositions concerning gender, for example (see Wolf in this volume).

These sorts of influences may even be felt in the midst of fieldwork—while presenting a seminar report during a research break, perhaps. Certainly, many of us can tell stories about our traumatic resocialization to academic discourse on route home from the field. Along the way, local realities—Alceme’s dream, the death of Min- hup—frequently become exemplifications chosen to illustrate a point whose rationale lies outside Alceme’s world, in a context in which Alceme does not laugh with others around the hearth just a few paces from one’s typewriter.

Once we are home, our written styles encourage narrative closure and a final analysis: in conventional ethnography, decisions need to be made about what’s what. Now, fieldnotes can be party to that. As a corpus, the notes may give us the sense that, for the moment anyhow, they contain the basis for all that can be written about a place: the fundamental intangibility and infinite complexity of social experience reduced to a “thing” which, even when very bulky, has finite dimensions. Given this finiteness, we can talk about how efficiently or inefficiently fieldnotes are used in this or that case in the production of ethnography (see Plath, this volume). And their concreteness restores our confidence in the possibility of “grasping” social reality.

But simultaneously, fieldnotes can defamiliarize our knowledge of
the field, and perhaps that is one reason why they disturb us so much (see Jackson, this volume), why some of us avoid using our notes when we write, and why stories about lost or destroyed notes (such as that of Leach’s Political Systems of Highland Burma) take on mythical dimensions. Having notes—all neatly typed or bound, all stored safe and sound—is one thing; it validates our anthropological communications. But using notes is quite another: that activity shows fieldnotes to be not a fixed repository of data from the field but a reinterpretable and contradictory patchwork of perspectives. We tightly fear that immersion in them might cause us to doubt our conviction about what’s what and (even worse!) lose our putative advantage over the historians.

In this way, fieldnotes can have an effect at home quite opposite from their effect in the field. While one may indeed have to turn away from direct engagement with people in one’s field community in order to “inscribe” notes and type them up, at home one has to disengage from ethnographic discourse in order to consult them. While this movement is not exactly like returning to the field, still it does put one back in touch—mediated and imperfect though it may be—with another set of categories, commitments, and values.

Moreover, it preserves the tension between what we talk about with our interlocutors in the field and our dialogue with our fellows at home. But after all, that tension is what animates an anthropological sensibility. Anthropology can no longer claim to produce descriptions of cultural traditions through an imaginative separation of Self and Other. A recognition that connections between the two cannot be factored out—that they are constitutive both of our scholarly practice and of the phenomena we study—has helped motivate the recent scrutiny of ethnographic writing. These connections are no less evident in fieldnotes than anywhere else. Thus it makes sense to extend that scrutiny to fieldnotes, as the corpus of still largely unexamined texts in which much of the significant work of decontextualizing and recontextualizing cultural categories and idioms takes place. This essay urges that equal attention be paid to the scenes of reading notes as to those of their writing, the better to appreciate those texts’ critical potentialities.

REFERENCES


Pretexts for Ethnography


