Fieldnotes: The Makings of Anthropology.
Roger Sanjek, ed. Cornell University Press.
1990.

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Notes on (Field)notes

This essay aims to complicate and decenter the activity of description in ethnography. It begins with three scenes of writing, photographs printed in George Stocking's *Observers Observed.*[1] The first, a recent photo by Anne Skinner-Jones, catches the ethnographer Joan Larcom glancing down at her notes while seated on a straw mat among women and children on the island of Malekula, Vanuatu. It is a moment of distraction. Larcom seems preoccupied with her notes. Two women look to the left, beyond the frame, at something that has caught their attention. Two boys stare straight into the camera. Another child's gaze seems riveted on the ethnographer's pen. The second image is a photograph from 1898 showing C. G. Seligman, Malinowski's teacher, in New Guinea. He is seated at a table surrounded by half a dozen Melanesian men. One of them sits rather tentatively on a chair drawn up to the table. Various ethnographic objects are scattered there. Seligman is intently writing in a notebook. The third scene, featured by Stocking on his volume's cover, finds Malinowski working at a table inside his famous tent in the Trobriands. He has posed himself in profile, turned away from a group of men who are looking on from just beyond the tent flaps.

[1]See Stocking 1983: 179, 82, 101. The volume contains other revealing scenes of fieldwork, more or less posed, which might be compared to the genre in realist painting which portrays the artist with model(s) in the studio.

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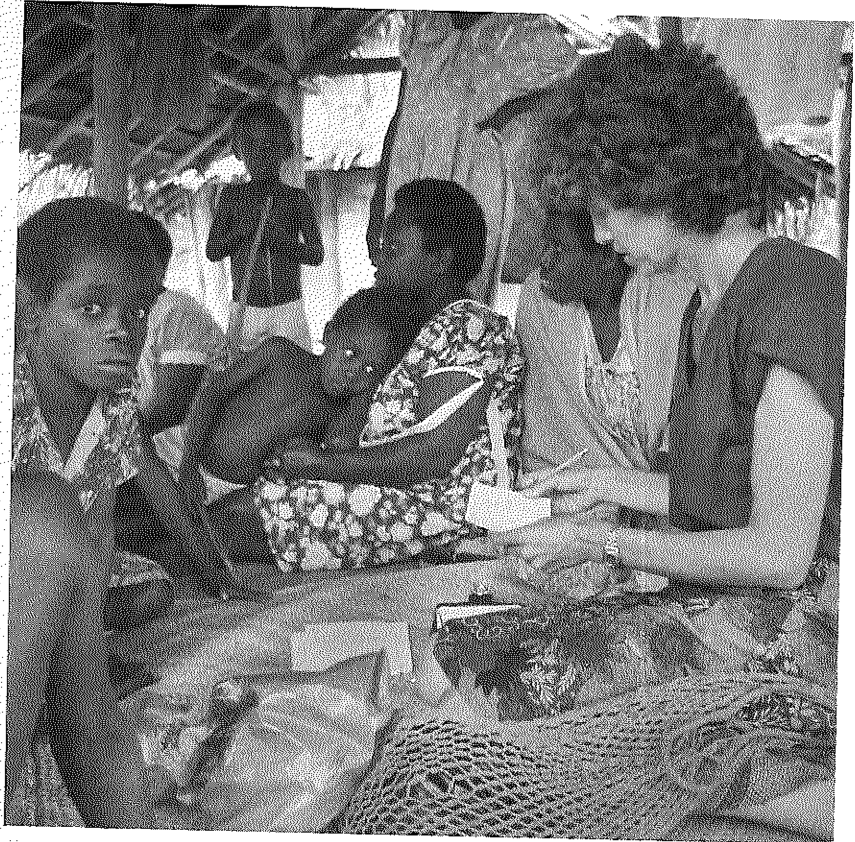
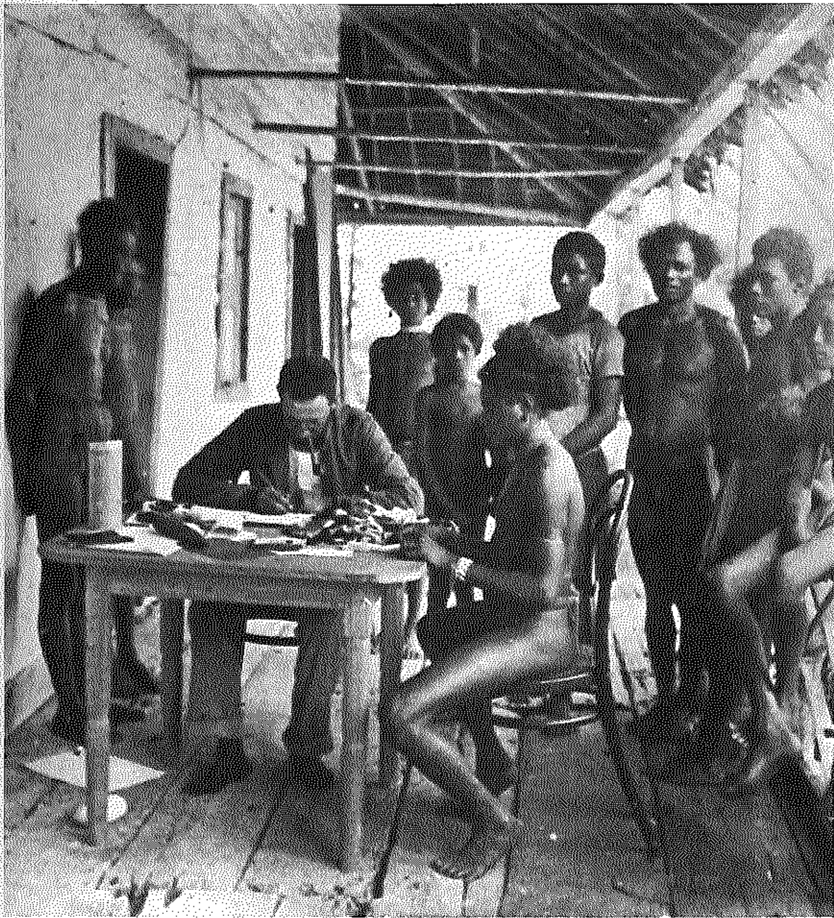
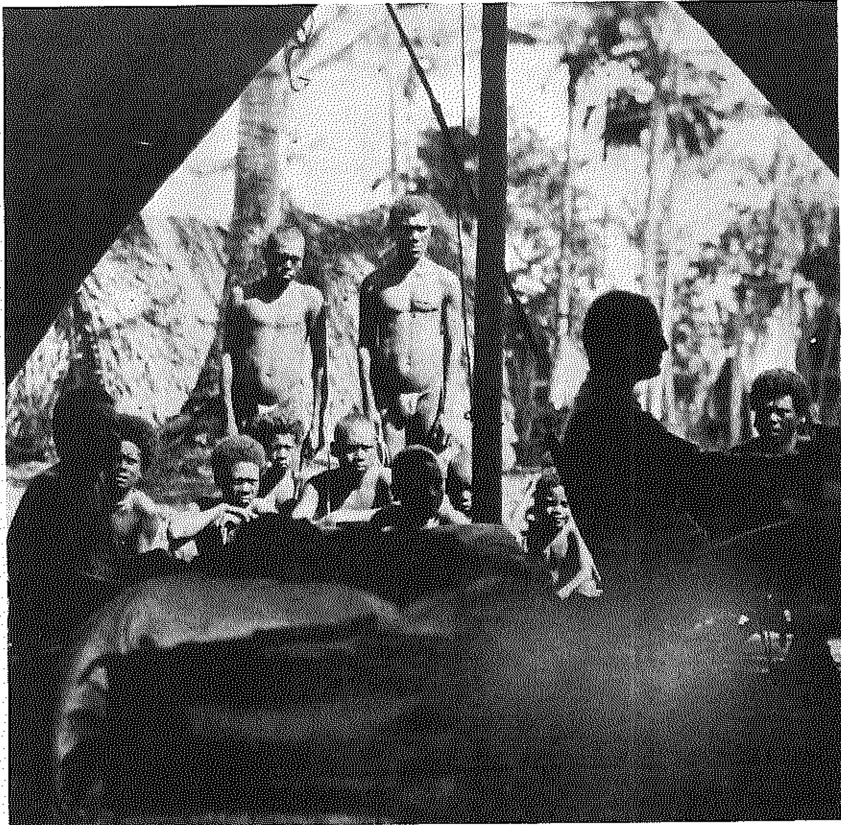


1. *Inscription.* Joan Larcom with informants in Southwest Bay, Malekula, Vanuatu. Courtesy Ann Skinner-Jones.



2. *Transcription.* C. G. Seligman at work, Hula. Courtesy University Museum of Archaeology and Anthropology, Cambridge, England.



3. *Description*. Malinowski at work, Omarakana. Courtesy Mrs. Helena Wayne-Malinowska.

These three remarkable photographs tell a lot about the orders and disorders of fieldwork. Each would repay close attention. But I am using them here merely to illustrate and to distinguish graphically three distinct moments in the constitution of fieldnotes. (I can only guess what was actually going on in any of the three scenes of writing.)

I use the first to represent a moment of *inscription*. I imagine that the photo of Joan Larcom glancing at her notes records a break (perhaps only for an instant) in the flow of social discourse, a moment of abstraction (or distraction) when a participant-observer jots down a mnemonic word or phrase to fix an observation or to recall what someone has just said. The photo may also represent a moment when the ethnographer refers to some prior list of questions, traits, or hypotheses—a personal "Notes and Queries." But even if inscription is simply a matter of, as we say, "making a mental note," the flow of action and discourse has been interrupted, *turned* to writing.

The second scene—Seligman seated at a table with his Melanesian informant—represents a moment of *transcription*. Perhaps the ethnographer has asked a question and is writing down the response: "What do you call such and such?" "We call it so and so." "Say that again, slowly." Or the writer may be taking dictation, recording the myth or magical spell associated with one of the objects on the tabletop. This kind of work was the sort Malinowski tried to dislodge from center stage in favor of participant-observation: getting away from the table on the verandah and hanging around the village instead, chatting, questioning, listening in, looking on—writing it all up later. But despite the success of the participant-observation method, transcription has remained crucial in fieldwork, especially when the research is linguistically or philologically oriented, or when it collects (I prefer "produces") extended indigenous texts. Boas spent quite a few hours seated at a writing table with George Hunt. Indeed a large part of Malinowski's published ethnographies (their many myths, spells, legends) are the products of transcription. In *Return to Laughter* Laura Bohannan (Bowen 1954) advised prospective fieldworkers: "You'll need more tables than you think."

The writing evoked by the scene of Malinowski inside his tent may be called *description*, the making of a more or less coherent representation of an observed cultural reality. While still piecemeal and rough, such field descriptions are designed to serve as a data base for later

writing and interpretation aimed at the production of a finished account. This moment of writing in the field generates what Geertz (1973) has called "thick descriptions." And it involves, as the Malinowski photo registers, a turning *away* from dialogue and observation toward a separate place of writing, a place for reflection, analysis, and interpretation. Stories of fieldwork often tell of a struggle to preserve such a place: a tent with the flaps closed, a private room in a house, a typewriter set up in the corner of a room, or, minimally, a dry, relatively quiet spot in which to spread out a few notebooks.

The three scenes of writing are, of course, artificially separated: they blend, or alternate rapidly, in the shifting series of encounters, perceptions, and interpretations called fieldwork. The term "fieldwork" has a misleading unity, and breaking it up in this way may at least have a defamiliarizing effect. Moreover, it should be apparent that, as I am using them here, these "scenes" are less representations of typical activities than images, or figures, standing for analytical abstractions. The abstractions refer to basic processes of recording and constructing cultural accounts in the field. I have found it useful to take these processes, rather than fieldnotes as such, as my topic. For it is clear from Jean Jackson's survey, as well as from the diversity of observations contained in this volume, that there can be no rigorous definition of exactly what constitutes a fieldnote. The community of ethnographers agrees on no common boundaries: diaries and journals are included by some, excluded by others; letters to family, to colleagues, to thesis supervisors are diversely classified; some even rule out transcripts of interviews. The *institution* of fieldnotes does exist, of course, widely understood to be a discrete textual corpus in some way produced by fieldwork and constituting a raw, or partly cooked, descriptive database for later generalization, synthesis, and theoretical elaboration. But within this institution, or disciplinary convention, one finds an enormous diversity of experience and opinion regarding what kind of or how much note-taking is appropriate, as well as just how these notes are related to published ethnographies. A historical account of this diversity (linked to influential teachers, disciplinary exemplars, and national research traditions) would be revealing. There is, however, a problem of evidence: most of the actual practice and advice is unrecorded or inaccessible. Fieldnotes are surrounded by legend and often a certain secrecy. They are intimate records, fully meaningful—we are often told—only to their inscriber.

Thus, it is difficult to say something systematic about fieldnotes, since one cannot even define them with much precision. The three processes marked off in this essay account for a good deal of ethnographic production without exhausting the subject. And it should be stressed at the outset that a focus on the interrelations of inscription, transcription, and description need not imply that writing is the essence of fieldwork. Its importance is suggested by *-graphy* in the word ethnography, but there is no point in replacing the misleading formula "participant-observation" with an equally simplistic "participant-inscription."[2] Fieldwork is a complex historical, political, intersubjective set of experiences which escapes the metaphors of participation, observation, initiation, rapport, induction, learning, and so forth, often deployed to account for it. The frankly graphocentric analysis that follows merely brings to center stage processes that have until recently been simplified or marginalized in accounts of ethnographic research.

Fifteen years ago Clifford Geertz asked—and answered—the crucial question underlying this collection of essays: "What does the ethnographer do—he writes" (1973: 19). His influential discussion went a long way toward opening up a broad domain for debate (see also Crapanzano 1977; Dumont 1978). But I will suggest in what follows that Geertz and the mainstream of "symbolic anthropology" unduly narrowed the domain of ethnographic writing to processes of inscription and interpretive description. My three scenes of writing are an attempt to complicate matters.[3]

[2]Jean Jackson and Simon Ottenberg (this volume) discuss the crucial function of memory as a (re)contextualizing process making fieldnotes (re)intelligible. The role of fieldnotes as mnemonic artifacts largely escapes my graphocentric analysis. Nor do I deal with the full range of documentary materials produced and gathered in the field—maps, photos, documents, objects of diverse sorts.

[3]In his book *Works and Lives: The Anthropologist as Author,* which appeared after this essay was completed, Geertz writes of cultural description with a good deal more hesitation than he did fifteen years before—"now that anthropologists are caught up in the vast reorganization of political relationships going on in the world and the hardly less vast rethinking of just what it might be that 'description' is, . . ." (p. 141) "The moral asymmetries across which ethnography works and the discoursive complexity within which it works make any attempt to portray it as anything more than the representation of one sort of life in the categories of another impossible to defend" (1988: 141, 144). Description as a perhaps impossible goal is not rejected in *Works and Lives.* But there is a new emphasis: thick description becomes contingent description, caught up in history, politics, and the imperfect arts of writing and translation.

Scene One

What is most extraordinary in the image chosen by Joan Larcom to represent her fieldwork in *Observers Observed* is the sense of confusion it registers. Data inscription appears not as an orderly process of collecting or recording but as an improvisation in the midst of competing, distracting messages and influences. The photo's play of gazes suggests (1) that the focused ethnographic moment always leaks beyond its frame into other "irrelevant" events; (2) that the ethnographic observer is always her- or himself observed; and (3) that any representation of this messy event, as here the photograph, is itself part of the event. The gazes, directed to the act of writing, to something outside the scene, and to the photographer, signal the confusion of fieldwork, its inescapable reflexivity, and the *struggle* to register data.

The photo is also appropriately ambiguous concerning the ethnographer's activity. Is she writing something down or looking something up? Are we witnessing the birth of a new, jotted text or a recourse to some notes that have been brought into the field, a prefiguration of what will count as important in the swirl of potentially meaningful discourse and activity? In the Anne Skinner-Jones photograph we cannot tell. Recent literary and textual theory argues that the ambiguity can, in fact, never be resolved. Inscription is both the making and remaking of texts. Writing is always to some degree rewriting. This is also the burden of Larcom's essay (1983), which analyzes her engagement with, simultaneously, the Mewun of Malekula and the unfinished texts of her predecessor in the field, A. B. Deacon. Larcom's essay portrays ethnographic fieldwork as fully historical: drawing on prior inscriptions to portray local customs over time *and* temporally situating its own interpretations of events and documents in an ongoing series. The critical and inventive use of prior written sources enmeshes ethnography in the history of ethnography. As Emiko Ohnuki-Tierney reminded us in her paper at the 1985 symposium, the rapprochement of ethnography and history in recent years diversifies the range of appropriate textual sources. The archive encroaches on the field; historical readings can no longer be seen as mere background for the essential work of firsthand discovery.[4]

[4]The latest convergence of history and anthropology has been widely discussed; see, among others, Cohn 1981; Davis 1981; Sahlins 1985; Thomas 1963; Wolf 1982. For the use of historical texts by anthropologists, see Evans-Pritchard's severe strictures (1971) on the Seligmans. "Ethnographic" topics and rhetoric have been adopted by social and cultural historians (see Rosaldo 1986), but as yet no systematic analysis exists concerning the differences and similarities of research *practice*, juxtaposing "the archive" with "the field"—seen both as textual, interpretive activities, as disciplinary conventions, and as strategic spatializations of overdetermined empirical data.

The belief in ethnography as an original production, a process of pure inscription most perfectly embodied in the fieldnote, is shaken. For of all the data used by fieldworkers, the texts created in the field have seemed most authentic, least tainted by prejudice. Fieldnotes embody cultural facts apparently under the control of their inscriber. Malinowski expressed the notion of originality a little too clearly, as usual, in his field diary (1967:140): "Feeling of ownership: it is I who will describe them or create them." But ethnographers can no longer claim this sort of originary or creative role, for they must always reckon with predecessors (and no longer only those most easily dismissed: missionaries, travelers, administrators). The field is more and more littered with "serious" ethnographic texts. One writes among, against, through, and in spite of them. This predicament undermines fieldnotes as the privileged empirical basis for a descriptive practice.

Indeed, one has, less and less, the illusion of control over the construction of any written corpus. Many literary analyses of intertextuality (e.g., Barthes 1970; Bloom 1975; Kristeva 1969) have made us confront the *un*originality of writing.[5] And recent studies of ethnography as a genre (Pratt 1986; Thornton 1983, 1985) bring out the many tropes it shares with unscientific, lay forms such as travel writing. Moreover, the originality of "primary" inscriptive practices has been challenged by theories of prefiguration and pre-encoding, most notably those of Hayden White.[6] Even to notice an event or fact, to find it important, White argues, is to presuppose some prior inscription or grid. The class of phenomena taken to be "the field" can be grasped—in sequence or separately—according to at least four modes of figuration: (1) as an image or pattern (metaphoric), (2) as a collection of empirical facts (metonymic), (3) as a hierarchical, functional, or organ-

[5]In Kristeva's words, "Every text takes shape as a mosaic of citations, every text is the absorption and transformation of other texts" (1969: 146).

[6]See esp. White's *Metahistory* (1973) and *Tropics of Discourse* (1978). Daniel Defert's analysis (1982, 1984) of *grilles de description* in early travel accounts identifies "obvious" units, or "natural" entities, which are projected prior to even the most detailed and accurate accounts. Thornton (1988) takes a similar approach to early ethnographies.

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ic whole (synecdochic), or (4) as a temporal, usually passing, reality (ironic). Kenneth Burke's four master tropes are here deployed to account for the dominant forms of historical narrative. White makes a strong claim that any historical or cultural "fact" can be registered as meaningful only by virtue of some prior code or figuration of the whole in which it belongs.

Robert Thornton (1988) makes an equally strong argument for the textual/rhetorical prefiguration of the facts in ethnographies that purport to describe social or cultural wholes. A classificatory rhetoric orders the most elementary items of behavior and experience included in the textual "corpus." (Thornton makes visible the commonsense metaphors of body, architecture, and landscape that underpin ethnographic co-constructions of text and society.) The most simple description, or even statistical counting, in the field presupposes that the items recorded are parts of larger social or cultural units whose imaginary configuration in terms of explicit or implicit wholes relies on rhetorical means.

Another account of the pre-encoding of facts has been offered by Johannes Fabian (1983). He argues that the differences constituting "us" and "them" in ethnography, a complex play of distances in each moment of inscription (visible in the photo of Joan Larcom), have been mastered and simplified in the form of an overriding *temporal* distance. "They" are placed in either a historical past or a mythic, oral (nonhistorical) condition. Fabian's critique makes us aware that every perception and inscription of an "event" implies a temporal positioning with political implications. Very concrete decisions of what to record in the field can follow from these prior assumptions. If one perceives an event—a performance or ritual—as a traditional survival, one may "naturally" exclude from one's data the modern, commercial, or evangelical forces that are everywhere in the culture but "peripheral" to the event. If, however, one sees the performance or ritual as emergent, predominantly located not in a past but in a possible future, modern things become interesting and will be much more prominent in one's corpus of inscriptions.

Of course, few ethnographers believe that the facts "speak" for themselves, or that the scientific observer merely collects or records them. But it is still widely assumed that inscription, the passage of experiential phenomena into writing, is at the origin of ethnography's more or less realistic descriptions. What I have said so far suggests that this is too simple a view of the writing, prefiguring, and remembering

that occur in the field. Inscription is intertextual, figurative, and historical all the way down to the most "immediate" perceptions.

Scene Two

Theorists who see ethnography as beginning with a process of inscription generally rely on Ricoeur's influential formulation (1971). Clifford Geertz gives a quick version in his introduction to *The Interpretation of Cultures,* an essay which I am rewriting here and to which I thus owe a great deal: "The ethnographer 'inscribes' social discourse; *he writes it down*. In so doing, he turns it from a passing event, which exists only in its own moment of occurrence, into an account, which exists in its inscriptions and can be reconsulted" (1973: 19; original emphasis). I have suggested, drawing on White, Thornton, and Fabian, that the very noting of an "event" presupposes a prior inscription. Moreover, my second scene of writing suggests further the limits of inscription as a model for what ethnographers do. The photograph of an ethnographer doing extended textual work with an indigenous collaborator reveals a kind of writing in the field that is often not a matter of catching "passing events" of social discourse as much as it is a process of transcribing already formulated, fixed discourse or lore. A ritual, for example, when its normal course is recounted by a knowledgeable authority, is not a "passing event." Nor is a genealogy. They are already inscribed. The same is true of everything paradoxically called "oral literature." A myth recited and taken down, a spell or song recorded in writing or on tape—these involve processes of transcription and explicit translation. I have suggested elsewhere the difference it makes when transcription and indigenous forms of writing are moved toward the center of ethnography (Clifford 1983: 135–42). For example, if writing in the field is not seen as beginning with inscription, then the ethnographic writer less automatically appears as a privileged recorder, salvager, and interpreter of cultural data. Greater prominence given to transcribed materials can produce a more polyphonic final ethnography. This effect already existed in the early works of Boas, Lowie, and others who, seeing their task as importantly philological, translated and commented on indigenous texts, many of them written by native "informants." (Even the term *informant* implies a story of inscription: "They tell me, I write it down.") The image of transcription (of writing over) interrupts the smooth passage from

writing down to writing up, from inscription to interpretive description. The authority of the researcher who brings passing, usually oral, experience into permanent writing is decentered.[7]

I do not mean to suggest, however, that transcription is an innocent, ethically superior, or nonauthoritative form of writing. It distributes authority differently. Authority is neither bad nor good in itself, but it is always tactical. It enacts power relations. The range of possible readings differs according to whether a cultural account presents itself as a description, for example, or as an exercise in philology. Fieldnotes, less focused or "cooked" than published ethnographies, reflect more diverse, often contested, contexts of authority. (This is perhaps one of the reasons why they have become interesting at a time like the present, when styles of scientific description and analysis are being intensely debated.) Fieldnotes contain examples of my three kinds of writing: inscription (notes, not raw but slightly cooked or chopped prior to cooking), description (notes sautéed, ready for the later addition of theoretical sauces), and transcription (reheated leftovers?). But the cooking metaphor, so tempting when it comes to fieldnotes, is inexact, because there are no "raw" texts. Transcription, which as a kind of copying appears to involve the least transformation, is in no way a direct or innocent record. The process may have the political effect of making canonical what is simply one telling of a myth or item of cultural lore. And transcription always raises questions about *translation*.

In a very acute essay, Talal Asad (1986) argues that the rather commonly invoked model of ethnography as translation hides the fact that cultures are not like coherent languages or texts but are composed of conflicting discourses. Moreover, the apparently neutral act of translating is enmeshed in global power inequalities. There are persistently "strong" and "weak" languages, he observes, and the vast majority of ethnographies are written in strong languages. Asad's analysis of how a strong language of ethnography overrides other languages adds a political dimension to our attention to fieldnotes.

The texts produced in the field are often polyglot. They include large quantities of the local vernacular plus diverse pidgins, shorthands, and languages of translation, along with the language or languages of the ethnographer. The final "written-up" ethnography smooths over the discursive mess—or richness—reflected in the fieldnotes. Is this inevitable? To a degree, yes. Who would want to read unimproved fieldnotes? But there are alternative uses and formats for these texts produced in the field. I have called attention elsewhere (Clifford 1986a: 15–17) to a recent series of publications from the University of Nebraska Press: the papers of James Walker (1980, 1982, 1983), who worked with the Lakota Sioux around the turn of the century. Thirty-eight Lakota "authorities" are listed at the back of the first volume, *Lakota Belief and Ritual*. Each section of the book is presented as the work of one or another of these authorities, interspersed with Walker's own notes and reflections. In the normal transition from fieldnotes to final ethnography, utterances tend to lose their individuated quality. Quotations from indigenous sources are often not given proper-name attribution, and even when they are, they merely serve to confirm or exemplify the ethnographer's general line. Two Crows is seldom heard denying things, as he more often does in contradictory, heterophonic fieldnotes.[8] Of course, vernacular expressions do appear in many ethnographies, according to protocols with which we are all familiar; for example, they often stand for problematic native "concepts." But we seldom encounter in published work any cacophony or discursive contradiction of the sort found in actual cultural life and often reflected in fieldnotes. A dominant language has overridden, translated, and orchestrated these complexities.

A culinary relapse: I am reminded of Roland Barthes's image of the sauce or glaze, the *nappe,* which in French cuisine smooths over and hides the productive, transformative processes of the cooking. Barthes makes this into an image for ideological, naturalizing discourse. I have the impression, as I try to find out about fieldnotes, that I can sometimes see through the *nappe* of the finished ethnography—beneath the unifying glaze, chopped meat.

Scene Three

Any systematic analysis of fieldnotes is hampered by the problem of access to a broad sample of texts. Moreover, individuals' reflections on

[7]I have analyzed critically this mode of authority, which identifies ethnography with a fraught passage from oral to literate, from event to text; see Clifford 1986b: 109–19. For a recent look behind the scenes of Boas's textual production which shows his Tsimshian collaborator, Henry Tate, "on a tightrope between oral and literary storytelling," see Maud 1989: 161.

[8]The issue of what to do with disagreeing, or heterophonic, Lakota voices was specifically confronted by Walker in writing up his fieldnotes for what would become his classic monograph, *The Sun Dance* (1917). In a revealing exchange of letters, Clark Wissler (of the American Museum of National History), urged Walker not to write too

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their own practice are limited in obvious ways.[9] The fullest published compendium of fieldnotes that I know is Geertz's *Religion of Java,* a work unusual to the extent that it is openly constructed from texts written during primary research. The book contains hundreds of indented passages identified as "transcriptions from the author's field notes" (1960: 15). These fieldnotes are largely of my third sort: composed, thick *descriptions.* Almost any example will give the flavor:

> We spoke about the difference between village and town patterns of *duwé gawé,* and she said the *buwuh* pattern was different. She said that the people on the Pohredjo row (this is the elite section of town, inhabited almost entirely by *prijajis*) wouldn't accept *buwuh.* They only accept gifts (called *cadeau,* following Dutch usage), and then they note down the price of the gift, and when the giver has a *duwé gawé* they return something of exactly the same value. [1960: 67]

The passage is indirect, summarized speech about custom, with parenthetical additions by the ethnographer, and this is a dominant mode throughout the book. The passage continues with a directly quoted interjection by the informant's brother, her own comments about how the exchange system doesn't work perfectly, and more parenthetical information about her class standpoint. The fieldnotes quoted in the book—often taking up as much as half the page—include a mixture of discursive positions and distinct viewpoints while maintaining, overall, a homogeneous tone.

Geertz provides an unusually specific appendix, which clarifies just how these notes were constructed and, to a degree, cleaned up for publication. Writing in the late 1950s, Geertz was far ahead of the field in textual self-consciousness. He would say things rather differently now, and it is unlikely that he would assert without hesitation, as he did then, that his book was "nothing more than a report," that his extensive use of fieldnotes was a way for the ethnographer "to get out of the way of his data, to make himself translucent so that the reader can see for himself something of what the facts look like and so judge

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ideal or unified an account of the sun dance. He made a subversive suggestion, not followed by Walker: "I often feel that the ideal thing would be to publish all the statements of informants together with an estimate and summary by the investigator" (Walker 1980: 29).

[9] Jean Jackson's interviews provide ample evidence of the highly personal, and often ambivalent, feelings of individual researchers to their own precious and flawed productions in the field.

the ethnographer's summaries and generalizations in terms of the ethnographer's actual perceptions" (1960: 7). But despite its sometimes too simple notions of transparency, this is one of the few ethnographies that give us a real glimpse of the making of cultural descriptions in fieldnotes. It embodies a kind of textual empiricism, rather different from Geertz's later position of textual interpretationism. If *The Religion of Java* does not provide us with a direct view of its author's "actual perceptions" in the field, it does offer an unusual, if partial, access to his construction of ethnographic facts.

Consider the book's first quoted fieldnote, which ends the short opening chapter. It is an ethnographic set piece sketching a typical *slametan,* the "simple, formal, undramatic, almost furtive little ritual" that lies "at the center of the whole Javanese religious system" (1960: 11). After setting out the "pattern" of events (when the ceremony is given, who cooks, who gets invited, what is chanted, how the food is distributed and received), Geertz then quickly elucidates the ritual's "meaning." He does this in a familiar ethnographic way, quoting and explicating the statements of unnamed Javanese. Sometimes he creates a collective persona, as in this definition of the ritual's psychic goal: "The wished-for state is *slamet,* which the Javanese defines with the phrase '*gak ana apa apa*'—'there isn't anything,' or, more aptly, 'nothing is going to happen (to anyone)'" (1960: 14). Then, at the end of a paragraph on Javanese beliefs about the omnipresence of spirits—against which *slametans* provide protection—the book's first indented fieldnote makes its appearance, introduced simply "As a Javanese put it."

> At a *slametan* all kinds of invisible beings come and sit with us and they also eat the food. That is why the food and not the prayer is the heart of the *slametan.* The spirit eats the aroma of the food. It's like this banana. I smell it but it doesn't disappear. That is why the food is left for us after the spirit has already eaten it. [1960: 15]

With this lucid and engaging statement, the chapter on *slametan* closes.

Like all direct extracts from fieldnotes the text "shows" the ethnography's representational data. In his paper at the 1985 AAA symposium, Michael Silverstein nicely analyzed this rhetorical function and added that rather like photographs in the text, quoted fieldnotes are "reality-close"; they have a "you are there" quality (for example, in the quotation above: "It's like *this* banana"). A reading of *The Religion of Java* which focused on its ways of establishing authority might see

the opening chapter as an elaborate staging of its final quotation. The last word on a "basic core ritual" (1960: 14) is given to a Javanese making an explicit cultural interpretation. This interpretation, presented *as a transcribed fieldnote,* associates the book's database with a direct access to the Javanese viewpoint. At the same time, the citation accomplishes a subtle fusion of native and ethnographic subjectivities in a common interpretive project. The passage, for all its "spoken" immediacy, is not surrounded by quotation marks. Geertz explains in his appendix (1960: 385–86) that such marks are reserved for more or less literal, or close, translations of things actually said. The passage in question is thus not an exact rendering but in some degree a reconstruction. It is an enunciation neither *by* a specific Javanese nor *by* Clifford Geertz; it falls somewhere between direct and indirect discourse, accomplishing a rhetorical fusion of viewpoints. It is the enunciation of an ethnographic persona speaking *cultural* truths.

The passage, endowed with both the personal presence of speech and the empirical function of a fieldnote, is an enunciation of *Javanese* knowledge. It does what any "good" ethnographic interpretation does, making a difficult custom or belief concretely comprehensible. Geertz chose it in part, certainly, for this reason: to show that his empirical data was a record not only of his observations but also of indigenous interpretations. Later he would explicitly argue that cultural facts are always already interpretations (Geertz 1973: 3–30). Moreover, since culture is prefigured as a complex but coherent whole, Javanese interpretations will not systematically contradict those of the ethnographer of Java. Geertz will account for all the interpretations he chooses to quote in *The Religion of Java.* And as we have seen, Javanese direct statements will, in their constitution as fieldnotes, have already been selected, focused, contextualized as "cultural" enunciations.

The book regularly presents its informants as interpreters giving lucid explanations of their beliefs and acts, sometimes with a laudable cultural relativism: "I don't know how it is in America, but here . . ." (1960: 14). Moreover, as in the first fieldnote quoted above, the research process is continually made manifest: "I asked her," "she said," then "he said," then a parenthesis on her personal background, and so forth. One might object that Geertz's notes smooth over a great deal, that they do not contain much on the ethnographer's subjective states, that reported interpretations seldom conflict radically, that a certain "ethnographic" tone suffuses all the purportedly individual voices. But how many ethnographies (let alone those written in the late 1950s, at the height of American social-scientific positivism) can satisfy such objections? What makes the fieldnotes selected for inclusion in *The Religion of Java* especially useful for my present purpose is the variety of ways in which they show cultural interpretations being constructed as fieldnotes. Javanese discourses and those of the ethnographer (descriptions, translations, contextual comments) are fused or, better, *orchestrated* to produce rich descriptions. Geertz's fieldnotes may be "thicker" than most. But the kind of selecting, narrating, contextualizing, and translating visible in them is in some degree practiced by any ethnographer who sits down to record and begin to make cultural sense of a busy day's impressions.

Travels with a Typewriter

Geertz's fieldnotes are, of course, anything but "raw." He tells us in his appendix (1960: 385) that they were carefully typed up every day or so. A short essay could be written about typewriters in the field (and soon, perhaps, one on word processors). There are intriguing glimpses in print. When Jean Briggs (1970) is ostracized by her Utku Eskimo hosts, she finds solace in her typewriter. Geertz represents the ethical ambiguities of fieldwork through a struggle over a typewriter with a Javanese informant (1968: 152–55). Colin Turnbull reveals somewhere in *The Forest People* (1961) that he has the machine with him (forcing us to reimagine his Mbuti villages, adding to the calm suffusion of forest sounds the tap-tap of fieldnotes in the making). To illustrate my third scene of writing I almost chose the famous photo that appears on the cover of this volume: Mead and Bateson in the Iatmul "mosquito room," facing each other from behind separate typewriters.

This moment of initial ordering, the making of a neat record (whether in type or script), must be a crucial one in the fieldwork process. "Good data" must be materially produced: they become a distanced, quasi-methodical corpus, something to be accumulated, jealously preserved, duplicated, sent to an academic advisor, cross-referenced, selectively forgotten or manipulated later on. A precious, precarious feeling of control over the social activities of inscription and transcription can result from creating an orderly text. This writing is far from simply a matter of mechanical recording: the "facts" are selected, focused, initially interpreted, cleaned up.

Most writing is sedentary activity. Unlike storytelling, it cannot be

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done while walking along a path. The turn to the typewriter involves a physical change of state, a break from the multisensory, multifocal perceptions and encounters of participant-observation. Writing of this sort is not "situated" like discourse or an oral story, which includes—or marks in the performance—the time/space of the present moment and audience. Rather, the present moment is held at bay so as to create a recontextualized, portable account. In crucial respects this sort of writing is more than inscription, more than the recording of a perception or datum of "evidence." A systematic reordering goes on. Fieldnotes are written in a form that will make sense elsewhere, later on. Some may even, like the notes included in *The Religion of Java*, pass directly into a published book. Turning to typewriter or notebook, one writes for occasions distant from the field, for oneself years later, for an imagined professional readership, for a teacher, for some complex figure identified with the ultimate destination of the research. Facing the typewriter each night means engaging these "others" or alter egos. No wonder the typewriter or the pen or the notebook can sometimes take on a fetishistic aura.

As we have repeatedly seen, fieldnotes are enmeshed in writing and reading that extends before, after, and outside the experience of empirical research. A fundamental question emerges. "The field," seen as a place of writing, leaks. Once one complicates and historicizes the "notes" in "field/notes," the boundaries of the first term, "field," begin to blur. How is the field spatially and temporally defined? Can one, properly speaking, record a field note while not physically "there"? Would a remembered impression first inscribed at one's home university count as a fieldnote? Or, what about a "thick description" written not at the site of research but while sojourning in the capital city of the host nation? Fieldnotes are by definition written "in" the field. But with increased coming and going, better global transport and mobility, where does the field begin and end? Indeed, the very identity of "fieldnotes" as a discrete corpus depends on a spatialization more and more difficult to maintain, a historically specific set of distances, boundaries, and modes of travel. As the historical and political relations of different parts of the planet shift, as cultures interpenetrate, and as ethnography turns back on its own culture, "the field" becomes more and more evidently an ideal construct.

It would be useful to trace a genealogy of the term "field," as used to designate a site of professional activity. While this is beyond my present scope, it is worth mentioning a few points of departure (Bertrand Pullman [1988] develops some of them in his analysis of the French term *terrain*). In various Western discourses "field" is associated with agriculture, property, combat, and a "feminine" place for ploughing, penetration, exploration, and improvement. The notion that one's empirical, practical activity unfolds in such a space has been shared by naturalists, geologists, archaeologists, ethnographers, missionaries, and military officers. What commonalities and differences link the professional knowledges produced through these "spatial practices" (De Certeau 1984)? What is excluded by the term "field?" The modern traveler, unlike the ethnographer, has no field, only a route; no body of classified data, only a narration. The primary "descriptions" of travelers are recorded in journals, not fieldnotes. How have these generic and professional differences been constituted and maintained? How has one set of practices come to be coded "objective," the other "subjective?" Such questions open up a larger domain of research concerned with the history of Western modes of travel, occupation, and dwelling. Within that general history professional ethnography appears as a particular, contested, spatial practice.

Arjun Appadurai (1986: 337) has raised similar spatial/historical questions with regard to the articulation of theory.

> At least since the latter part of the nineteenth century, anthropological theory has always been based on the practice of going *somewhere*, preferably somewhere geographically, morally, and socially distant from the theoretical and cultural metropolis of the anthropologist. The science of the other has inescapably been tied to the journey elsewhere. But the question of what kind of elsewhere is tied in complicated ways to the history of European expansion, the vagaries of colonial and postcolonial pragmatics, the shifting tastes of Western men of letters. In turn, changes in anthropological theorizing, influenced in ill-understood ways by these shifting loci of investigation, have themselves influenced fashions in anthropological travel. Places (i.e., particular areas, locations, cultures, societies, regions, even civilizations) are the objects of anthropological study as well as the critical links between description and analysis in anthropological theory.

The issues raised here are far-reaching and will require, as Appadurai has said, considerable development.[10] I can only suggest, in a passing

[10] Appadurai organized a session on place in anthropological theory and practice at the December 1986 meetings of the American Anthropological Association. Many of the papers presented there appeared in *Cultural Anthropology* 3 (February 1988).

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way, how they impinge on the *topos* of fieldnotes. Appadurai's crucial point is that description and analysis are systematically linked (and distinguished) by specific historical *spatializations*.

From this perspective, a corpus called *field*notes serves the function of reifying and naturalizing a "place" to be kept separate from the various operations of theorizing, fictionalizing, and writing up that conventionally occur elsewhere. The largely unexamined distinction between "fieldnotes" and other forms of ethnographic writing (the intimate journal; or letters home; or more openly analytic, interpretive, or explanatory styles of writing) serves to constitute and protect a bounded "object" of study, a collection of textualized cultural facts that will serve as a fairly stable base for interpretation and theorizing even long after the field research has been accomplished. This spatially defined corpus resists the historicity of the long-term writing and rewriting processes involved in making an ethnography. Once recognized, however, the inescapable temporality of writing and rewriting unravels synchronic spatializations. And it blurs conventional frontiers separating, for example, "fieldnotes" from "writing up."

The problematic corpus, the disciplinary convention "fieldnotes," tends to dissolve into more general processes of writing—inscription, transcription, and description. And as one questions the specificity of writing done in "the field," one is led to confront the ways a cultural science defines and maintains its objects of study. I have suggested that ethnography—a practice fused, after the 1920s, with academic fieldwork—has tended to construct its object as something to be *described*. There are alternatives. A dominant paradigm of *transcription* (closer to the practice of Boas or Lowie, for example) constructs the other philologically, as a collection of discourse requiring translation and exegesis.[11] Or an ethnography less concerned to separate itself from "subjective" travel writing might adopt an openly *inscriptive* stance, registering the circumstantial situations of a perceiving, interpreting subject, noting events and statements as part of a passing sojourn of research. (Indeed, many recent autobiographical, reflexive, ethnographies can be seen as signs of a rapprochement between ethnographic and travel genres.) I have argued that all three modes of writing are active in fieldwork. But they have been hierarchically organized, under a dominant rhetoric of description, in ways that are now in question.

[11]The Walker collections mentioned above are recent examples (see also Evers and Molina 1987). For an ethnography (written by an anthropologist and a linguist) which combines description with extensive textual exegesis, see Bensa and Rivierre 1982.

Toward a Decentering of Description

The fieldnotes cited throughout *The Religion of Java* are typed-up, constructed, and written-over "descriptions." Actually, they contain little description in the strict sense. (Description is a specific, rather uncommon, form of writing.)[12] But their overall effect is descriptive: they select and foreshorten perceptions and statements in ways that constitute an objective, uncontested world of interpretations, indigenous and scientific. In the process, interpretations cease to be primarily debates, dialogues, transcriptions, or circumstantial inscriptions. I have argued that the construction of "thick" cultural descriptions involves a *turning away* from inscription and transcription to a different form of writing. The photo of Malinowski stages rather precisely this moment of turning away from encounter, speech, participation, and observation toward the writing table, the notebooks, the typewriter. A crucial line—in the photo, the shadowy threshold between the tent's inside and outside—must be maintained, crossed and recrossed. Various rituals and conflicts surround this transition. And as Jean Jackson's survey confirms, the turning toward solitary writing can be the focus of strongly ambivalent feelings: "It takes you away from the action" or "It keeps you from going native."

The process of field research is potentially endless. One can never have enough conversations, learn the language well enough, grasp all the "hidden" and emergent domains of indigenous life. Yet one must arrive at some baseline or adequate corpus of facts. The writing of descriptive fieldnotes, "good" data oriented toward a coherent cultural object, provides a body of knowledge prefigured for theoretical development. This textual (portable and permanent) corpus offers a conventional "empirical" ground, or starting point, in a situation where, as Geertz intimates, "it's interpretations all the way down" (1973: 29).

But descriptions are not merely interpretations. They are *written* rhetorical constructions. A fieldnote featured by Geertz (1973: 7–9) in his influential essay on "thick description" provides a particularly clear example: the story of Cohen the Jewish merchant in French colonial Morocco leading a raid against maurauding Berbers and claiming five hundred of their sheep as an indemnity. An ironic colonial tale, replete with Conradian touches (the French captain says to Cohen: "If you get killed, it's your problem!"), the tale is presented as a "not-untypical"

[12]See particularly the work of Hamon (1981) and Beaujour (1981).

excerpt from Geertz's field journal. Its composed, narrated quality is patent. And it is, one assumes, derived from interlocution, narration, and rewriting. The events take place in 1912, their source an unnamed "informant." The field journal excerpt—"quoted raw, a note in a bottle"—brings us to *see* the events. For example, after the conflict is settled, a sharply etched scene:

> The two armed Berber groups then lined up on their horses at opposite ends of the plain, with the sheep herded between them, and Cohen, in his black gown, pillbox hat, and flapping slippers, went out alone among the sheep, picking out, one by one and at his own good speed, the best ones for his payment.

Here is description. But who saw this scene? Not Cohen. The "informant"? *His* informant? Or, as I suspect, the ethnographer as he sat at his writing table, pulling together jottings, memories, transcriptions of the account (or accounts) he heard?

Geertz cites this "fieldnote"—obviously complex and literary—to show that ethnographic data are always constructions of other people's constructions ("winks upon winks upon winks"). His point is important and trenchant. But Geertz's well-known formula for ethnography, "thick description," is more ambiguous. It can either be read as an oxymoronic critique of the very notion of description ("interpretations all the way down") or be taken as a charter for an interpretive science (which describes, with hermeneutic complexity, a cultural object). By associating ethnographic construction with description, however thick or problematic, Geertz limits a possibly far-reaching critique. For description inevitably suggests a specular, representational relation to culture. I have argued that such a relation is always rhetorically (also historically and politically) mediated. Ethnography cannot, in practice, maintain a constant descriptive relationship to cultural phenomena. It can maintain such a relationship only to what is produced in fieldnotes, and especially in the most "focused" products of writing in the field, those of my third scene. Other forms of writing, inscriptive and transcriptive, may register quite different relationships to the people, discourses, and events studied in fieldwork. One form of ethnographic writing, description, has too often been made to stand for the entire ethnographic process. But whether it is writing down, writing over, or writing up, the work of ethnography is intertextual, collaborative, and rhetorical. It is possible to be serious, truthful, factual, thorough, scrupulous, referential—without claiming to be describing anything.

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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 Cushman 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 reflexive 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 Merrill, Hilly Geertz, Roger Sanjek, and Julie 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 1987).