Fieldnotes

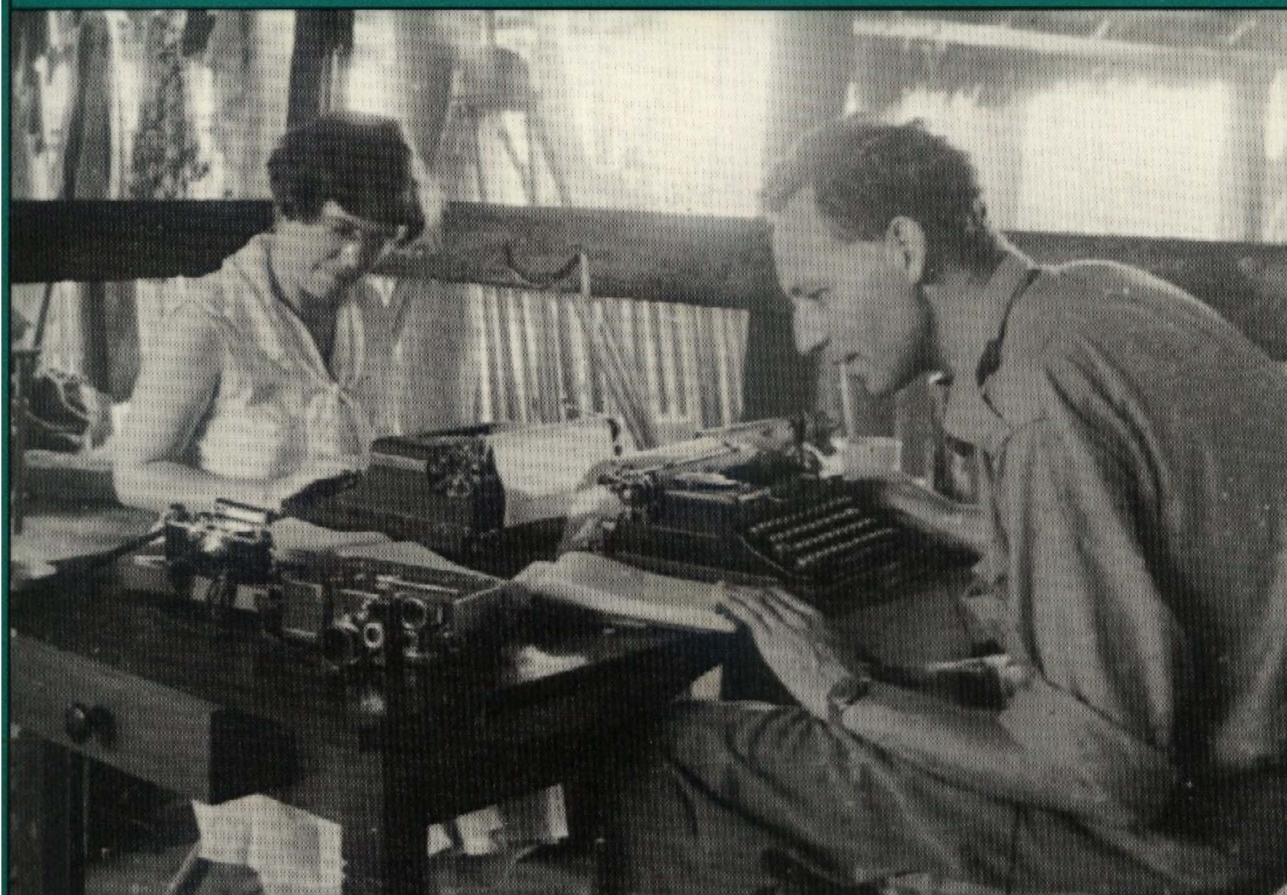## THE MAKINGS OF ANTHROPOLOGY

EDITED BY Roger Sanjek

FIELDNOTES

The Makings of Anthropology

Edited by ROGER SANJEK

CORNELL UNIVERSITY PRESS

Ithaca and London

ROGER SANJEK

·A Vocabulary for Fieldnotes

Anthropologists often characterize themselves as mavericks and individualists, holding an "I did it my way" attitude about fieldwork, as Jean Jackson confirmed in several of her interviews. Despite this iconoclastic "Indiana Jones syndrome," as she calls it, there is considerable order and pattern in the ways anthropologists operate, more than many may wish to believe. Patterns in fieldnote practice have changed from the 1880s to the 1980s, as I show in "The Secret Life of Fieldnotes" (in Part III). But first we need to establish a vocabulary for the discussion of fieldnotes.

"What are fieldnotes?" George Bond asks (this volume). He answers that they are first, certainly, texts; they are documents with "the security and concreteness that writing lends to observation . . . immutable records of some past occurence." Yet fieldnotes are written, usually, for an audience of one. So they are also "*aides-mémoire* that stimulate the re-creation, the renewal of things past," Bond explains. Fieldnotes can make difficult reading for anyone other than their author, as Robert J. Smith discovered in his first reading of Ella Lury Embree's fieldnotes about the Japanese village of Suye Mura. Fieldnotes are meant to be read by the ethnographer and to produce meaning through interaction with the ethnographer's headnotes.

"Headnotes," the felicitous term coined by Simon Ottenberg, identifies something immediately understandable to ethnographers. We come back from the field with fieldnotes and headnotes. The fieldnotes stay the same, written down on paper, but the headnotes continue to evolve and change as they did during the time in the field. Ethnography, Ottenberg explains, is a product of the two sets of notes. The headnotes are more important. Only after the anthropologist is dead are the fieldnotes primary.

Other anthropologists have written about headnotes without using the term (Davis 1984: 304–5; Ellen 1984b: 279; Holy 1984: 33; Van Maanen 1988: 118). On her third visit to Manus in 1965, Margaret Mead was struck by the importance of her headnotes: "Because of my long acquaintance with this village I can perceive and record aspects of this people's life that no one else can. . . . It is my individual consciousness which provides the ground on which the lives of these people are figures" (1977: 283).

Niara Sudarkasa (Gloria Marshall), while working in another field site, wrote a rich account of her 1961–62 fieldwork in the Yoruba community of Awe. Her fieldnotes, diaries, and letters remained at home; only her dissertation and a few photographs were with her. "What follows, therefore, might best be described as remembrances of, and reflections upon, my efforts as an anthropologist in the making. These are the encounters, the evaluations, the episodes that are chiseled in memory" (Marshall 1970: 167). She relied on her headnotes.

Martin M. C. Yang's 1945 classic, *A Chinese Village,* was written from headnotes alone. In China during 1931 he drafted a paper about his home community which was later published. Still later,

early in 1943 Ralph Linton invited me to work on a project entitled "The Study of Modern Chinese Rural Civilization" in the department of anthropology at Columbia University. . . . The project, which lasted about sixteen months, resulted in my writing *A Chinese Village.* . . . In my imagination I almost completely relived my boyhood and adolescent years. I did not merely recall facts or occurrences, but mentally and emotionally retraced my role in the life of the community. All came back to me—my parents, brothers, sisters; the people of adjacent neighborhoods, of the village, the market town, the market-town school;

their personalities, lives, and work; their relations with each other.
[Yang 1972: 71–72]

Srinivas wrote *The Remembered Village* also primarily from head-
notes. And like Yang, but more extensively, he had done earlier
writing about Rampura (see Srinivas 1987 for several of these papers).
A. C. Mayer raised the question about Srinivas's book:

> Has not that memory been "mediated" by diary-writing and note-
> taking . . . by the later "processing" of the field notes, and for some of
> the data, by the writing up in articles? . . . The question is, then: how far
> was Srinivas able to forget his field notes and other writings? . . . He
> may have had his memory "shaped" by these other data, in much the
> same way, though to a much lesser extent, as might the person working
> openly with notes in an orthodox way? . . . Perhaps, then, Srinivas has
> not so much used a new method of providing ethnography . . . as varied
> the mix—of memory and written aids—in the usual one? [Mayer 1978:
> 43–44]

Mayer is correct, of course. Srinivas's headnotes of 1970, his memories
at the time he wrote the book, were different from the headnotes
formulated in Rampura at the time of his fieldwork in 1948 and 1952.
All the episodes of writing and thinking about Rampura between these
points in time affected the headnotes and led to *The Remembered Vil-
lage*.

Several of the authors in this volume comment on the headnotes-
fieldnotes relationship. Jean Jackson mentions that for many anthropol-
ogists, changing topical interests and theoretical orientations "make re-
reading fieldnotes an eye-opening experience." Margery Wolf writes
that feminism brought new questions to the fieldnotes she and Arthur
Wolf had produced in Taiwan. Nancy Lutkehaus's post-fieldwork
headnotes provoked a reading of Camilla Wedgwood's Manam Island
fieldnotes different from that preceding Lutkehaus's residence there.
Rena Lederman considers extensively the tensions between fieldnotes
and the evolving "sense of the whole," both during and after fieldwork.
George Bond concludes, "When we review our notes we fill in gaps,
we give order to the immutable text."

The Field and Writing

Fieldnotes are produced in the field, but where is the field? Clifford
asks: "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?" And what of the increasing number of anthropologists who do fieldwork "at home," often in their home communities?

Lederman offers an answer. Being "in the field," she says, "need not involve any traveling at all: it sometimes simply involves a shifting of attention and of sociable connection within one's own habitual milieus." Fieldnotes are "of" the field, if not always written "in" the field.

But *what,* physically, are they? Anthropologists bring back a variety of objects from fieldwork, including much paper. Jackson found no defining consensus on what to include; notes on readings, photocopied archival material, a ceramic dish, even the ethnographer her- or himself ("I am a fieldnote," stated one storer of headnotes)—all were considered fieldnotes by some. Anthropologists also bring back photographs, films, videotapes, audio recordings, and recovered documents of many sorts, including informant letters or diaries.

Here our focus is on what the anthropologist *writes* in the field: " 'What does the ethnographer do?'—he writes" (Geertz 1973: 19). We shall identify scratch notes, fieldnotes proper, fieldnote records, texts, journals, diaries, letters, reports, and papers written in the field (cf. Davis 1984: 297–304; Ellen 1984b).[1] We will briefly discuss also taped interviews and informant statements, which are often transcribed outside the field but then become written documents used in writing ethnography, like field-produced fieldnotes.

Scratch Notes

For many anthropologists, a first step from field perception to paper is handwritten "scratch notes," to use another of Ottenberg's well-chosen phrases (cf. Ellen 1984b: 279–80, 282). Scratch notes are sometimes produced in the view of informants, while observing or talking with them, and sometimes out of sight.

William Partridge, in Colombia, felt uncomfortable carrying a notebook early in his 1972–73 research, but with time he was able to record

---

[1] Ottenberg's and Clifford's essays guided my analysis of the fieldwork literature. I read Ellen's edited volume (1984a) after writing the first draft of "A Vocabulary for Fieldnotes." All of our views of fieldwork writing are gratifyingly coincidental, even if we, or other authors in this volume, do not always use the same terms for conceptualizing different types of field writings. I wish to acknowledge the published priority of Ellen's typology (1984b) and of Davis (1984).

notes in front of his informants (Kimball and Partridge 1979: 52, 171). Lederman always carried a steno pad; sometimes she wrote fuller notes as people were talking, and at other times she reconstructed her observations later, from "abbreviated jottings" on the pads. In outdoor observation among the Skolt Lapps in 1958–59, Pertti Pelto was often prevented by cold weather from producing more than bare scratch notes (1970: 265–66). Edward Norbeck, in Japan in 1950–51, choosing to "devote as little time as possible to writing while in the presence of informants," produced his scratch notes afterward; during long interviews he often excused himself "to go to the toilet, where I hastily jotted down in Gregg shorthand key words to jog my memory later" (1970: 255).

Morris Freilich, in 1956 research among Mohawks in Brooklyn and Canada, soon learned that open note-taking would not be tolerated: "[I] had to keep a small notebook in my hip pocket and periodically go to the men's room in the bar or the outhouse at Caughnawaga and write notes to myself. As frequently as possible, I would go to a coffee shop to write down longer statements" (1970b: 193. See also Gupta 1979: 113; Keiser 1970: 230). William Sturtevant (1959) even published a short statement about his technique of writing scratch notes unobserved during long ceremonial events: he used a two-inch pencil on two- by three-inch slips of paper held together by a paperclip in his pants or jacket pocket. Some of Hortense Powdermaker's fieldnotes in Mississippi were written with similar surreptitiousness (1966: 175, 178).

Scratch-note production is what James Clifford calls *inscription*: "A participant-observer jots down a mnemonic word or phrase to fix an observation or to recall what someone has just said." It might also record fuller observations or responses to questions the ethnographer brings. Either way, as Clifford observes, "the flow of action and discourse has been interrupted, *turned* to writing." For some of Jackson's anthropological informants, inscription disrupts participant-observation: "Fieldnotes get in the way. They interfere with what fieldwork is all about—the doing."

Inscribing scratch notes, usually on a small pad contemporaneous with or soon after the events observed or words heard, *is* anthropological fieldwork (Boissevain 1970: 74–75, 79; Freilich 1970b: 200–201; Gonzalez 1970: 171; Gulick 1970: 133–34; Kobben 1967: 42; Marshall 1970: 190; Powdermaker 1966: 94–95; Whitten 1970: 351; Yengoyan 1970: 416). But so is the "typing up" Ottenberg speaks of, the production of an enhanced and expanded set of fieldnotes (see Beals 1970: 50;

Beattie 1965: 41; LeClair 1960: 34–35; Marshall 1970: 190; Powder-maker 1966: 95; Wolff 1960: 241).

Scratch Notes to Fieldnotes

This second stage of fieldnote production is epitomized in the photo-graph on the cover of the paperback edition of this book, Margaret Mead and Gregory Bateson at work in "the mosquito room" in the Iatmul village of Tambunam in 1938. They sit opposite each other at a small desk, each behind a typewriter. Bateson is looking to his left at a small notebook, his handwritten scratch notes. Mead, her notebook to her right, next to Bateson's, is either reading her typewritten page or thinking. They are busy in *description*, as Clifford characterizes it: "the making of a more or less coherent representation of an observed cultural reality . . . for later writing and interpretation aimed at the production of a finished account."

The scratch-notes-to-descriptive-fieldnotes writing act must be timely, before the scratch notes get "cold" (Mead 1977: 202). But more than preserving their warmth is involved. As Ottenberg notes, other ingredients are added in the process. Aneeta Minocha, whose circum-stances of field research in a women's hospital in Delhi made taking scratch notes relatively easy, is precise about her additions in writing second-stage descriptive fieldnotes.

> During my talks I scribbled key words on a small notebook. Later I wrote extensive reports of my conversations, and also recorded my explanations and interpretations as they occurred to me at that time. I also recorded the contexts in which particular conversations took place, as well as the general physical and emotional condition of the infor-mants, their appearance and behavior, and the gestures they used. Usually it took me three to four hours to put on paper five to six hours of field work. It was because of such immediate recording of my field experiences that I was able to recreate the atmosphere in which each conversation or event took place. Even now, as I write, I can vividly feel the presence of the participants. [1979: 213]

John Gulick, in a Lebanese village in 1951–52, used brief scratch notes in conjunction with his memory of conversations to produce his fieldnotes.

> Often . . . I would have to wait until the evening to do this, and tired though I usually was at the end of the day, I found that it was essential to write the day's notes before going to sleep. If I failed to do this and

postponed note writing till the next day, I found that the notes were useless, except insofar as they might contain simple factual information. The subtleties of cues and responses—some of which one can catch in notes if one writes them soon enough—became lost in sleep, and what I wrote the next day was essentially a second-hand account, an over-simplified version, in which the events and my reactions to them were truly blurred. [1970: 134]

Other anthropologists may handwrite fuller, longer-lasting, scratch notes (Powdermaker 1966: 95), though these also vary in completeness from one time to another (Beals 1970: 55; Honigmann 1970: 44; Wagley 1977: 18). Few are as candid about the compromises they make as Pelto:

My plan was to type up the day's field notes each evening, or, at the latest, the next morning. However, I was frequently at a roundup or other activity for as long as two weeks at a time, which meant that on returning to home base I would have to schedule lengthy typing sessions to catch up on back notes. While typing up my notes, I often recalled significant events that I had not jotted down in my notebook. I wrote up these additional notes in the same manner as the information from the notebook, although the nature of the materials often made it clear which data had been written on the spot and which were later recollections. [1970: 266]

A backlog of scratch notes to be typed plagues more anthropologists than Pelto—probably most anthropologists (see Briggs 1970: 33; LeClair 1960; Powdermaker 1966: 170). When possible, some ethnographers take short periods away from their fieldwork location to catch up on processing their scratch notes (Norbeck 1970: 25; Shah 1979: 32). Mead comments on the pleasure that being caught up brings, if only momentarily: "For the first time in two months I am almost up to date in writing up notes, which is the nearest I can ever come to affluence. It's impossible to get on the credit side of the matter, but just to be free of the knowledge that there are pages and pages of faintly scratched, rapidly cooling notes waiting for me is almost affluence" (1977: 228–30).

The disposition of scratch notes is probably the wastebasket in most cases. Ottenberg kept his for some years, then threw them out. Norbeck apparently kept his longer. He wrote in 1970 about his fieldwork in Japan: "My handwritten field notes consisted of two very slim notebooks more or less filled with cryptic symbols. My typewritten

notes consisted of a file of 5 by 8 inches equal to perhaps 2000 manuscript pages. The slim notebooks contained . . . the basis for typing lengthy accounts" (1970: 256).

Fieldnotes Proper

When Solon Kimball arrived in West Ireland in 1933, it had been "drilled" into him that success "would be evident in fat piles of field notes" (1972: 183). The "lengthy accounts" brought back from the field—Norbeck's 2,000 cards, for example—are the heart of our concern with fieldnotes. It is this body of description, acquired and recorded in chronological sequence, that I shall term "fieldnotes proper," though others have different names for it: "journal," "notebooks," "daily logs." Scratch notes precede fieldnotes, and other forms of writing in the field are arranged around them.

At the core of the more specialized fieldnote records and journal from Margery and Arthur Wolf's 1958–60 research in Taiwan are, on five- by eight-inch cards, "some 600 closely typed pages of what we came to call G data, or general data. These notes include detailed descriptions of funeral ceremonies, intensive interviews with unhappy young women, lengthy explanations by village philosophers, and rambling gossip sessions among groups or pairs of women and men." Simon Ottenberg's 1952–53 Afikpo fieldnotes are similar—"a thicket of ethnography." Rena Lederman's New Guinea "daily logs" were handwritten, from her steno-pad notes, in chronologically kept bound books: "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."

Nancy Lutkehaus and Robert Smith, coming across other ethnographers' fieldnotes, have found in them the properties and problems that Wolf, Ottenberg, and Lederman ascribe to their own. Following Malinowski's advice to produce "a chaotic account in which everything is written down as it is observed or told," Wedgwood kept her 1933–34 fieldnotes in "thirty-four neatly bound notebooks" that record "observations of daily activities, genealogical data, fragments of texts with interlineal translations, narrative descriptions of events and processes, and drawings diagramming such things as house construction and the various parts of an outrigger canoe" (Lutkehaus, this volume). Among the Suye Mura field materials were "two typescript journals. John Embree's contained 1,276 pages; Ella's 1,005." Ella Embree, reports

Smith, "wrote down what she had seen and heard, and often what she thought about it, at the end of every day. The journal . . . begins on December 20, 1935, and ends on November 3, 1936. The difficulty was that increasing familiarity led the journal's author to use short-hand references to individuals and places."

Allen and Orna Johnson (this volume) suggest solutions to the problems of unevenness and haphazard organization that may characterize comprehensive fieldnotes. They also point out, provocatively, that the "interpretive" and "scientific" camps of contemporary anthropology have had little to say about the implications of their positions for the fieldnotes that anthropologists produce: "We suspect that both humanistic and scientific anthropologists keep their journals in roughly comparable ways. . . . Open discussion of our fieldnotes . . . might reveal more similarities between varieties of anthropologists, illuminating the bases that link us as a unified profession."

Whether in handwritten bound books or typed on five-by-eight cards or full-sized typing paper ("I . . . use the best rag-content paper" [Mead 1977: 11]), a substantial corpus of sequentially produced, wide-ranging fieldnotes is at the heart of the ethnographic enterprise (Barnett 1970: 4–5, 28; Boissevain 1970: 79, 81; Ellen 1984b: 283; Fenton 1972: 109; Gulick 1970: 133, 134; Honigmann 1970: 40; Wolcott 1981: 256; Wolff 1960: 241). Extracts from such fieldnotes have been published in several books discussing fieldwork (see Boissevain 1970: 75; Conklin 1960: 119–25; Freilich 1970b: 197–98; Kimball and Partridge 1979; Kobben 1967: 37–38, 43–47, 50, 53–54; Mitchell 1978: 101–3, 107–8, 160, 172–76, 185, 232–33; Wagley 1977: 90–93; Whiting and Whiting 1970: 293, 299–311).

Fieldnote Records

Some of Jean Jackson's anthropological informants contrasted "fieldnotes," in the sense of "a running log written at the end of each day," with "data." For these ethnographers, fieldnotes are "a record of one's reactions, a source of background information, a preliminary stab at analysis." Data, for them, are sociological and demographic materials, organizable on computer cards or disks.

The Johnsons point to the differences in design and use between fieldnotes and more specialized field materials—both the "question-naires and surveys" of quantitatively oriented anthropologists and the "folktales, life histories, or taxonomies" of the humanistically in-

clined. Robert Maxwell (1970: 480), reviewing his 1964 research in Samoa, distinguished "thesis-relevant information" ("tests and systematic observations that provided me with enough data for a dissertation") from "soft data" (his fieldnotes, recorded on 1,500 five-by-eight cards, concerning "the sociological characteristics of the village, the dreams of the inhabitants, . . . general information on the way people in Laovele pattern their lives," and a mass of details on the lives of two individuals).

In an organizational sense, these contrasts are between fieldnotes proper and fieldnote records—information organized in sets separate from the sequential fieldwork notes that anthropologists produce (Ellen 1984b: 286). While Jackson and the Johnsons identify a strain of contemporary anthropological thinking in which fieldnote records, or "data," are a more important goal than wide-ranging fieldnotes, and Maxwell provides an example, the point here is larger than "scientific" models of fieldwork.[2] Records, as the Johnsons note, are produced by all brands of anthropologists; this was the case for many decades before anthropology became a "behavioral science" in the 1950s.

In addition to the two sets of fieldnotes totaling more than two thousand pages from the Embrees' fieldwork in Japan, Smith was presented with their household census records, along with documents, letters, reports, photographs, and an informant's diary. The records from Margery and Arthur Wolf's 1958–60 Taiwan research were even more extensive: thousands of pages of timed observations of children, hundreds of pages of formal interviews of children and parents, and hundreds of questionnaires administered in schools, all in addition to their "G data" fieldnotes.

Other extra-fieldnote records that anthropologists have mentioned in accounts of fieldwork include household data cards, genealogies, and folders for information on "certain persons . . . and subjects such as kinship, godparenthood, church organization" (Boissevain 1970: 75, 77–78, 80); a list of personal names and their meanings, informant comments on a set of photographs, questionnaires, life histories, and a day-by-day record on political developments "in which every conversation, rumor and event was kept" (Codere 1970: 157–61); forms for data on knowledge of plants and animals and on material culture, and a

---

[2]Ottenberg writes in a personal communication, "There is a danger for some persons of overemphasizing records at the expense of fieldnotes. We had an ethnomusicology student who in his research did great work with the video camera but it so preoccupied him that he had few written notes."

World Health Organization form on household composition and pos-sessions, economics, and health and nutrition (Dentan 1970: 95–96); a questionnaire on values and the Thematic Apperception Test (TAT), both adapted for local use (Diamond 1970: 138–39); topical notes on "change, children, communication, co-operatives, dances, employ-ment, interpersonal relations, law, leadership, marriage, personality and recreation" and a "data bank" on individual community residents (Honigmann 1970: 40, 66); and Rorschach tests, a comprehensive "sociocultural index schedule," and an "expressive autobiographic interview" (Spindler and Spindler 1970: 280–82, 285, 293–95).

As these accounts explain, some fieldnote records are envisioned in "research designs" before fieldwork, and others are developed as the research progresses. Lederman carefully explains the evolution of her "daily log" fieldnotes and "typed files" records, and the relationship between them. Her records, kept according to topic in ring binders, included accounts of complex events, long interviews, a household census, land tenure histories, data on garden plots and pig production, gift exchange account books, and systematic interviews on exchange network memberships, marriage, bridewealth, and mortuary presta-tions.

In a valuable account of William Partridge's fieldwork in Colombia, the precise points at which systematic records emerged from field-notes are identified. Some six months after arrival in his research community, Partridge wrote Solon Kimball: "I am going to begin a series of directed interviews," choosing respondents from "the *costeño* [coastal] hamlet of laborers, the *cachaco vereda* [mountain settlement] La Piedra, and selected older people of the town's upper crust. I will record the interviews on five-by-eight-inch sort cards." Up to that point, information from these three groups had been included in Partridge's chronological fieldnotes. Six months later a new set of records—interviews on marijuana production and use—was begun. Again, this crystallized data collection already under way in Par-tridge's fieldnotes (Kimball and Partridge 1979: 131, 172).

The balance between fieldnotes and records is unique in each re-search project, and most if not all anthropologists produce both kinds of documents. Many ethnographers would probably feel uncomfort-able speaking of research as fieldwork if it produced records but no fieldnotes. Yet the demands of particular subdisciplines and theoretical approaches increasingly drive fieldworkers toward more directed rec-ord collection. Attention to wide-ranging fieldnotes correspondingly recedes.

John Hitchcock, in his 1960–62 fieldwork in Nepal, used a carefully formulated interview guide, yet "much that we learned was picked up fortuitously" and recorded as fieldnotes.

> On balance . . . it was a boon to have well-defined research objectives and easily drawn lines between relevance and irrelevance. Yet the situation was not without paradox. The same design that was guide and support . . . could become a demon rider . . . and I railed at it. . . . It did not truly lay to rest a conscience enhanced if not derived from written exposure to eminences like Boas. . . . The communal live sacrifice at the fortress described in *The Magars of Banyan Hill* [Hitchcock 1966] could not have been written without notes that from the point of view of the research design did not seem strictly relevant. [1970: 176]

Margery Wolf, in writing *The House of Lim* (1968) and *Women and the Family in Rural Taiwan* (1972), drew upon both fieldnotes and records. She was "gratified by all the seemingly purposeless anecdotes, conversations verging on lectures, and series of complaints that *had* been recorded. Clearly, the presence of unfocused, wide-ranging, all-inclusive fieldnotes was essential to the success of this unplanned project." During her 1980–81 interviews in China, it was impossible to produce much in the way of similar fieldnotes; in her view, a more restricted and limited book necessarily resulted.

"If we are to develop authentic descriptions of individual behavior and beliefs," the Johnsons write, "we must accompany the subject into the several significant settings that evoke the many facets of the whole person." They identify the dangers of records without fieldnotes: "The tight, deductive research designs of the behavioral scientist are necessarily reductionistic. . . . Anthropologists generally agree that most human behavior is overdetermined, serving multiple purposes or reflecting multiple meanings simultaneously." Among ways to balance record-oriented research with wide-ranging ethnographic fieldnotes, the Johnsons propose a "cultural context checklist" as a medium for constantly reintroducing holistic concerns into fieldwork routines—much as Honigmann (1970: 43) reports that reviewing Murdock's *Outline of Cultural Materials* was useful to him.

Texts

Among fieldnote records, "texts" are a particular kind, with their own long history in anthropology. They are produced by *transcription*, Clifford's third type of ethnographic fieldnote writing. Transcription,

unlike inscribing scratch notes, usually involves an encounter between informant and ethnographer away from ongoing social action and conversation. Ideally, the ethnographer and informant sit alone together; the ethnographer carefully records answers to posed questions, or writes down in the informant's own words and language a dictated myth, spell, recipe, or life history remembrance. While handwritten transcriptions may be retyped and translated later, the point is to secure the informant's precise words *during* the fieldwork encounter, as they are spoken. The results of such fieldwork procedure are texts.

Texts figure prominently in the fieldnotes of Franz Boas. He published more than 3,000 pages of Kwakiutl texts and translations, many written by George Hunt, and some 6,751 pages of texts from all his fieldwork (Codere 1966: xiv; White 1963: 23–24). These texts give us "the lineage myth as its owner tells it, the potlatch speech as it was given, the point-by-point procedures in making a canoe," according to Helen Codere (1966: xxx), who knows as well as any anthropologist the full Boas corpus. Her three examples stand for three different social contexts of transcription: (1) a myth recited for the anthropologist—a text reproduced away from its normal context of recital; (2) a speech given during an event—a text recorded in the context of its social production, heard by natives and ethnographer alike; (3) an account of a technical procedure—a text created at the prompting of the ethnographer and not recoverable in such form elsewhere.

Although the second context—recording ongoing speech events—certainly results in texts, it partakes of both inscription and transcription. In a contemporary sociolinguistic appraisal of interview methods, Charles Briggs (1986) argues against imposition of the Western/middle-class interview speech event and in favor of culturally grounded forms of listening and talk, learned over time through participant-observation. His cautions are relevant to both the first, displaced mode of transcription and the third, fabricative one. His argument would favor the second inscription-transcription mode. Texts resulting from such ongoing speech events would also be more appropriate to the goals of text transcription professed by Boas.

These goals, according to Stocking, are well presented in a 1905 Boas letter on the importance of published texts:

> I do not think that anyone would advocate the study of antique civilizations . . . without a thorough knowledge of their languages and of the literary documents in these languages. . . . In regard to our American Indians . . . practically no such literary material is available for study. . . .

My own published work shows, that I let this kind of work take precedence over practically everything else, knowing it is the foundation of all future researches. Without it . . . deeper studies . . . will be all but impossible. Besides this we must furnish . . . the indispensable material for future linguistic studies. [Stocking 1974: 122–23]

The linguistic value of Boas's displaced and created texts is most useful in work on morphology, syntax, and semantics; it is less so for stylistics and pragmatics than the texts of actual speech events would be (Jacobs 1959). In "antique civilizations," texts and physical remains are all we have. In living societies, however, other anthropologists have not elevated text-recording in fieldwork to the height that Boas did; rather, they have valued participant-observation, with its other forms of note-taking. Nonetheless, it *is* the potential of texts to assist in "deeper studies" that has accounted for their continuing transcription.

For Boas, one aim of ethnography was to "disclose . . . the 'innermost thoughts,' the 'mental life' of the people," and texts were a means "to present Kwakiutl culture as it appears to the Indian himself" (Codere 1966: xi, xv). With fieldnotes and other kinds of records, texts have been used by other anthropologists to meet similar goals. On Manus Island in 1928–29, Reo Fortune "concentrated on texts, once he had trained Pokanau to dictate the contents of last night's seance. He took everything down in longhand" (Mead 1972:174). The limits of displaced transcription, however, were revealed to Mead in 1953 when Pokanau told her that her more rapid typing of his texts permitted him to "'put it all in.' The 'all' simply meant an incredible number of repetitions." But it is precisely "repetition" and other performative and paralinguistic features that today so interest analysts of transcribed texts of ongoing rituals and other speech events.

Like Mead (see also 1977: 297), Mandelbaum in India in 1937 transcribed texts directly by typewriter from his English-speaking Kota informant Sulli. Although "my notes and the quotations of his words usually preserve the structure of his utterance, . . . as I typed I would repair, for the sake of future clarity, some of his direct speech" (1960: 279n). Sulli's texts covered a wide range of Kota culture. He also dictated texts for Murray Emeneau, who mentioned in *Kota Texts* (1944)—based entirely on Sulli's displaced oral productions—that he was a "fine storyteller who adjusted to the slow pace of dictation without losing the narrative and entertainment qualities which are characteristic of Kota tales" (Mandelbaum 1960: 306). In candor, Man-

delbaum also adds that Sulli's narratives tended "to be neater and more integrated than was the historical actuality," and that he tended "to figure much larger in his account than he may have in the event" (1960: 307). Displaced and created texts are here certainly Kota "culture as it appears to the Indian himself." Like all texts, nonetheless, they and their creator are positioned in their local society.

Life histories turn around the disadvantages that such texts, created at the ethnographer's prompting, have for any general appreciation of "the mental life of the people." Instead, they purposely position the informant within her or his local society. In addition to large chunks of texts, life histories as genre present analysis based upon fieldnotes and other forms of records. John Adair (1960: 495–97) describes the life history fieldwork process, with an extract from his transcriptions once they reached a text-productive stage. Informative accounts of collecting life history fieldnote texts are provided by James Freeman (1979), Sidney Mintz (1960) and Edward Winter (1959). Langness and Frank (1981) offer a history and overview of this ethnographic option.

With literacy, the displaced oral productions and created accounts of informants may take on a self-edited form (Goody 1977, 1986, 1987) more like ethnography and, before recent interests in narrativity and rhetoric, well suited to the ethnographer's textual goals. Recalling fieldwork with the Copper Eskimo, Jenness conveys the frustration of many past text transcribers with nonliterate informants and their non–Western/middle-class speech conventions.

> We then closeted ourselves with two old men, whose hearts we warmed with some hard biscuits and cups of steaming chocolate. The comfortable tent and the unusual beverage loosened their tongues. . . . In the end it was not their secretiveness that hampered our researches, but our ignorance of their ways of thought and their own inability to narrate a story from the ground upward; for they invariably began with the crisis, so to speak, and worked backward and forward, with many omissions and repetitions, on the tacit assumption that our minds moved in the same groove as theirs and that explanations were needless. [1928: 202–3]

Sulli's texts no doubt reflect his schooling. So did the detailed, sequential account of the three-day Agarabi male initiation ritual dictated to James B. Watson on his second New Guinea field trip in 1963–64 by "a handsome, clean-cut youth" whose "clothing, his bearing, and his excellent pidgin, deliberately interspersed with English, be-

trayed that he had been to school and had also worked for a time in a town or on the coast."

> "The First Day," the young man announced like a title, flashing me a self-conscious smile. He began to detail the preliminaries of the ritual. . . . I finished the last unused leaf of the notebook and . . . continued the notes on the inside back cover, then on the outside. . . . He stopped to ask if I did not have another book. . . . I called out to the house . . . for someone to bring me the book. . . . We picked up where we had stopped. . . . My eyes were straining now from seldom looking up. Page by page we noted all the events of "The Second Day," finally reaching the third. . . . At last the session ended. . . . We had been at it for well over two hours. . . . My collaborator told me cheerfully that he would be available tomorrow for any further questions. . . . Sure that I knew the village well ten years ago, I had found no one like this. . . . No elder I had ever talked to could do what had just been done. [Watson 1972: 177–79]

The next step with literate informants, as Boas long ago learned with George Hunt, is to add texts written by the informants themselves to the ethnographer's own body of fieldnotes. This happened spontaneously for Mintz in 1953 after he asked Don Taso, a Puerto Rican sugar cane worker, if he could tape-record his life story. "He asked for time to think about it. . . . The following evening when we sat down together again, he produced from his pocket several sheets of lined paper, torn from a child's notebook, on which he had written down his story. . . . So the formal gathering of the data on Taso's life began with a written statement." Mintz published an English translation of this text, and reproduced a page from the handwritten Spanish original, in *Worker in the Cane: a Puerto Rican Life History* (1960: 27–31; illus. 4).

Letters from informants on ethnographic topics (Kluckhohn 1960: 450; Lowie 1960: 431–32) are another form of text, as is "The Diary of an Innkeeper's Daughter," found among the Suye Mura materials that accompanied the Embrees' fieldnotes when Smith received them. In Rwanda in 1959–60, in addition to transcribing forty-eight life histories, Codere (1970: 157) had a dozen Rwandan "reporters" fill many notebooks for her. Meeting the Boasian mandate, "the good notebook material does give a picture of the activities and preoccupations of the young Rwanda that year, of their mobility, and of their version of what they saw around them." Several of Jean Jackson's anthropological informants also gave their field informants notebooks to produce

their own fieldnotes (see also Beattie 1965: 26–27, 30–34; Epstein 1961; Evans-Pritchard 1974; Lewis 1951: xix; Parsons 1917; Schapera 1935: 318). Perhaps the uncertainty of ownership between sponsor and author of these informant-produced texts is involved in the lack of clarity many of Jackson's informants expressed over what to include under the "fieldnotes" label.

Journals and Diaries

Journals and diaries are written products of fieldwork that serve indexical or cathartic purposes for ethnographers (Ellen 1984b: 289). Chronologically constructed journals provide a key to the information in fieldnotes and records (cf. Carstens et al. 1987); diaries record the ethnographer's personal reactions, frustrations, and assessments of life and work in the field. In some cases the same account will contain elements of both forms, as is evident of two extracts from S. F. Nadel's "diary" of his Nuba fieldwork (Husmann 1983; see also Turner 1987: 94). Latterly, the increasingly intertextual nature of post-field ethnographic writing has intruded on both journals and diaries. Journals may now record reactions to ethnographies read or reconsidered in the field; and diaries, one suspects, may be written with the aim of publishing a "personal account" of fieldwork (as with Barley 1983; Cesara 1982; Rabinow 1977; Romanucci-Ross 1985. See Geertz 1988: 89–91).

In her Pacific fieldwork Margaret Mead kept "a diary"—or *journal*, using the distinction I make here—"stripped of comment, as an index to events and records. This was an act of responsibility in case my field work was interrupted and someone else had to make sense of it" (1977: 11). Honigmann's 1944 and 1945 journals from his fieldwork among the Canadian Kaska Indians were similarly a daily record of activity; his fieldnotes were "on 4″ × 5″ slips of paper and categorized according to the advice in George P. Murdock's manual called *Outline of Cultural Materials*" (1970: 40). In Honigmann's case, there were no "fieldnotes proper"; the journal and topical fieldnote records together contain the information that more ordinarily appears in chronologically kept fieldnotes. Boissevain's 1960–61 Malta fieldwork journal—"a daily diary into which I entered appointments and a rather terse summary of persons and places visited during the day" (1970: 79–80)—is another example of the journal form.

Rosemary Firth's 1939–40 Malayan fieldwork diary was something

different from these three examples of journals, or from that of her husband:

> [It] became for me a sort of lifeline, or checking point to measure changes in myself. I believe Raymond Firth kept a mainly chronological-record type of diary when he was in Tikopia [Firth 1936: 2] and Malinowski the more personal sort when he was in the Trobriands. Mine was used as an emotional outlet for an individual subjected to disorientating changes in his [sic] personal and social world. Perhaps ideally, both kinds should be kept; first the bare facts, the news summary as it were, then the personal reactions. [1972: 15]

Bronislaw Malinowski's *Diary in the Strict Sense of the Term* (1967) is certainly well titled. It has been the subject of many assessments, of which that of Anthony Forge—like Malinowski, an ethnographer of Melanesia—is both sympathetic and useful.

> It was never intended for publication. . . . These diaries are not about the Trobriand Islanders. . . . They are a partial record of the struggle that affects every anthropologist in the field: a struggle to retain a sense of his own identity as an individual and as a member of a culture. . . . Under these circumstances a diary is . . . your only chance of expressing yourself, of relieving your tensions, of obtaining any sort of cathar-sis. . . . The negative side of fieldwork . . . predominates in the diaries . . . a place to spew up one's spleen, so that tomorrow one can start afresh. [1972: 292–96. Also see Geertz 1988: 73–83; Mead 1970: 324n]

Other anthropological diarists, whose work we do not see in full as we do Malinowski's, stress the personal functions identified by Forge. When experiencing "despair and hopelessness" in her fieldwork in Mexico, Peggy Golde (1970a: 75) vented her feelings in her diary. Margery Wolf, ranging more widely, recorded her "irritation with village life, some wild hypotheses of causation, an ongoing analysis of the Chinese personality structure, various lascivious thoughts, dia-tribes against injustice, and so forth."

Diamond Jenness's 1913–16 Arctic fieldwork led to both diary (1957: 9, 88) and fieldnotes (1928: 14, 28, 41, 83–84). *Dawn in Arctic Alaska,* covering the first months of his research, portrays Alaskan Eskimos much more acculturated to Western society (1957: 100, 103, 122) than

the Canadian Copper Eskimo described in *The People of the Twilight* (1928), one of the earliest and best of many personal ethnographic accounts. *Dawn in Arctic Alaska* was written from Jenness's diary, he tells us (1957: 8)—plus his headnotes, of course. An extract from the diary is included (1957: 88–89), and the book incorporates both the factual (journallike) and the personal (diarylike) qualities that his field diaries clearly contain. No prefacing statement identifies Jenness's textual sources for *The People of the Twilight,* but its chronological structure must also be based on his diary; again, the factual and the personal are comingled.

The intertextual environment of contemporary anthropology figures centrally in the extensive personal journals—"the most private of my notes" which "I imagine I would never want to make public"— that Rena Lederman kept along with her fieldnotes and records during her New Guinea research: "There are reactions to the books and articles I was reading—some anthropology, some history, and some other things—usually entered . . . in the form of ideas for a dissertation/book or for articles."

A textual influence on anthropological journals and diaries that has registered powerfully in recent decades is Lévi-Strauss's *Tristes Tropiques* (1955), in English translation since 1961. Clifford Geertz says of it: "Though it is very far from being a great anthropology book, or even an especially good one, is surely one of the finest books ever written by an anthropologist" (1973: 347; see also 1988: 25–48). While other personal accounts of fieldwork predate it (Cushing 1882–83; Jenness 1928; Kluckhohn 1927, 1933; Osgood 1953; Wissler 1938), none except Laura Bohannan's *Return to Laughter* (Bowen 1954) has had nearly the impact of Lévi-Strauss's work, as is evident from references to it in several fieldwork accounts (Alland 1975; Rabinow 1977; Romanucci-Ross 1985). One also suspects its inspiration or stylistic influence in several others where it is not mentioned (Barley 1983, 1986; Cesara 1982; Gearing 1970; Maybury-Lewis 1965; Mitchell 1978; Read 1965; Robertson 1978; Turnbull 1961; Wagley 1977; Werner 1984).

Stirred by this burgeoning genre since the mid-1950s, intentions to write personal fieldwork accounts later have no doubt revivified a fieldwork diary tradition that had been giving way to indexical journals under the growing influence of social anthropology and behavioral science models. Simon Ottenberg, writes of his 1952–53 Afikpo fieldwork: "I did not keep a diary . . . which I very much regret today.

But we were brought up in a positivistic age where personal impressions were seen as less important than the 'facts out there.' "

Letters, Reports, Papers

Fieldnotes, records, texts, and journals and diaries remain in the field with their author and one-person audience. Many ethnographers mail carbon copies of fieldnotes home for safekeeping, but not, normally, for reading by anyone else. The exceptions are usually graduate students who send sets of fieldnotes to university advisors and mentors, as did William Partridge to Solon Kimball (Kimball and Partridge 1979).[3] Kimball's investment in Partridge's fieldwork via return letters was considerable—and unusual; in few other places in the fieldwork literature are similar involvements recorded. When advisors write to students in the field, it is more likely in response to those in-field compositions written to leave the field—letters, reports, and papers.

Probably most anthropologists in the field write letters to family members and friends, to mentors and professional colleagues. Letters, first of all, inform others that one is alive and well, or alive and recovering. They also allow the fieldworker to report on his or her psychological state and reactions—see Rosemary Firth's letter to her father (1972: 16)—although not as fully or cathartically as do personal diaries. "The long letters that Ruth and I wrote to our families are poor substitutes for a diary" (Dentan 1970: 89).

Perhaps more significantly, letters allow the ethnographer to try out descriptions and syntheses in an informal fashion. Hazel Weidman's 1957–58 field letters from Burma include evocative descriptions of Rangoon and of the hospital in which she conducted fieldwork (1970: 243–46). Buell Quain's 1938 letter from Brazil to his advisor Ruth Benedict (Murphy and Quain 1955: 103–6) is a rounded, rich description of Trumai Indian culture, more human in tone than the abstractions of fieldnotes.

Letters are a first step in committing headnotes to paper (e.g., Mitchell 1978: 96–101, 104–7). As Lutkehaus reveals, Camilla Wedgwood's letters from Malinowski, received while she was doing fieldwork in Manam, indicate that her letters to him were the beginnings of

[3] Triloki Nath Pandey's letters to his advisor Fred Eggan were indeed his fieldnotes: he did not take notes in front of his Zuni informants, but he could safely write to his "boss" (1979: 257).

her analyses. "Cut out certain portions of your information and pub-
lish them in *Man* as it might be easier to do it out of informal letters
than for you to stew over the writing up of an article," he advised her.
Letters certainly can be a useful tool in constructing a personal account
of fieldwork such as A. F. Robertson's for his 1965–66 research in
Uganda (1978: 1–2).

Like her ethnography, and her marriages, Margaret Mead's letters
from the field are monumental. A substantial selection of them (Mead
1977), published shortly before her death in 1978, form an essential
complement to her memoirs (Mead 1972) and Jane Howard's biogra-
phy (1984) for an understanding of Mead's career in anthropology.
"Letters written and received in the field have a very special signifi-
cance. Immersing oneself in life in the field is good, but one must be
careful not to drown. . . . Letters can be a way of occasionally righting
the balance as, for an hour or two, one relates oneself to people who
are part of one's other world and tries to make a little more real for
them this world which absorbs one, waking and sleeping" (Mead
1977: 7).

In her early fieldwork Mead wrote individual letters to relatives,
friends, and mentors Franz Boas, Ruth Benedict, William F. Ogburn,
and Clark Wissler. But from her first fieldwork in Samoa in 1925–26,
she also typed multiple carbons of letters addressed to a group; her
mother too retyped letters and sent them to others. This practice
netted Mead return mail of seventy or eighty letters every six weeks in
Samoa, as well as setting a pattern that continued through her field
experiences into the 1970s. By the 1950s her field letters were circulat-
ing to fifty or more persons (1977: 8–10).

The final two forms of fieldwork writing we will consider are
reports and papers. In preparation for such writing, as well as for later
dissertations and publications and to identify gaps in their fieldnotes,
many anthropologists report "rereading," "reviewing," "working
up," "going over," "organizing," and "thumbing through" their field-
notes while in the field (Barley 1983: 91, 112, 169–70; Becker and Geer
1960; Ellen 1984b: 282; Firth 1972: 21; Gonzalez 1970: 171; Jenness
1928: 14; Lévi-Strauss 1955: 376; Pelto 1970: 263–64; Read 1965: 39;
Whitten 1970: 351; Yengoyan 1970: 417–18). On his own, Pelto "occa-
sionally wrote short essays on such materials (sometimes in the form
of letters from the field)" (1970: 266).

Most reports, however, are directed outside the field, toward spon-

sors and overseers of the research. From Samoa, Mead sent the National Research Council a report (1977: 42). John and Ella Embree wrote "progress reports to the Social Science Research Committee of the University of Chicago which had funded the study," as Smith found in the cache of their Suye Mura materials. In the month before leaving Somaliland in 1957, I. M. Lewis wrote a report that "runs to 140 roncoed foolscap pages and is pompously titled *The Somali Lineage System and the Total Genealogy: A General Introduction to Basic Principles of Somali Political Institutions*" (1977: 236). Similarly, Lederman's first extensive writing was a report on Mendi rural political economy, written for the Southern Highlands Province Research Committee, and submitted before she left the field in 1979.

Reports, if read, may produce responses useful in later ethnographic writing. Boissevain sent the Colonial Social Science Research Council a 14,000-word, six-month report from Malta: "Writing the report forced me to rethink basic problems and to look at my material. . . . In doing so I discovered numerous shortcomings. . . . Moreover . . . I was able to elicit valuable criticism and comments from my supervisor [Lucy Mair] and her colleagues at the London School of Economics. This feedback was invaluable. . . . I should have been consolidating my data frequently in short reports" (1970: 80, 84). In addition to letters and fieldnotes, Partridge sent Kimball six-week and six-month reports (both reproduced in Kimball and Partridge 1979: 28–48, 136–48). Unlike too many supervisors, Kimball replied to Partridge with his reactions and suggestions.

Professional papers are occasionally written from the field, although the lack of library resources makes this difficult. Frank Hamilton Cushing wrote many papers while at Zuni pueblo between 1879 and 1884, several of which were published (Green 1979: 12–13), among them his personal fieldwork account, "My Adventures in Zuni" (Cushing 1882–83; Green 1979: 46–134). Ninety years later Partridge wrote "Cannabis and Cultural Groups in a Colombia Municipio" after a year in the field; flew to deliver the paper at the 1973 Ninth International Congress of Anthropological and Ethnological Sciences in Chicago; and returned to complete the final months of his research (Kimball and Partridge 1979: 190, 192, 220). The paper was subsequently published (Partridge 1975). While in Bunyoro, Beattie wrote a paper for an East African Institute of Social Research conference (1965: 44, 51). Also in the field, Lederman prepared an abstract and outline for a paper she presented at

the American Anthropological Association meeting later that year after returning home, no doubt a more common experience than that of Partridge.

Tape Transcripts

Transcripts of taped, dictated fieldnotes and texts may be typed out of the field—by paid assistants in some cases—but the resulting documents work much like fieldnotes in relation to later forms of ethnographic writing. Dictating fieldnotes is by no means a common practice among ethnographers, though the technology to do so has been available for decades (but see Barley 1983: 62; Warner and Lunt 1941: 69). Speaking into a microphone while one is alone would no doubt appear a suspicious practice in many parts of the world. But I suspect the missing scratch-notes-to-fieldnotes step is the primary reason that dictation is rarely used. Sitting and thinking at a typewriter or computer keyboard brings forth the "enlarging" and "interpreting" that turns "abbreviated jottings" and personal "shorthand" into fieldnotes. Margaret Mead wrote in 1953, "I don't dare use tape because there is no chance to work over and revise—or, if one does, it takes as long" (1977: 252). Untypically, Gertrude Enders Huntington and her family members, in a study of a Canadian Hutterite colony in the early 1960s, dictated some fifty typed pages' worth of fieldnotes a week into a tape recorder; they also kept written fieldnotes and records, but writing time was at a premium in this communal society (Hostetler and Huntington 1970: 213). If tape-recording one's own fieldnotes has not become a popular ethnographic practice—for good reason—taping texts is another story. Laura Nader, in a short study in Lebanon in 1961, tape-recorded informant accounts of cases of conflict; these proved "much richer in contextual information" than similar cases recorded by hand (1970: 108). R. Lincoln Keiser taped interviews and life histories with Chicago Vice Lord gang members in 1964–65: "I was able to record highly detailed accounts of interviews that I could not have written by hand. Transcribing the tapes was the main difficulty. It took me months of steady work to finish" (1970: 230).

Untranscribed tapes sit in many offices and studies. The disadvantages mentioned by Keiser are real, but so are the advantages that he and Nader found in having instant texts of the sort that Boas and others labored for hours to record by hand, and with the oral features

that are often lost in written transcription encounters. Agar used participant-observation, documents, and taped "career history interviews" in his study of independent truckers. The lengthy interviews, "a format designed to let the interviewee have control," were the core of his research: "to work with this material, transcripts are necessary; their preparation is tedious work, since a clean hour of talk might take six to eight hours to transcribe. . . . Transcription was done on a word-processor to facilitate 'proof-listening'—going over the transcript, listening to the tape, and checking for errors" (1986: 178). Agar had an assistant transcribe most of the interviews, and his ethnography includes extensive quotations from these texts.

Current anthropological interests in political language and what Audrey Richards (1939; see also Briggs 1986) called "speech in action" require a good ear and a quick hand, or a tape recorder. The tape recorder is probably winning out. As David Plath reminds us, portable tape recorders are now a commonplace in rural villages as well as cities worldwide; their use by ethnographers in taping others no longer invites curiosity. New-fashioned styles of fieldwork are emerging in which transcriptions of taped texts are the primary if not the only form of fieldnotes produced (Agar 1980, 1986). Quinn's cultural analyses of American marriage (1981, 1982, 1987) are based on taped interviews— "patterned as closely as possible after ordinary conversations"—that average fifteen to sixteen hours for each partner in eleven married couples (1982: 776). As in Agar's work, extensive quotations from these texts appear in her publications, and the relationship between fieldnotes and analysis is as close as in any more traditional ethnography. Technology marches on, and taped texts are here to stay.

REFERENCES

Adair, John. 1960. A Pueblo G.I. In Casagrande 1960, 489–503.

Adams, Richard N., and J. Preiss, eds. 1960. *Human Organization Research*. Homewood, Ill.: Dorsey.

Agar, Michael H. 1980. *The Professional Stranger: An Informal Introduction to Ethnography*. New York: Academic Press.

———. 1986. *Independents Declared: The Dilemmas of Independent Trucking*. Washington, D.C.: Smithsonian Institution Press.

Alland, Alexander, Jr. 1975. *Where the Spider Danced*. New York: Anchor Press.

Barley, Nigel. 1983. *Adventures in a Mud Hut: An Innocent Anthropologist Abroad*. New York: Vanguard Press.

———. 1986. *Ceremony: An Anthropologist's Misadventures in the African Bush*. New York: Holt.

Barnett, Homer G. 1970. Palauan Journal. In Spindler 1970, 1–31.

Beals, Alan R. 1970. Gopalpur, 1958–1960. In Spindler 1970, 32–57.

Beattie, John. 1965. *Understanding an African Kingdom: Bunyoro*. New York: Holt, Rinehart & Winston.

Becker, Howard, and Blanche Geer. 1960. Participant Observation: The Analysis of Qualitative Field Data. In Adams and Preiss, 1960, 267–89.

Boissevain, Jeremy. 1970. Fieldwork in Malta. In Spindler 1970, 58–84.

Bowen, Elenore Smith [Laura Bohannan]. 1954. *Return to Laughter*. New York: Anchor Books.

Briggs, Charles. 1986. *Learning to Ask: A Sociolinguistic Appraisal of the Role of the Interview in Social Science Research*. New York: Cambridge University Press.

Briggs, Jean. 1970. Kapluna Daughter. In Golde 1970b, 17–44.

Carstens, Peter, Gerald Klinghardt, and Martin West, eds. 1987. *Trails in the Thirstland: The Anthropological Field Diaries of Winifred Hoernle*. Communication 14. Cape Town: Centre for African Studies, University of Cape Town.

Casagrande, Joseph B., ed. 1960. *In the Company of Man: Twenty Portraits of Anthropological Informants*. New York: Harper Torchbooks.

Cesara, Manda. 1982. *Reflections of a Woman Anthropologist: No Hiding Place*. New York: Academic Press.

Codere, Helen. 1966. Introduction. In Franz Boas, *Kwakiutl Ethnography*, xi–xxxii. Chicago: University of Chicago Press.

———. 1970. Field Work in Rwanda, 1959–1960. In Golde 1970b, 141–64.

Conklin, Harold. 1960. Maling, a Hanunoo Girl from the Philippines: A Day in Parina. In Casagrande 1960, 101–25.

Cushing, Frank Hamilton. 1882–83. My Adventures in Zuni. *Century Illustrated Monthly Magazine* 25:191–207, 500–511; 26: 28–47. [In Green 1979.]

Davis, John. 1984. Data into Text. In Ellen 1984a, 295–318.

Dentan, Robert K. 1970. Living and Working with the Semai. In Spindler 1970, 85–112.

Diamond, Norma. 1970. Fieldwork in a Complex Society: Taiwan. In Spindler 1970, 113–41.

Ellen, R. F., ed. 1984a. *Ethnographic Research: A Guide to General Conduct*. San Diego: Academic Press.

———. 1984b. Notes and Records. In Ellen 1984a, 278–93.

Emeneau, M. B. 1944. *Kota Texts*. Berkeley: University of California Publications in Linguistics.

Epstein, A. L. 1961. The Network and Urban Social Organization. *Rhodes-Livingstone Institute Journal* 29:28–62.

Evans-Pritchard, E. E. 1974. *Man and Woman among the Azande*. New York: Free Press.

Fenton, William N. 1972. Return to the Longhouse. In Kimball and Watson 1972, 102–18.

Firth, Raymond. 1936 [1963]. *We, The Tikopia: A Sociological Study of Kinship in Primitive Polynesia*. Boston: Beacon Press.

Firth, Rosemary. 1972. From Wife to Anthropologist. In Kimball and Watson 1972, 10–32.

Forge, Anthony. 1972. The Lonely Anthropologist. In Kimball and Watson 1972, 292–97.

Freeman, James. 1979. *Untouchable: An Indian Life History*. Stanford, Calif.: Stanford University Press.

Freilich, Morris, ed. 1970a. *Marginal Natives: Anthropologists at Work*. New York: Harper & Row.

———. 1970b. Mohawk Heroes and Trinidadian Peasants. In Freilich 1970a, 185–250.

Gearing, Frederick. 1970. *The Face of the Fox*. Chicago: Aldine.

Geertz, Clifford. 1973. *The Interpretation of Cultures*. New York: Basic Books.

———. 1988. *Works and Lives: The Anthropologist as Author*. Stanford, Calif.: Stanford University Press.

Golde, Peggy. 1970a. Odyssey of Encounter. In Golde, 1970b, 65–93.

———. 1970b. *Women in the Field: Anthropological Experiences*. Chicago: Aldine.

Gonzalez, Nancie L. Solien. 1970. Cakchiqueles and Caribs: The Social Context of Field Work. In Freilich 1970a, 153–84.

Goody, Jack. 1977. *The Domestication of the Savage Mind*. Cambridge: Cambridge University Press.

———. 1986. *The Logic of Writing and the Organization of Society*. Cambridge: Cambridge University Press.

———. 1987. *The Interface between the Written and the Oral*. Cambridge: Cambridge University Press.

Green, Jesse, ed. 1979. *Zuni: Selected Writings of Frank Hamilton Cushing*. Lincoln: University of Nebraska Press.

Gulick, John. 1970. Village and City Field Work in Lebanon. In Freilich 1970a, 123–52.

Gupta, Khadija A. 1979. Travails of a Woman Fieldworker: A Small Town in Uttar Pradesh. In Srinivas et al. 1979, 103–14.

Hitchcock, John T. 1966. *The Magars of Banyan Hill*. New York: Holt, Rinehart & Winston.

———. 1970. Fieldwork in Gurkha Country. In Spindler 1970, 164–93.

Holy, Ladislav. 1984. Theory, Methodology, and the Research Process. In Ellen 1984a, 13–34.

Honigmann, John J. 1970. Field Work in Two Northern Canadian Communities. In Freilich 1970a, 39–72.

Hostetler, John A., and Gertrude Enders Huntington. 1970. The Hutterites: Fieldwork in a North American Communal Society. In Spindler 1970, 194–219.

Howard, Jane. 1984. *Margaret Mead: A Life*. New York: Fawcett Crest.

Husmann, Rolf. 1983. Preface. In Jana Salat, *Reasoning as Enterprise: The Anthropology of S. F. Nadel*, 1–7. Göttingen: Herodot.

Jacobs, Melville. 1959. Folklore. In *The Anthropology of Frank Boas*, ed. Walter Goldschmidt, 119–38. San Francisco: Chandler.

Jenness, Diamond. 1928 [1959]. *The People of the Twilight*. Chicago: University of Chicago Press.

———. 1957 [1985]. *Dawn in Arctic Alaska*. Chicago: University of Chicago Press.

Keiser, R. Lincoln. 1970. Fieldwork among the Vice Lords of Chicago. In Spindler 1970, 220–37.

Kimball, Solon T. 1972. Learning a New Culture. In Kimball and Watson 1972, 182–92.

Kimball, Solon, and William Partridge. 1979. *The Craft of Community Study: Fieldwork Dialogues*. Gainesville: University Presses of Florida.

Kimball, Solon, and James B. Watson, eds. 1972. *Crossing Cultural Boundaries: The Anthropological Experience*. San Francisco: Chandler.

Kluckhohn, Clyde. 1927. *To the Foot of the Rainbow*. New York: Century.

———. 1933. *Beyond the Rainbow*. Boston: Christopher.

———. 1960. A Navaho Politician. In Casagrande 1960, 439–65.

Köbben, A. J. F. 1967. Participation and Quantification: Field Work among the Djuka (Bush Negroes of Surinam). In *Anthropologists in the Field*, ed. D. G. Jongmans and P. C. W. Gutkind, 35–55. New York: Humanities Press.

Langness, L. L., and Gelya Frank. 1981. *Lives: An Anthropological Approach to Biography*. Novato, Calif.: Chandler & Sharp.

LeClair, Edward, Jr. 1960. Problems of Large-Scale Anthropological Research. In Adams and Preiss 1960, 28–40.

Lévi-Strauss, Claude. 1955 [1974]. *Tristes Tropiques*. Trans. John and Doreen Weightman. New York: Atheneum.

Lewis, I. M. 1977. Confessions of a "Government" Anthropologist. *Anthropological Forum* 4:226–38.

Lewis, Oscar. 1951. *Life in a Mexican Village: Tepoztlan Restudied*. Urbana: University of Illinois Press.

Lowie, Robert H. 1960. My Crow Interpreter. In Casagrande 1960, 427–37.

Malinowski, Bronislaw. 1967. *A Diary in the Strict Sense of the Term*. New York: Harcourt, Brace & World.

Mandelbaum, David G. 1960. A Reformer of His People. In Casagrande 1960, 273–308.

Marshall, Gloria [Niara Sudarkasa]. 1970. In a World of Women: Field Work in a Yoruba Community. In Golde 1970b, 165–91.

Maxwell, Robert. 1970. A Comparison of Field Research in Canada and Polynesia. In Freilich 1970a, 441–84.

Maybury-Lewis, David. 1965 [1988]. *The Savage and the Innocent*. Boston: Beacon Press.

———. 1967. *Akwe-Shavante Society*. Oxford: Oxford University Press.

Mayer, A. C. 1978. The Remembered Village: From Memory Alone? *Contributions to Indian Sociology* 12:39–47.

Mead, Margaret. 1970. Field Work in the Pacific Islands, 1925–1967. In Golde 1970b, 291–331.

———. 1972. *Blackberry Winter: My Earlier Years*. New York: William Morrow.

———. 1977. *Letters from the Field, 1925–1975*. New York: Harper & Row.

Minocha, Aneeta A. 1979. Varied Roles in the Field: A Hospital in Delhi. In Srinivas et al. 1979, 201–15.

Mintz, Sidney. 1960 [1974]. *Worker in the Cane: A Puerto Rican Life History*. New York: Norton.

Mitchell, William. 1978. *The Bamboo Fire: An Anthropologist in New Guinea*. New York: Norton.

Murphy, Robert F., and Buell Quain. 1955. *The Trumai Indians of Central Brazil*. Seattle: University of Washington Press.

Nader, Laura. 1970. From Anguish to Exultation. In Golde 1970b, 95–116.

Norbeck, Edward. 1970. Changing Japan: Field Research. In Spindler 1970, 238–66.

Osgood, Cornelius. 1953. *Winter*. New York: Norton.

Pandey, Triloki Nath. 1979. The Anthropologist-Informant Relationship: The Navajo and Zuni in America and the Tharu in India. In Srinivas et al. 1979, 246–65.

Parsons, Elsie Clews. 1917. *Notes on Zuni*. American Anthropological Association Memoirs 19 and 20. Washington, D.C.: American Anthropological Association.

Partridge, William. 1975. Cannabis and Cultural Groups in a Colombian Municipio. In *Cannabis and Culture*, ed. Vera Rubin, 147–72. The Hague: Mouton.

Pelto, Pertti J. 1970. Research in Individualistic Societies. In Freilich 1970a, 251–92.

Powdermaker, Hortense. 1966. *Stranger and Friend: The Way of an Anthropologist*. New York: Norton.

Quinn, Naomi. 1981. Marriage Is a Do-It-Yourself Project: The Organization of Marital Goals. *Proceedings of the Third Annual Conference of the Cognitive Science Society*, 31–40. Berkeley: University of California.

———. 1982. "Commitment" in American Marriage: A Cultural Analysis. *American Ethnologist* 9: 775–98.

———. 1987. Convergent Evidence for a Cultural Model of American Marriage. In *Cultural Models in Language and Thought*, ed. Dorothy Holland and Naomi Quinn, 173–92. New York: Cambridge University Press.

Rabinow, Paul. 1977. *Reflections on Fieldwork in Morocco*. Berkeley: University of California Press.

Read, Kenneth E. 1965. *The High Valley*. New York: Scribner.

Richards, Audrey I. 1939. The Development of Field Work Methods in Social Anthropology. In *The Study of Society*, ed. F. C. Bartlett et al., 272–316. London: Routledge & Kegan Paul.

Robertson, A. F. 1978. *Community of Strangers: A Journal of Discovery in Uganda*. London: Scolar Press.

Romanucci-Ross, Lola. 1985. *Mead's Other Manus: Phenomenology of the Encounter*. South Hadley, Mass.: Bergin and Garvey.

Schapera, I. 1935. Field Methods in the Study of Modern Culture Contacts. *Africa* 8:315–28.

Shah, A. M. 1979. Studying the Present and the Past: A Village in Gujarat. In
   Srinivas et al. 1979, 29–37.

Spindler, George D., ed. 1970. *Being an Anthropologist: Fieldwork in Eleven Cul-
   tures*. New York: Holt, Rinehart & Winston.

Spindler, George, and Louise Spindler. 1970. Fieldwork among the Menomini. In
   Spindler 1970, 267–301.

Srinivas, M. N. 1987. *The Dominant Caste and Other Essays*. Bombay: Oxford
   University Press.

Srinivas, M. N., A. M. Shah, and E. A. Ramaswamy, eds. 1979. *The Fieldworker
   and the Field: Problems and Challenges in Sociological Investigation*. Delhi: Oxford
   University Press.

Stocking, George W., Jr. 1974. *The Shaping of American Anthropology, 1883–1911: A
   Franz Boas Reader*. New York: Basic Books.

Sturtevant, William C. 1959. A Technique for Ethnographic Note-Taking. *Ameri-
   can Anthropologist* 61:677–78.

Turnbull, Colin. 1961. *The Forest People*. New York: Anchor Books.

Turner, Edith. 1987. *The Spirit and the Drum: A Memoir of Africa*. Tucson: Univer-
   sity of Arizona Press.

Van Maanen, John. 1988. *Tales of the Field: On Writing Ethnography*. Chicago:
   University of Chicago Press.

Wagley, Charles. 1977. *Welcome of Tears: The Tapirape Indians of Central Brazil*.
   New York: Oxford University Press.

Warner, W. Lloyd, and Paul Lunt. 1941. *The Social Life of a Modern Community*.
   New Haven, Conn.: Yale University Press.

Watson, James B. 1972. Talking to Strangers. In Kimball and Watson 1972, 172–
   81.

Weidman, Hazel Hitson. 1970. On Ambivalence and the Field. In Golde 1970b,
   237–63.

Werner, Dennis. 1984. *Amazon Journey: An Anthropologist's Year among Brazil's
   Mekranoti Indians*. New York: Simon & Schuster.

White, Leslie A. 1963. *The Ethnography and Ethnology of Franz Boas*. Austin: Texas
   Memorial Museum.

Whiting, Beatrice, and John Whiting. 1970. Methods for Observing and Record-
   ing Behavior. In *A Handbook of Method in Cultural Anthropology*, ed. Raoul
   Naroll and Ronald Cohen, 282–315. New York: Columbia University Press.

Whitten, Norman E., Jr. 1970. Network Analysis and Processes of Adaptation
   Among Ecuadorian and Nova Scotian Negroes. In Freilich 1970a, 339–402.

Winter, Edward. 1959. *Beyond the Mountains of the Moon: The Lives of Four
   Africans*. Urbana: University of Illinois Press.

Wissler, Clark. 1938. *Indian Cavalcade*. New York: Sheridan House.

Wolcott, Harry F. 1981. Home and Away: Personal Contrasts in Ethnographic
   Style. In *Anthropologists at Home in North America: Methods and Issues in the Study
   of One's Own Society*, ed. Donald A. Messerschmidt, 255–65. New York:
   Cambridge University Press.

Wolf, Margery. 1968. *The House of Lim*. Englewood Cliffs, N.J.: Prentice-Hall.

——. 1972. *Women and the Family in Rural Taiwan*. Stanford, Calif.: Stanford University Press.

Wolff, Kurt. 1960. The Collection and Organization of Field Materials: A Research Report. In Adams and Preiss 1960, 240–54.

Yang, Martin M. C. 1972. How *A Chinese Village* Was Written. In Kimball and Watson 1972, 63–73.

Yengoyan, Aram A. 1970. Open Networks and Native Formalism: The Mandaya and Pitjandjara Cases. In Freilich 1970a, 403–39.

Examples of Fieldnotes

Canoe magic

Most of the magic associated with building canoes belongs to Zabebia — ie that concerned with making the canoe light & [?]ull, launching the canoe. Tula alone knows the coconut magic for caulking the canoe. (Mogaua). Also + Tsuina & by Ababa know the *nwabwa* caulking magic, wh if I (+ ǫ) went to hear I / did die (I think Te really believes this — the threat of death isn't a conscious means merely of keeping ǫ in the dark).

27 VI 33

INDEX.

2. An index page from one of Wedgwood's field notebooks.

There are two roof making groups in the village: one in Imamura consisting of Kitagawa old man, Sakaguchi and two Kurohiji (Chokichi's brother and Kumaichi).

Another group consists of Kaneda, Ishikawa and a Kamo man. They are usually invited to work by regions - i.e. Imamura in that buraku and neighboring, Oade for this region. But I heard people say that the Oade kumi is better.

I watched a Kawaze group of children play "ishi iri". At first a set of small squares are made like this
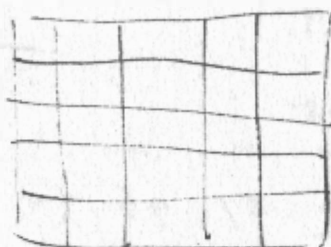
Some children vary this and make ~~vertical~~ horizontal lines curved, or the whole set in a circle.
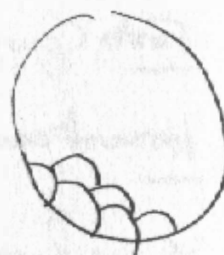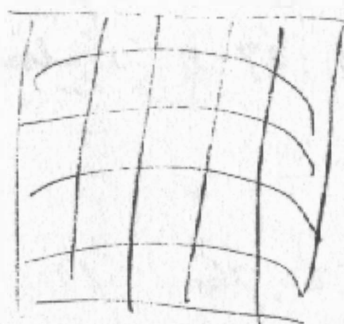
A small stone is put in some square at either end and one must cover the entire surface knocking the stone with a finger trick from one square to another. As one covers the course one rubs out lines between squares already covered, which makes it that much harder for the next person who had to shoot longer distances. between squares. When all lines have been rubbed out, the whole square is

3. A page from Ella Embree's Suye Mura fieldnotes. (Size: 8.5 by 11 inches.)

Sept.30.

Today is 15th of Aug. o.c. - jugoya kayaxitkexfaiixgexx.
Flower arrangement of kaya (the fall grass) and cooked taro and
potatoes - now out (sweet?) are offered to jugoya san, although
I only know of Mrs.Koeda who made them and she has no worms to
look after.

Many of the worms are spinning, but some are still down and
these days people are busy with them.

After supper children began to gather, they went from house
to house collecting straw from each (and 2-3 sen from none farmers)
which they brought to the empty lot next ot us. Bunji and the two
Aiko houng men came to do the job - other young men came up later
but did not do much. The rope was woven by Bunji and Kurahei's
servant while the son held the pole which they used as support,
the kids were to turn the rope as it emerged at the other end
thus helping it to twist. When the tremendous rope was ready - done
under pretty heavy rain instead of moonlight - the men came into our
hall and one made a huge warabi while the other one made an equally
huge ashi naka.

Senko were offered to the jidzo san and the zori hung there. Then
the rope was coiled in the one huge lump and Senko were stuck into it
here and there and children told to give an offering prayer - they
all leaned over the coil and inchanted unintelligible words in
imitation of praying. Then they grabbed one end and the tug of war
began. There is no winning and loosing since the rope won't break
but they just pull, now one side getting stronger, now the other
pulling each other along the slippery road. Eventually they the
rope became weak and when tired of the game they stop. Children
were chief participants - all girls and boys turned out in their
undo outfits.

4. Another page from Embree's notes, for September 30, 1936.

RELIGION

General: (See Churches; Schools, Education and Missions; Moslems; History, Amade-Otosi, Long Juju-Aro; Swear Erosi; Yam Priest and Shrine; Wrestling; Warfare, Inheritance; Funerals; Compound and Umudi, Village and Wards, Village-group, Ogo; Ikru, etc.). Here list under General only items that do not seem to fit well elsewhere.

Term Erosi, 3437
Miscellaneous, 5-6, D-42-43.
Three erosi in Government Station, 119.
Sacrifical materials, 247.
Three women's erosi, 299-300, 2241-42, 2455, 2284, L-96-99, 2307, 2376, 2377, 2438-39. See ikru, umudi, funerals, inheritance, Phoebe's notes.
Egbo erosi, ogbo's tree, 538.
Cross River erosi, 544-45.
Seven animals, 2436-37, 2521-22 and various.
Ogbe, 1504-11.
Aho- New Year, 973-75, 1089-90 and Phoebe's notes. Mine 2nd trip also?
Sacrifices to shrines on aho day, 975-79.
Catholic attitude toward work, L-74
Blessing, 1729
Lack of belief in erosi and in dibias, 2441; see else Enwo's attitudes, Tom Ibe's, mainly y regard to Ogo. Also Jane Nwachi. Also CE 94-95.
Fish in streams are erosi, O-81, O-33-84.
[handwritten] ʿhᵢ., ²ⁱ/ₚₐₗₑ, ²ʰₒₛₑ ₙₑ Eᵗᵥ Cₕₐᵢₑₖₐ -OK-57-65, OK-66-76.

Diviners and Divination

[handwritten] 532.
55, 164-68, 1054-56, 1084, 167-168, 168, 411-415, 530-34, 805, 1498-99, 1505-09, 1512-14, 1572-74, 1521, 1571, 1837-40, 1853-54, 1966-67, 1972, 2161-63, 2119, 2174-79, 2186, 2224-48, 1505-09, 2262, 2283-87, 2288-89, 2296-2306, 2323-34, 2345, 2366-70, 2371-75, 2376-80, 2403-06, 2429-30, 2436-39, 2440, 2459-66, 2468-72, 2473, 2483-84, 2475-78, 2491-92, 2494, 2499-2506, 2509-10, 2513-15, 2523, 2534, 2524-28, 2530-31, 2532, 2533, 2535-40, 2541-44, 2562-67, 2572-74, 2594-2603, 2607-17, 2647-55, 2656-62, 2830, 2737, LA-281, C-44, CE-66-67, 1722, *[handwritten] OK-9-10.*

obanji

2288-89, 2307-14, 2327, 2328, 2349, 2461, 2543, 2609-17, 2647-51, 2651-55. See Phoebe's notes.

Ndemeja

2328, 2376-80, 2437. See Phoebe's notes.

Illness (see above) see various other categories, especially for first trip.)

1666, 1707, 1762-65, 1766, 1972, 2241-42, 2242-43, 2244-45, 2296, 2298-2300, 2303, 2304-06, 2333, 2366-70, 2371, 2403, 2404, 2429-30, 2483-84, 2523, 2535-39, C-17-18, C-81, C-83-84, CE-66-67.

Reincarnation (See Ikru, especially genealogies, obanji, *[handwritten] Phoebe.* Diviners, etc.)

530-34, 1389-90, L-141-42, 1498, 1500, 1571, 1572, 1769, 2283-87, 2346, *[handwritten] IC 56-7,* 2376-80, 2402, 2410-16, 2439, 2454, 2468-72, 2616-17, O-38, O-50. *[handwritten] 510-11, 1061-63*

5. A page from Simon Ottenberg's index to his 1952-53 and 1959-60 Afikpo field-notes. (Size: 8.5 by 11 inches.)

general 133-40, 1084-90, 354-60,746,1005-07, 1011-13, D-62-65, L-127-30,
    L-238-43, 1018-25, 921-24, 1084-90, 2279,1449.
change in powers under British 155, more powerful in old days 719
cases where village-group grades not called 310, 317, 322, 334, 385, &tc.
village-group grades ont concerned with warfare L-132
Oni Ekara and yam planting season-shrines and activities, etc. 745-46, 2280
hori 746, 1477.
order of greeting at age grade meetings 868
village-grades, especially how they move up 354-60, 921-25, 1014-17
Esa try various cases at market 1011-13-general rules as well
ikpukeisi 1012, L-124-27, L-128,131, D-44, D-62-63, D-109, D-252, 746, 155, 121,
village-group pleaders 1421-28   149, 2279,
oni Ekara and Amade L-118, L-120, L-121, L-124-35   D-90,
   shale 212 356, 357, 361, etc 1477, etc.   O-20-27, 0-27-
                                                                 1918, 36
D.O. interferes Afikpo market, 11,25, 156,   2025.
Back women against ATWA and D.O. in Igwe affair 77-78, 148-49,
in Ilorozo (prepare-daed)ceremony 123-29
case in Esa court 147
Esa set day for bush burning 157, 347,
Esa set day for farming and harvest 346
Esa give ceremony to rainmaker for rain 164-67
Esa backs up decisions of Afikpo dibia society 168
ostracism of Enwo at brideprice ceremony, Mgbom 172-73, in general 303-04,
attempt to regulate bride price circumvented 307-8, 314,
part of Esa in "plant yam together" land dispute and in land cases
    in general, 342-343,
one large Mgbom grade in Clan Esa instead of two small ones 361,
ask young men as messengers, but never ask village grades 365
lay down changes in ogo initiation rulings 366-67
what grades one joins in village-group when a member of two in
    village 395
pass marriage resettlement rulings 400-01   in which court,
sanction money collection to try Ibi murder case, 402-03
to try ikwu land case 566-67 in Esa court
elders and Afikpo people do not go Amaseri market-ruling 806
clan law violaters do not take uhichi 850
send Esa man in Ilwu dispute 860
meeting-Oji's preresentations, bride price regulations, fining those
    who side with Okpoha in dispute, 866-67
meetingYam festival and Yam priest 1010-11,(see YAM PRIEST AND SHRINE), 915-18,
omume title members do not have to pay certain fines of village-group
        grades 1035, take part in ceremonies 1094, 2348
in Afikpo-Amaseri market dispute 1104-05 and see OASES
market payment dispute 1117
in Ngodo-Amachara-Ukpa school site dispute 1113,
Esa rules a limit of foo foo for marriage feast 1126
in omume title 1129
elders and D.O. rule first ogo ceremony should occur on Saturdays 1234
attempt to settle Anofia Nkalo-Ndibe dispute D-84-85
resolve to import Ogu men to catch criminals D-232,
try to maintain Ibe Osim women priest at shrine L-25-26
in New Yam Festival dispute L-19-23.
Igwe and elders 178-179
Meeting 378-404

6. Another page from Ottenberg's fieldnote index.

March 11

There is other evidence other than G's saying so, that parts of the Bara language are lost, at least in this maloca. He often says the 'viejos' talk thus - the right way. We the younger people, don't, or have forgotten, etc. Is it because this maloca is isolated and they have the most contact with Tuyukas?

Two instances: Juanico gave me two forms for eyelash and eyebrow; G didn't accept the one for eyebrow, said there was only one term, I didn't tell him Juanico had told me the other. Also Juanico gave me a term for Forehead that G didn't accept a couple of times; accepted it yesterday (differentiating it from 'face')

April 2

More formal work with tribe-language. G said there is no word for 'tribe' (which I knew), but mohoka can be asked, 'what people are they'. Questions are: (note difference in interrogative pronouns:

ñivo̅ nomohoko pakho kututi Estribina

ñe wadego pakho kututi ko / ñeno wadegu eahani

G said entity of mohako was always distinguishable by a separate language, that word for them was always the same, meaning 'Bara people'/ 'people who speak Bara' and that the questions were synonymous in that they always elicited the same answers, referring to specific persons or groups.

kututrihe
note: *kuvike* used - "to have"

July 6

Marcelino had a quarrel with the dressed Maku Sunday morning of the fiesta - outside in front. He was doing most of the talking, but the other man wasn't acting subservient or anything. Other men looked on, expressionless. Aside from that, there was little interaction between guests and Makus. The girl, Isiria, danced. Others looked at them. Girls giggled that old woman's breasts were funny-looking - one much bigger than the other. They aren't greeted or acknowledged in any way. In this case, they are (seem to be)

Miguel's particular pets.

7. Three fieldnote cards from Jean Jackson's 1969-70 work among the Bara Indians of the northwest Amazon River basin. The notes were filed by subject in baskets made for them by the Bara. (Size: 4 by 6 inches.)

HOUSE LINE PREPARATIONS: Alwesa
13 October 78

He has been referred to recently as one one of those who
wants to kill pigs this Xmas. People say he never kills
his pigs and so has alot of them saved up to kill soon.

He says now that he wanted to join Sale in killing pigs
soon but he doesnt think he can because he hasnt found the
shells he needs to pay off his wife's line -- ya ya tia.

Introduction

What does he need in order to kill pigs: his list of
debts to his wife's kin:

1. Pundiaep -- he owes him one shell and K8
                he will return this with "5": three shells and
                K20

2. W of Pundiaep -- two pigs
                he will return this with five shells for
                each plus two shells as nOpae

3. F of Waekiem in Komia: he owes him K10
                he will return to him two shells, one of
                which is nopae
4. Z of his W Kalta living in Tambul: he owes her two pigs
                and one shell
                he will return K80 for one pig
                        5 shells for another pig
                        one shell for the shell
                        he has already given the nopae of one shell
5. Marup Okipuk he owes K40
        he will return two shells for this. He has already
        given the nopae of one shell

6. Pepena he owes K10
        he will return this with two shells

7. Tamalu, a Kagol Yakop man in Komia, W's line he owes one shell
        he will return two shells

Pigs: He killed three pigs at the recently parade.
        He says he has four he can kill at the houseline
        He says no women are looking after pigs for him elsewhere
                and so he has no other mok ya ri payment to make
        (Kus, overhearing this, says "Ah, he must have about 20
                to kill, he's lying!)

8. A page from one of Rena Lederman's formal interviews in the Mendi Valley, Papua
New Guinea, October 13, 1978. (Size: 8.5 by 11 inches.)

Sunday, 26 June

Timbew came by a bit angry about Maklen's marriage arrangements (Maklen is her next-to-last D; the last is in Grade 4 w/ Andrew Ipopi in Banz). Pospeya's son had showed the family sufficient wealth, in a formal presentation, already — Munganawe (F) had been satisfied. But Paki had befriended a man from Olmanda when both were in town — the Olmanda had given Paki food & school fees, and had asked Paki to find him a wife later on. Paki had suggested Maklen — and now the Olm. is inviting the family to check out the wealth he wants to give them. Timbew was angry because Naro (M's 'Z'H, Paki's F) said he couldn't go along w/ her to look it over since Rina is sick. T. says "So what are we women to do? Munganawe won't go: he's pleased w/ Pospeya's son's wealth. Warge commented that he'd been asked to go too, but he refused since he's not one to talk out "What am I going to do? Stand up and say 'this isn't good enough!'?" (!)

Who does talk out, then? Tui (who's been sitting around the fire with Warge + me this morn) says two here are known for it: Kiluwa and Walipa. I suggest Onge, and they agree. They also added: Munganawe, Takuna, Kandi, and joked that it isn't a matter of being 'headmen' but just of having a "loud voice"! (A partial truth...)

Warge reported that when his ZD in Pimanda

---

9. Two pages from Rena Lederman's 1983 "daily log" fieldnotes in the Mendi Valley, Papua New Guinea. (Size: 7 by 9.75 inches.)

got married yesterday, she was given K1700 of _nopae_ (ie - _not_ counting the money given to repay the wedding pigs!). The new husband is an ADC and has been stationed all over the SHP.

More about _Nande_'s garden: turns out that its not such a special case. M of Nande's MM is from Egari (You). _Mopna_ gave Nande the garden. Mopna was divorced from her Bela H and returned to her F's place to live. So Nande is actually gardening in the place of her MF (not her MMM) — this is the garden of her MM too, of course, in terms of _use_, but its a Yansup garden (MF's group); ~~effective~~ her M's group. Nothing unusual about that at all. She isn't gardening on lands of her MM's group (Marup) — though, in this case, she _could_ have since its Kuma! ⟩

(Egari: You) ⊙〓△ (Kuma: Marup)

(Marup) Wokian ○〓▲ (Sol: Yansup)

(Komboli Molsem)
▲ = ● Kurum   mopna ● = △ (Bela: Kondup)   ● Timbeu
      (Yansup) (Yansup)   (Yansup)

⦸
Nande ←— GARDEN
(Molsem)
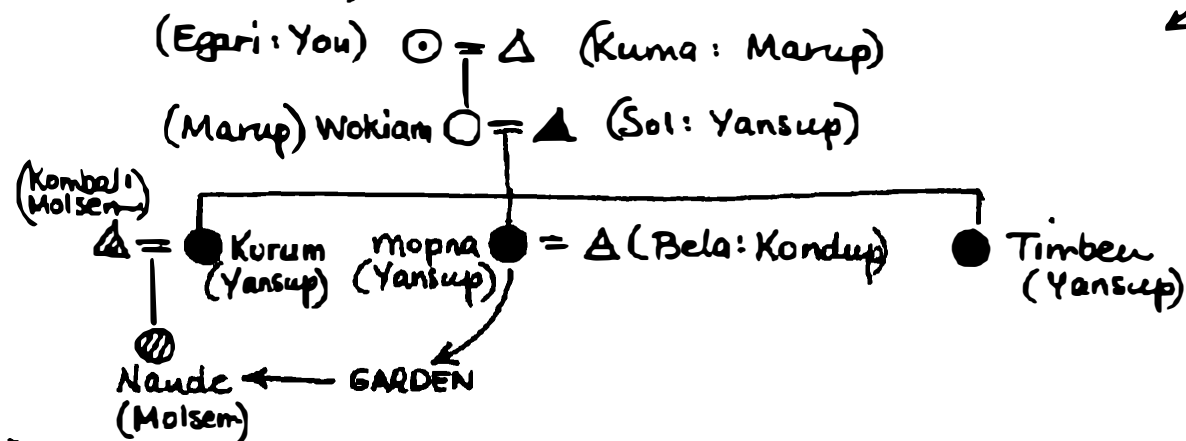
See typed pp., _Poga_ interview for this date on Keyosem's changed affiliation.

Nande wanted _Alin_ to hear her _konaen_ which

G_en Ankrah's mother was visiting M/M Q today. He introduced called her as "Madam Quaynor" at the family accounting. She is the senior woman in the Q family.

2 men visited Mr. Q in the afternoon. One lives at Kwabenya and work at the A,mic Emergy installation. The other, with a Fanti sounding name, Archie Davidson, was a primary teacher with Mr. Q in the founding of Gnat in the early 1950's. He said then "The old men were fighting for better pay and conditions for us." He is now a sociologist with the VRA and studied at Rutgers. They were drinking schnapps.

Attikpo bought shorts to be fixed by an intineratnt tailor who set up his machine in front of the Q house.

T gave me data on the family and funeral affairs.

11/2 mon   Mrs. Solo Q visited M/M Q in the morning.

Legon, notes, OT 5:30.

we met Ben and Alex at 7 pm and walked to the Adovors.

Alex asked Ben to buy smuggled matches "butterflies" for him at the kiosk opp. Yankah becasue he says the Ghana matches are no good. He said they wouldnot sell to him becasue they thought he was a cop. Ben did not get any either. Alex said becasue Ben is known they would sell to him.

We walked to the Adovor's house and met Attikpo who was visiting someone else there. He also used to live there. We went up and Alex excused himself. Mrs. Adovor came up from the kitchen.

She said Mr. Adovor's cousin had his VW stolen today and Mr. Adovor went to help him. Mr. A's brother came by and asked about the affair.

We discoussed Ewe food which they see as identifying them as different, say from the Ashantis who eat only "fufu and ampesi," and Gas wholike kenkey and banku. The Ewes they say have "so many foods." They later talked about having sent Ewe food overseas tokin and friends studying.

Mr. A came with a friend who is from Ezodze and works for the VRA at Akosombo; he is visiting in Accra. He works in the fisheries and studied fish farm ng in Seattle for 2 years. He told us how he used farina and peanut butter to substitute for gari and groundnut paste. He said the food he missee most from US was pie, esp. lemon pie. He said Accra is too fast and expensive for him.

He said you canot tell tribe of women by dress in Ghana becasue the Ga and Akan and Ewe women all dress alike (also true in LOme.) "Even by the face you cant tell," if there are no marks, he said.

Mr. A said the Adas are related to the Gas, but there has been some marriage with Ewes on the bomber, and they go to each others marketsм Some on the boarder speak Ewe. He said their names are either Ewe or Ga. The Ewes he said have vert distinctibw names.

"Our names are very different."

We talked about dress differences in men's traditional clothes. The Akans dont wear jumpers, but the Gas, Ewes and Fantis do, with cloth, Mr. A said. The Gas wear the long shorts. They said the stocking cap is only worn by Anloga people.

The E"es along the Volta river, eg. Sogakofe, have a very difficult dialect for other Ewes to speak. Mrs. A said Adama, head of the oppsestion speaks this dialect, from Sogakofe.

10. A page from Roger Sanjek's 1970 Adabraka, Ghana, fieldnotes. (Size: 8.5 by 11 inches.)

7 May 1988 - Carmela George's Cleanup Day

Milagros and I arrived at 10 am, as Carmela told me, but 97th street, the deadend, was already cleaned out, and the large garbage pickup truck, with rotating blades that crushed everything, was in the middle of 97th Place. I found Carmela, and met Phil Pirozzi of Sanitation, who had three men working on the cleanup, plus the sweeper that arrived a little later. The men and boys on 97th place helping to load their garbage into the truck included several Guyanese Indians in their 20s, whom CArmela said have been here 2-3 years ['They're good.']; several families of Hispanics, and Korean and Chinese. They were loading tv sets, shopping carts, wood, old furniture, tree branches and pruning, and bags and boxes of garbage. Most houses had large piles of stuff in front, waiting for the truck. The little boys hanging on and helping were Hispanic, except for one Chinese. They spoke a mixture of Spanish and English together, when painting the LIRR walls.

Carmela had put flyers at every house on Wednesday, and Police 'No Parkin Saturday' signs [D   ] were up on the telephone poles. A few cars were parked at the curb, but most of the curbside on the three blocks was empty so the sweeper could clean the gutters.

The sweeper this year was smaller than the one in 1986, and there was no spraying of the streets, only sweeping the gutters. As before, people swept their curbs, and in some cases driveways, into the gutter. Carmela was a whirlwind. She asked her elderly Italian neighborh Jenny, who did not come out, if she could sweep the sand pile near Jenny's house in their common driveway. Jenny said don't bother, but Carmela did it anyway. She was running all around with plastic garbage bags, getting kids to help paint off the grafitti on the LIRR panels she had painted in the past, and commandeering women to clean out the grassy area near the LIRR bridge at 45th Ave and National Street. She got a Colombian woman from 97th Place, and gave her a rake and plastic bag. She then rang the door bell across from the grassy area, behind the bodega, and an Indian-looking HIspanic women came down, and later did the work with the Colombian woman..

Mareya Banks was out, in smock, helping organize and supervising the kids doing the LIRR wall painting. Milagros helped with this, and set up an interview appointment with Mareya. She also met a Bolivian woman, talking with Mareya, and sweeping her sidewalk on 45th Avenue.

Carmela also had potato chips and Pepsi for the kids, which the Colombian women gave out to them, and OTB t-shirts.

Phil said this was the only such clean up in CB4. A man in Elmhurst does something like this, but just for his one block. They Dept. likes this, and hopes the spirit will be contagious. We like anything that gets the community involved. He said it began here because the new people didn't understand how to keep the area a nice place to live. Carmela went to them, and now they are involved.

11. A page from Roger Sanjek's 1988 Elmhurst-Corona, Queens, New York, field-notes, printed from a computer word-processing program. (Size: 8.5 by 11 inches.)

ROGER SANJEK

Fieldnotes and Others

The primary relationship of fieldnotes is to their writer-reader, the ethnographer who produces them. Yet as objects they are seen, and sometimes read, by others. As Bond, Obbo, and Lutkehaus detail in this volume, these others are diverse—"the other" (as interpretationists are wont to call their informants) whom they are about; other "others" in the society studied but outside the immediate ethnographic range; and other anthropologists: teachers, colleagues, and those who may later read or even inherit and write from the original author's fieldnotes.

Informants, Publics, and Fieldnotes

Few anthropologists today, or even in the past, hide their researcher role as Mead did among the Omaha Indians. Most take notes openly— at least during ethnographic and formal interviews—though some ethnographers, like Whyte (1955, 1960), prefer not to write even scratch notes in front of informants but to rely later on their memory. Informants are aware of writing and its resultant documentary forms, if not of all the kinds of notes the anthropologist maintains. On some occasions, particularly rituals and ceremonies, the informants *expect* ethnographic note-taking (Powdermaker 1966: 87).

They also hear and see typewriters. The act of typing in the field, however—the reworking of scratch notes to typed or recopied field-notes—can dampen rapport when its desired privacy interferes with sociability. This was a particular problem for Jean Briggs, living in close quarters with an Eskimo family.

> I found it hard sometimes to be simultaneously a docile and helpful daughter and a dutiful anthropologist. Though Allaq appeared to accept my domestic clumsiness as inevitable, she may have felt less tolerant on the occasions when it was not lack of skill that prevented me from helping her but anxiety over the pocketful of trouser-smudged, dis-organized field notes that cried out to be typed. [1970: 25]

Briggs eventually moved her typewriter, and later her residence, to a separate tent. The point of contention in the iglu had been between rapport and fieldnotes. The outcome, a sober lesson in what fieldwork is all about, makes one wonder why so few of the extended personal accounts discuss fieldnote writing with any candor at all, let alone the measure provided by Briggs.

In situations where informants can read, other anxieties may arise as well. John Adair, working at Zuni Pueblo in the late 1940s, was confronted by reaction to a newspaper article on sacred clowns based on Cushing's earlier account.

> I learned that one of the men of the house where I was living had entered my room during my absence and looked through the notes which I had been careful to hide under the mattress. . . . There he had run across the native name for these clowns in a life history I was taking. . . . This discovery didn't help me with my relations with my landlord or his veteran sons. [1960: 492]

Rumors spread about Adair, and for this and other reasons he moved to a new residence. He was aware, of course, of Zuni resistance to anthropologists and knew that "in 1941, the Tribal Council confis-cated the field notes of an anthropologist and burned part of them. He was asked to leave Zuni within twenty-four hours" (Pandey 1972: 322n).

Experience or knowledge of social research methods is now com-mon in many world areas and creates expectations about what an anthropologist should or should not be doing. In Adabraka, Ghana, in 1970–71, I remember vividly when a newspaper reporter living in the same building asked me when I was going to begin doing my ques-

tionnaire. Shah (1979: 31) deliberately chose to work in a Gujarat village where an economist had conducted a survey in 1930: "A few villagers who knew English would inspect our field notes and a few who did not asked us to translate them. The villagers gained confidence in us only after they could place us in the social categories with which they were familiar," those of both researcher and fellow Indian.

The Whittens chose to head off misunderstandings in Nova Scotia and avoid any loss of rapport. "We showed people our manner of writing and filing notes, our genealogies, maps and mechanical aids (typewriter)" (Whitten 1970: 382). Other ethnographers have deliberately read fieldnotes back to informants, as Osgood (1940: 53) did to Billy Williams, to verify and expand on them. Stanner, in perhaps the finest essay in all the fieldwork literature, relates that when in 1954 he went over his notes of twenty years earlier with his Australian Aborigine informant Durmugam, they stood up well and provoked valuable reflections from their original source (1960: 86).

Many informants, even those who are illiterate, well understand the permanency of written records and may enlist the anthropologist to put things of *their* choosing down on paper. The Bow Society priests directed Cushing to transcribe their prayers and songs in precise, archaic Zuni (Green 1979: 149). Mead writes:

> When I arrived among the Manus . . . they had already been quarreling for thousands of years about how many dogs' teeth [their currency] somebody had paid to somebody else. . . . So the first thing they said to me when I came along was, "Ah, now Piyap [Mead] can write it down. You write down every single transaction and we won't need to quarrel any more." [Howard 1984: 106]

Read (1965: 203) had a similar experience of being asked by New Guinea Highland informants to record transactions in his notebook.

The relativities of text and experience discussed with subtlety by George Bond have also had their equivalents for other ethnographers. Schapera, whose 1938 *Handbook of Tswana Law and Custom* was a product of *his* fieldnotes, found its distributed copies returned to him with annotations by Tswana chiefs for the second edition (Comaroff and Comaroff 1988: 563). Like the Yombe with Bond's ethnography, they had turned it into an open text in which to record *their* notes.

Christine Obbo's essay here relates interest in her fieldnotes by Kampala chiefs and officials curious about the neighborhoods and activities she was studying. She details her strategies to put them off, as

well as her unsettling encounters with anthropologist and academic colleagues and their efforts to read her fieldnotes. Government officials, usually convinced that some greater secret than actually exists lies in fieldnotes, have attempted on occasion to read them elsewhere as well: in Ecuador, Ralph Beals's fieldnotes (Paul 1953: 229); and in India, those of Cora Du Bois, who left hers accessible to Indian intelligence officers to allay suspicions that she and her research team were American spies (1970: 224). According to Clifford's account (1988: 277–346) of the Cape Cod Mashpee Indian land claim trial, the threat of subpoena of an anthropologist-witness's fieldnotes was raised; and the fieldnotes of one anthropologist informant of Jean Jackson actually were subpoenaed.

Students and Colleagues

Few students arrive in the field ever having seen ethnographic fieldnotes. Mead, in her field methods course at Columbia, made a point of showing hers to her students (1972: 142–43); so do Ottenberg and Wolcott (1981: 256). Some anthropologists have also shared their notes with students working in the same field setting, such as Ottenberg with a student working among the Limba (this volume), Wolff (1960: 249n) with a student working in Loma, and Wagley (1977: 76) with Judith Shapiro working among the Tapirape. Foster opened his fieldnote files to three students working in Tzintzuntzan, requiring them to share their notes with him in return, and they may freely use and cite each other's data (Foster 1979: 178). The Comaroffs (1988: 559) have had access to Schapera's Tswana fieldnotes; as in the other instances, their mention bespeaks an amicable relationship.

All these cases, except Mead's, Ottenberg's, and Wolcott's teaching, illustrate a collegial practice of sharing field data rather than a didactic one of showing how to write fieldnotes. More usual (but one wonders) are situations in which a teacher reads a student's fieldnotes and reports as they are mailed home, or brought back from the field. Nancy Lutkehaus's essay in this volume discusses Malinowski's written responses to Wedgwood's field letters. Kimball and Partridge (1979) detail a similar dialogue founded on letters and reports more than on fieldnotes proper. Ruth Benedict's attention to students' fieldnotes was remarked upon by Mead (1974: 34, 59): "She made the most of her own field work, but I think she got greater enjoyment out of working over

her students' field notes, teaching them how to organize them and trying to make a whole out of their often scattered observations." The heartfelt acknowledgments in many dissertations and books no doubt evidence similar attention from other anthropological teachers.

Reports of colleagues sharing fieldnotes are also few but usually involve amicable relations, unlike the efforts at appropriation encountered by Obbo. Opler was given copies of fieldnotes by the other students—John Gillin, Jules Henry, Regina Flannery Herzfeld, Sol Tax—in a 1931 Laboratory of Anthropology field training party led by Benedict among the Apache (Opler was committed to continued work among the Apache, while the others were not) and also exchanged fieldnotes through the 1930s with another ethnographer of the Apache, Grenville Goodwin (Opler 1973: 11–12, 13, 22). Scudder and Colson, in their long-term Gwembe Tonga fieldwork in Zambia, had an agreement:

> Each would supply a carbon of all field notes to the other and . . . each had the right to publish independently using the total body of information. This agreement still stands and has worked well. Over the years we have shared ideas as we read field notes, talked, and pooled experience. [1979: 234]

Acknowledgments in ethnographies point to similar cooperation. In *Navaho Witchcraft* Kluckhohn cites the fieldnotes of eleven anthropologists (1944: 244–52). Hildred Geertz (1961: 170) acknowledges drawing on her colleague and husband Clifford Geertz's fieldnotes on Javanese families. A for-the-record mention by Evans-Pritchard of others' use of his Nuer fieldnotes a year before his first published article appeared perhaps points to the power asymmetry in student-teacher relationships: "The chapter on the Nuer (Chap. VI) in *Pagan Tribes of the Nilotic Sudan,* by Prof. C. G. and Mrs. B. Z. Seligman, 1932, was compiled from my notebooks" (Evans-Pritchard 1940: 2 n.3; see also vii).[1] Are students ever free to deny fieldnotes to those who sponsor their research? Fieldnote deposition was required of researchers at the Rhodes-Livingstone Institute in then Northern Rhodesia and at the East African Institute of Social and Economic Research in Uganda, although what was done with them by anyone other than their authors is unclear (Richards 1977: 180).

[1] Evans-Pritchard's notes are duly acknowledged by the Seligmans in their book (1932: xiii).

As discussed in "The Secret History of Fieldnotes" (Part III, this volume), the lone ethnographer designing, conducting, and writing up his or her own fieldwork adventure is mainly Malinowskian myth (though true in his case) and post-1960 individual grant practice. Until recent decades there have been more Argonauts than Jasons. Fieldwork in the classical period was less Odyssey than Iliad, organized in programs, projects, schemes, and teams, with larger purposes than those envisioned in single-investigator research designs.[2] After Samoa, Mead collaborated with Fortune in Manus, on the Omaha reservation, and in the Sepik region; and with Bateson and others in Bali and on her return to Manus. Fieldnotes were shared. Many of today's leading American anthropologists are products of organized research efforts. Harvard, home of teams, has housed the Yankee City, Ramah, Values in Five Cultures, Modjokuto, Six Cultures, Chiapas, and Kalahari Research projects. Team projects continue in anthropology, but they were much less central to the discipline in the 1970s and 1980s than earlier.

In team projects, the role of fieldnotes and their circulation varies with project organization. Mead's Bali research was unusual in its multimedia pattern of integration.

> The investigator may make a running record of the behavior of a group of individuals against a time scale. Where cooperative field-work is being done, a parallel photographic or Cine record, or a combination of the two may be added to this. The observations may be parceled out among a number of observers, one taking ceremonial behavior, another informal behavior not immediately oriented to the ceremony, another recording only verbatim conversations, or another following a single individual through the same period. (This is the method which is now being used in our Balinese researches by Mr. Bateson, Miss Jane Belo, Mrs. Katharane Mershon, and myself, with the addition of three trained

[2]In this context, I disagree with Marcus and Cushman (1982: 26) and Van Maanen (1988: 73–74) that the post-1960s personal accounts have "demystified" ethnographic fieldwork. This puts ethnography itself into a timeless "ethnographic present." The emphasis these writings place on *individual* experience and self-knowledge (Clifford 1986: 13–15; Van Maanen 1988: 106–9)—on fieldwork as "rite of passage" in a personal rather than professional sense—are "reflexive" of the decline of fieldwork project dominance since the 1960s, and the ascendancy of government funding of individual proposals. The historical "experimental moment" (Marcus and Fischer 1986) is historically determined.

literate native observers, I Made Kaler, Goesti Made Soemoeng, and I Ketoet Pemangkoe, working in shifting cooperative combinations.) [Mead 1940: 328]

The result of this fieldnoting/photographing/filming was unconventional photograph-based behavioral analysis (see Mead 1970: 258–59, Plates I–XVI; Whiting and Whiting 1970: 309–12). From Mead's similar team fieldwork in Manus in 1953–54 (1956: 495–96), she returned to more traditional fieldnote-based prose ethnography.

Warner's 1930s Yankee City (Newburyport, Massachusetts) project involved eighteen fieldworkers, who produced a wealth of records, informal and ethnographic interviews, and "dictaphone" fieldnotes of observations of events and organized behavior, filed according to categories and subcategories of the family, economic organization, associations, government, churches, and sports. One copy of his or her fieldnotes was retained by each fieldworker, and they all submitted another copy, and weekly and annual written reports of their research. These documents, with the files, were available to other field team members, although direction of the project analysis, involving twenty-five persons, remained in Warner's hands (Warner and Lunt 1941: ix, 44–75). The fieldworkers did not write their own ethnography; Warner was author or senior coauthor of all five resulting volumes.

The control of fieldnotes in Oscar Lewis's Tepotzlan team project was similar. He was sole author of the ethnographic volume resulting from the work of his fifteen-person team; the only separately written sections of *Life in a Mexican Village* (Lewis 1951) are a chapter on Rorschach test results and appendixes on maize and potsherds, none of these written by members of the field team. A much looser arrangement of a more tightly designed three-year research project in white and Indian Minnesota communities allowed "substantial field-work experience for eighteen graduate students in anthropology": the final report of project supervisors Pertti Pelto and J. Anthony Paredes was complemented by six master's theses, two doctoral dissertations, and jointly and separately authored journal articles (Pelto 1970: 270–87).

This model of several coordinated fieldworkers in the same or nearby locations, each writing his or her own ethnographic reports, has marked most team projects from the 1940s through the present. Fieldnote coordination, however, has varied. Kluckhohn's 1939–48 Ramah Navajo project (Lamphere 1979: 22–28) involved a score of researchers, each pursuing individual projects published separately,

though a volume based on project fieldnotes about forty-eight children was coauthored by Dorothea Leighton and Kluckhohn, and Kluckhohn drew on other fieldworkers' notes in *Navaho Witchcraft* (1944). Project fieldnotes were filed at Harvard according to categories devised by Kluckhohn.

In 1948 Kluckhohn's Comparative Study of Values in Five Cultures Project (Lamphere 1979: 28–32)—comparing Navajo, Zuni, Mormons, Texans, and Spanish Americans—began with Rockefeller Foundation support. By its 1953 conclusion, thirty-seven fieldworkers had participated, again with separate projects and publications (a summary volume appeared only in 1966). From the beginning, a common-user organization of fieldnotes was adopted by the project.

> Field notes were typed on ditto masters, and the contents of each page of notes was analyzed in terms of the inventory of culture content devised by the Human Relations Area Files at New Haven. Each item in the inventory has its own code number, and so each page of notes acquired from one to half a dozen numbers, depending upon how its contents were analyzed. A copy of each page of notes was then filed under every content category involved. A participant in the project would then be able to refer quickly to the numbered heading in the file to see what others besides himself had recorded on a large number of predefined subjects. [Gulick 1970: 135n]

All Kluckhohn's Navajo files were moved to the Laboratory of Anthropology at Santa Fe in 1963. The pre-HRAF Ramah notes filed in Kluckhohn's own categories proved difficult for Lamphere (1979: 32) to use for later Ramah research: "It was as if the 'key' to the Ramah Files had died with Kluckhohn. Only hours of digging through 'cut up' field notes revealed facts that might easily have come to light in a conversation with him."

The HRAF categories, not tailored to a caste-divided community, were also used in a 1950s Cornell team study in an Indian village. Fieldworkers had their own projects and typed four copies of their notes for distribution to Lucknow, Cornell, the village field station, and back to the fieldworker. Though notes were available to all project members, including those who joined during later stages, the continual delays in typing up fieldnotes from scratch notes vitiated project intercommunication plans. No one read all the fieldnotes, and informal discussion in the field site proved the most important source of team integration (LeClair 1960; cf. Du Bois 1970: 222–23).

Such communication of headnotes, as well as fieldnotes, was maxi-
mized in the procedures developed by a group of five researchers in a
mid-1970s ethnographic study of San Diego inner city hotels. Paul
Bohannan, the project director, met with two or more fieldworkers at
least every three weeks in "debriefing sessions," where detailed re-
ports on fieldnotes were presented, discussed, and taped. Bohannan
then took notes on the tapes, averaging twenty pages, and indexed
them according to subjects and persons of interest to the project.

> These notes differ significantly from a fieldworker's notes. They con-
> tain not only data, but, clearly demarcated as such, formulations and
> preliminary analyses. Some of these latter points can be suggested to
> fieldworkers, more or less as assignments. Others go back to form the
> protodraft of analysis. [Bohannan 1981: 38]

As the project focus narrowed, life history interviews were conducted,
and indexed by Bohannan according to the same project categories
(1981: 40).

A complex use of fieldnotes marked the Six Cultures project in
which two-person teams and local assistants conducted fieldwork
simultaneously in Kenya, India, Mexico, New England, the Philip-
pines, and Okinawa in 1954–55. In addition to general ethnographic
coverage, they agreed to collect detailed data on child rearing, using a
"Field Guide for a Study of Socialization," which all participated in
drafting and which was later published. Copies of fieldnotes were sent
to Beatrice Whiting at Harvard, who monitored the research (Whiting
1966: vii, ix). The six ethnographic studies, authored by the field
researchers, appeared both in an edited volume in 1963 and separately
in 1966 (see Fischer and Fischer 1966). Each ethnography was based on
the researchers' own fieldnotes, but two analytic volumes were also
published: *Mothers of Six Cultures* (Minturn, Lambert, et al. 1964),
based on formal interviews; and *Children of Six Cultures* (Whiting and
Whiting 1975).

The Whitings' volume analyzes fieldnotes on the behavior of 134
children between ages three and eleven, recorded in five-minute be-
havior sequences, with each child observed fourteen times or more
over the course of several months (Whiting and Whiting 1975: 30–31,
39–42). Except in New England, local bilingual assistants translated
what was said during the five-minute periods. Examples of the field-
notes on which the analysis is based, and of the coding procedures,
were published as well (1975: 187–220).

A unique team project was the study of Elmdon, a village of 321 people, fourteen miles from Cambridge University (Strathern 1981). It was begun in 1962 by Audrey Richards[3] and Edmund Leach as a student fieldwork training exercise; by 1975 nearly thirty anthropologists and other students had participated (Richards 1981). Most stayed for two weeks or less, residing at the home of Richards, who had moved to Elmdon in the late 1950s. They recorded family histories and genealogies; they made notes on casual conversations, village activities, pubs, and meetings. "Interviews were never more than loosely structured. Notes were often taken in the presence of the person talking, or jotted down immediately afterwards. The students usually indicated remarks recorded verbatim" (Strathern 1981: 271). A few students who spent longer fieldwork periods in Elmdon produced reports on local history, housing problems, and farming.

Richards retired in Elmdon in 1964. She also took notes, though intermittently and inconsistently and not with the short-term enthusiasm of a full-time fieldworker:

> The notes I took during a period of over twenty years' residence in Elmdon are not as systematic as those which resulted from two fifteen-month trips to Zambia in 1930–1 and 1933–4. I have, of course, a much richer supply of those stored memories and impressions on which anthropologists rely to give life to their descriptive work. [1981: xx]

In 1975 Richards published *Some Elmdon Families* as a work of local documentation. Still, with seventeen collective notebooks and other documents, she hoped to write

> something like an old-fashioned anthropological village study. . . . But a temporary run of bad health made me doubtful whether I would be able to complete the work. At this stage, Marilyn Strathern . . . offered to analyse our kinship data, which was complex owing to the degree of intermarriage in the village. However, it soon became clear that the book must be hers alone. She had developed very interesting ideas on

[3]In the Ghanaian sense, Audrey Richards was the Queen Mother of social anthropological fieldwork—from her discussion of censuses and quantitative approaches in 1935 and her paper on field methods (and "speech in action") in 1939 through her championing of anthropology in the Colonial Social Science Research Council, her assistance and direction to ethnographers of East Africa during 1950–56 as director of the East African Institute of Social Research, and the Elmdon study to the example of her Bemba and Ganda ethnography (Beattie 1965: 6, 37; Gladstone 1986; Richards 1935, 1939, 1977, 1981).

the phenomenon of the core families which were of greater complexity and originality than my own would have been. *Kinship at the Core* is the result. [1981: xxiii]

Strathern had worked in Elmdon in 1962 and returned briefly in 1977. She also drew on Richards's headnotes—on her "insights and feelings about the village, quite as much as on her extensive data," and on "an invaluable commentary on my first draft" (Strathern 1981: xxxi, xxxiv).

Inheriting Fieldnotes

Few anthropologists have ever assumed the labor-of-love task of producing an ethnography from fieldnotes written by others. When they have done so, it has usually been to complete the work of those who died young—Bernard Deacon, Buell Quain, Robert Pehrson, Grenville Goodwin. Their ethnographic executors did not enjoy access to the original headnotes; they faced problems beyond those of Marilyn Strathern, who had the collaboration of Audrey Richards as well as her own brief fieldwork experience in Elmdon, or Robert Smith, who benefited from the cooperation of Ella Lury Wiswell (Smith and Wiswell 1982: ix–xii; Smith, this volume).

After fourteen months of fieldwork in the New Hebrides, Deacon died in 1927 on the eve of his departure. As Lutkehaus explains, Camilla Wedgwood had no easy task in editing his fieldnotes into *Malekula: A Vanishing People in the New Hebrides*, published in 1934. The notes were sketchy and disorganized, and some of them had also vanished (Langham 1981: 235–36; Larcom 1983; Lutkehaus 1986). "To reinterpret fieldnotes requires knowing something about what was taken for granted when the notes were written—difficult enough for the writer to deal with, let alone another reader" (Van Maanen 1988: 124). Wedgwood's Rivers-influenced Cambridge training, shared with Deacon, provided the intellectual integument for the ethnography (Larcom 1983; see also Langham 1981: 212–41); however, Larcom, whose 1974 fieldwork was among one of the groups with whom Deacon had worked, argues that a better approximation of Deacon's evolving headnotes was contained in his letters from the field.

Quain died in Brazil in 1939 after four months of fieldwork among the remote Trumai Indians the year before. His handwritten field-

notes, records, and journal (Murphy and Quain 1955: 1) were typed
by his mother and turned over by Quain's friend Charles Wagley to
Robert Murphy, who never knew Quain. Murphy faced the same
dilemmas that Smith experienced upon receiving Wiswell's notes.

> It soon became clear that ordering and editing were not enough [even
> though] the notes were rich in detail and insights. . . . he would have to
> read and re-read the notes, learn the names of the numerous informants
> and other individuals mentioned there and in the diaries, identify them
> as to age, sex, status, family membership, etc., familiarize himself with
> place names and Trumai terms just as a field investigator would have to
> do. [Wagley 1955: v–vi]

The headnotes Murphy brought to the writing were a combination
of his own fieldwork experience among the Mundurucu Indians of
Brazil, a theoretical orientation, and what he could glean from the
fieldnotes of Quain's headnotes (Wagley 1955: vi): "It is impossible . . .
to so neatly separate the Murphy from the Quain in this monograph,
for Quain's interests and ideas have influenced my interpretation of the
data" (Murphy and Quain 1955: 2). Murphy used the notes to formu-
late descriptive prose, quoting from them directly only once (1955:
95–96). The book was published under their joint authorship.[4]

When Robert Pehrson died in the field in 1955 (Barth 1966), Jean
Pehrson, who had shared the fieldwork with her husband, typed 200
pages of his chronological fieldnotes on the Marri Baluch nomads of
Pakistan. With letters, two papers by Jean Pehrson, and texts, they
were turned over to Fredrik Barth, who had also received half a
dozen field letters from his friend Pehrson. Yet despite their detail, the
notes remained opaque, and Barth found writing from them frustrat-
ing—until in 1960 he spent five weeks in the locale where the well-
remembered Pehrsons had worked. Their informants' knowledge of
Pashto, which Barth had learned in his Pakistan fieldwork among the
Pathans, made communication easy. With his own headnotes, Barth
found Pehrson's fieldnotes "more tractable" and writing possible. For
besides his own Marri Baluch fieldnotes, Barth concludes,

> clearly I had also accumulated data of other kinds, which were not
> recorded in the Pehrsons' notes but which are needed in anthropological

---

[4]Lévi-Strauss also drew upon Quain's fieldnotes for a contribution to the 1948
Handbook of South American Indians (Murphy and Quain 1955: 83).

analysis. . . . I believe [these critical supplementary data] are mainly connected with the concrete "stage" or setting in which social life takes place: the sizes of habitations, the uses of space, the physical as well as the conventional opportunities for communication. . . . The interpretation of actions, both in a strategic means-ends perspective and as messages of communication, depends on this knowledge, and case material remains highly ambiguous when it is lacking. [Barth 1966: x–xi]

With these physical coordinates, which Pehrson took for granted, now in mind, Barth wrote *The Social Organization of the Marri Baluch,* using Pehrson's materials (rather than his own fieldnotes) and quoting liberally from them in the text. The book was accordingly presented as "by Robert N. Pehrson, compiled and analyzed from his notes by Fredrik Barth" (Pehrson 1966).

The job of Keith Basso in editing Grenville Goodwin's fieldnotes for publication was much simpler than that faced by Wedgwood, Murphy, or Barth. Goodwin, who did fieldwork among the Apache of Arizona during the late 1920s and 1930s, had written *The Social Organization of the Western Apache* (1942) and several papers before he died in 1940 (Basso 1971: xi–xii, 3–25). Goodwin had outlined further monographs, and his widow, Janice Goodwin, organized the remaining fieldnotes and supervised their typing from longhand. The published volume (others are planned), *Western Apache Raiding and Warfare* (Basso 1971), consists of six verbatim narratives of elderly informants transcribed in 1931–32, plus other informant statements on several topics that Goodwin had used to organize his notes. These are highly readable texts, without Boasian linguistic literalism. Nonetheless, the considerable number of Apache terms used in the notes led Basso to conduct ten weeks of linguistic fieldwork (he had done earlier research in other Apache groups) to authenticate cultural translation. Historical rather than ethnological in aim, Goodwin's materials provide an Apache view of the unrest between the 1850s and the completion of United States pacification in 1890.

As more anthropologists return, like Lutkehaus, Lamphere, and Larcom, to scenes of earlier ethnography, and as we ask new questions about the discipline's history, access to fieldnotes will become more important. The archival homes of the papers of Cushing, Boas, Rivers, Malinowski, and Mead were not difficult for scholars to find, but the notes of other anthropologists are scattered (Kenworthy et al. 1985: 5–6; Raspin 1984). The process of archiving one's own fieldnotes and papers is an issue of uncertainty, ambivalence, and presumption for

most ethnographers (see Ottenberg's and Wolf's essays in this volume). There are also practical issues—paper quality, preservation measures, the range of documents that make a useful collection—which few think about early enough (Kenworthy et al. 1985: 1–3, 10–11, and passim).

There remains the problem of how to preserve headnotes. More documentation of the stage coordinates that Barth identifies might help others make sense of fieldnotes. So, no doubt, do the letters from the field (as Larcom found for Deacon), those preliminary written releases of what Lederman terms the "sense of the whole" component of headnotes. Certainly, also, would more reminiscences of fieldwork tied to professional as much as personal aspects: that is, to writing in the field as well as to rapport and self-discovery. But the primary locus for the preservation of headnotes should be in their joint productions with fieldnotes: in published ethnography, the whole point of why fieldwork is done.

REFERENCES

Adair, John. 1960. A. Pueblo G.I. In Casagrande 1960, 489–503.
Adams, Richard N., and J. Preiss, eds. 1960. *Human Organization Research*. Homewood, Ill.: Dorsey.
Barth, Fredrik. 1966. Preface. In Pehrson 1966, vii–xii.
Basso, Keith, ed. 1971. *Western Apache Raiding and Warfare: From the Notes of Grenville Goodwin*. Tucson: University of Arizona Press.
Beattie, John. 1965. *Understanding an African Kingdom: Bunyoro*. New York: Holt, Rinehart & Winston.
Bohannan, Paul. 1981. Unseen Community: The Natural History of a Research Project. In Messerschmidt 1981, 29–45.
Briggs, Jean. 1970. Kapluna Daughter. In Golde 1970, 17–44.
Casagrande, Joseph B., ed. 1960. *In the Company of Man: Twenty Portraits of Anthropological Informants*. New York: Harper Torchbooks.
Clifford, James. 1986. Introduction: Partial Truths. In *Writing Culture: The Poetics and Politics of Ethnography*, ed. James Clifford and George E. Marcus, 1–26. Berkeley: University of California Press.
———. 1988. *The Predicament of Culture: Twentieth-Century Ethnography, Literature, and Art*. Cambridge, Mass.: Harvard University Press.
Comaroff, Jean, and John L. Comaroff. 1988. On the Founding Fathers, Fieldwork, and Functionalism: A Conversation with Isaac Schapera. *American Ethnologist* 15:554–65.
Du Bois, Cora. 1970. Studies in an Indian Town. In Golde 1970, 219–36.

Evans-Pritchard, E. E. 1940. *The Nuer*. Oxford: Oxford University Press.

Fischer, John L., and Ann Fischer. 1966. *The New Englanders of Orchard Town,* *U.S.A.* Six Cultures Series, vol. 5. New York: Wiley.

Foster, George M. 1979. Fieldwork in Tzintzuntzan: The First Thirty Years. In Foster et al. 1979, 165–84.

Foster, George M., Thayer Scudder, Elizabeth Colson, and Robert V. Kemper, eds. 1979. *Long-Term Field Research in Social Anthropology.* New York: Academic Press.

Freilich, Morris, ed. 1970. *Marginal Natives: Anthropologists at Work.* New York: Harper & Row.

Geertz, Hildred. 1961. *The Javanese Family: A Study in Kinship and Socialization.* Glencoe, Ill.: Free Press.

Gladstone, Jo. 1986. Significant Sister: Autonomy and Obligation in Audrey Richards' Early Fieldwork. *American Ethnologist* 13:338–62.

Golde, Peggy, ed. 1970. *Women in the Field: Anthropological Experiences.* Chicago: Aldine.

Goodwin, Grenville. 1942. *The Social Organization of the Western Apache.* Chicago: University of Chicago Press.

Green, Jesse, ed. 1979. *Zuni: Selected Writings of Frank Hamilton Cushing.* Lincoln: University of Nebraska Press.

Gulick, John. 1970. Village and City Field Work in Lebanon. In Freilich 1970, 123–52.

Howard, Jane. 1984. *Margaret Mead: A Life.* New York: Fawcett Crest.

Kenworthy, Mary Anne, Eleanor M. King, Mary Elizabeth Ruwell, and Trudy Van Houten. 1985. *Preserving Field Records: Archival Techniques for Archaeologists and Anthropologists.* Philadelphia: University Museum, University of Pennsylvania.

Kimball, Solon, and William Partridge. 1979. *The Craft of Community Study: Fieldwork Dialogues.* Gainesville: University Presses of Florida.

Kluckhohn, Clyde. 1944 [1967]. *Navaho Witchcraft.* Boston: Beacon Press.

Lamphere, Louise. 1979. The Long-Term Study among the Navaho. In Foster et al. 1979, 19–44.

Langham, Ian. 1981. *The Building of British Social Anthropology: W. H. R. Rivers and His Cambridge Disciples in the Development of Kinship Studies, 1898–1931.* Boston: Reidel.

Larcom, Joan. 1983. Following Deacon: The Problem of Ethnographic Reanalysis, 1926–1981. In *Observers Observed: Essays on Ethnographic Fieldwork,* ed. George W. Stocking, Jr., 175–95. Madison: University of Wisconsin Press.

LeClair, Edward, Jr. 1960. Problems of Large-Scale Anthropological Research. In Adams and Preiss, 28–40.

Lévi-Strauss, Claude. 1955 [1974]. Trans. John and Doreen Weightman. New York: Atheneum.

Lewis, Oscar. 1951. *Life in a Mexican Village: Tepoztlan Restudied.* Urbana: University of Illinois Press.

Lutkehaus, Nancy. 1986. "She Was *Very* Cambridge": Camilla Wedgwood and

the History of Women in British Social Anthropology. *American Ethnologist* 13:776–98.

Marcus, George E., and Dick Cushman. 1982. Ethnographies as Texts. *Annual Review of Anthropology* 11:25–69.

Marcus, George E., and Michael M. J. Fischer. 1986. *Anthropology as Cultural Critique: An Experimental Moment in the Human Sciences*. Chicago: University of Chicago Press.

Mead, Margaret. 1940. *The Mountain Arapesh, II: Supernaturalism*. Anthropological Papers 37:317–451. New York: American Museum of Natural History.

———. 1956. *New Lives for Old: Cultural Transformations—Manus, 1928–1953*. New York: Morrow.

———. 1970. The Art and Technology of Fieldwork. In Naroll and Cohen 1970, 246–65.

———. 1972. *Blackberry Winter: My Earlier Years*. New York: Morrow.

———. 1974. *Ruth Benedict*. New York: Columbia University Press.

Messerschmidt, Donald A., ed. 1981. *Anthropologists at Home in North America: Methods and Issues in the Study of One's Own Society*. New York: Cambridge University Press.

Minturn, Leigh, William Lambert, et al. 1964. *Mothers of Six Cultures: Antecedents of Child Rearing*. New York: Wiley.

Murphy, Robert F., and Buell Quain. 1955. *The Trumai Indians of Central Brazil*. Seattle: University of Washington Press.

Naroll, Raoul, and Ronald Cohen, eds. 1970. *A Handbook of Method in Cultural Anthropology*. New York: Columbia University Press.

Opler, Morris, ed. 1973. *Grenville Goodwin among the Western Apache: Letters from the Field*. Tucson: University of Arizona Press.

Osgood, Cornelius. 1940. Informants. In *Ingalik Material Culture*, 50–55. New Haven, Conn.: Yale University Publications in Anthropology.

Pandey, Triloki Nath. 1972. Anthropologists at Zuni. *Proceedings of the American Philosophical Society* 116: 321–37.

Paul, Benjamin. 1953. Interview Techniques and Field Relationships. In *Anthropology Today*, ed. A. L. Kroeber, 430–51. Chicago: University of Chicago Press.

Pehrson, Robert H. 1966. *The Social Organization of the Marri Baluch*. Comp. and ed. Fredrik Barth. Viking Fund Publications in Anthropology 43. New York: Wenner-Gren Foundation for Anthropological Research.

Pelto, Pertti J. 1970. Research in Individualistic Societies. In Freilich 1970, 251–92.

Powdermaker, Hortense. 1966. *Stranger and Friend: The Way of an Anthropologist*. New York: Norton.

Raspin, Angela. 1984. A Guide to Ethnographic Archives. In *Ethnographic Research: A Guide to General Conduct*, ed. R. F. Ellen, 170–78. San Diego: Academic Press.

Read, Kenneth E. 1965. *The High Valley*. New York: Scribner.

Richards, Audrey I. 1935. The Village Census in the Study of Culture Contact. *Africa* 8:20–33.

———. 1939. The Development of Field Work Methods in Social Anthropology. In

The Study of Society, ed. F. C. Bartlett et al., 272–316. London: Routledge & Kegan Paul.

———. 1977. The Colonial Office and the Organization of Social Research. *Anthropological Forum* 4:168–89.

———. 1981. Foreword. In Strathern 1981, xi–xxvi.

Scudder, Thayer, and Elizabeth Colson. 1979. Long-Term Research in Gwembe Valley, Zambia. In Foster et al. 1979, 227–54.

Seligman, C. G., and Brenda Z. Seligman. 1932. *Pagan Tribes of the Nilotic Sudan*. London: Routledge.

Shah, A. M. 1979. Studying the Present and the Past: A Village in Gujarat. In *The Fieldworker and the Field: Problems and Challenges in Sociological Investigation*, ed. M. N. Srinivas, A. M. Shah, and E. A. Ramaswamy, 29–37. Delhi: Oxford University Press.

Smith, Robert J., and Ella Lury Wiswell. 1982. *The Women of Suye Mura*. Chicago: University of Chicago Press.

Stanner, W. E. H. 1960. Durmugam, a Nangiomeri. In Casagrande 1960, 63–100.

Strathern, Marilyn. 1981. *Kinship at the Core: An Anthropology of Elmdon, a Village in North-west Essex in the Nineteen-sixties*. Cambridge: Cambridge University Press.

Van Maanen, John. 1988. *Tales of the Field: On Writing Ethnography*. Chicago: University of Chicago Press.

Wagley, Charles. 1955. Foreword. In Murphy and Quain 1955, v–ix.

———. 1977. *Welcome of Tears: The Tapirape Indians of Central Brazil*. New York: Oxford University Press.

Warner, W. Lloyd, and Paul Lunt. 1941. *The Social Life of a Modern Community*. New Haven, Conn.: Yale University Press.

Whiting, Beatrice. 1966. Introduction. In Fischer and Fischer 1966, v–xxxi.

Whiting, Beatrice, and John Whiting. 1970. Methods for Observing and Recording Behavior. In Naroll and Cohen 1970, 282–315.

———. 1975. *Children of Six Cultures: A Psycho-Cultural Analysis*. Cambridge, Mass.: Harvard University Press.

Whitten, Norman E., Jr. 1970. Network Analysis and Processes of Adaptation among Ecuadorian and Nova Scotian Negroes. In Freilich 1970, 339–402.

Whyte, William Foote. 1955. Appendix: On the Evolution of "Street Corner Society." In *Street Corner Society*, enl. ed. 279–358. Chicago: University of Chicago Press.

———. 1960. Interviewing in Field Research. In Adams and Preiss 1960, 352–73.

Wolcott, Harry F. 1981. Home and Away: Personal Contrasts in Ethnographic Style. In Messerschmidt 1981, 255–65.

Wolff, Kurt. 1960. The Collection and Organization of Field Materials: A Research Report. In Adams and Preiss 1960, 240–54.