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## 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 occurrence." 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 and Fieldnotes*

"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 headnotes. 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 Village*.

Several of the authors in this volume comment on the headnotes-fieldnotes relationship. Jean Jackson mentions that for many anthropologists, 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 milieu." 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).<sup>1</sup> 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

<sup>1</sup>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; Powdermaker 1966: 95; Wolff 1960: 241).

### Scratch Notes to Fieldnotes

This second stage of fieldnote production is epitomized in the photograph 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 circumstances 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 informants, 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 oversimplified 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 "questionnaires 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.<sup>2</sup> 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

<sup>2</sup>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 possessions, 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, employment, 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 prestations.

In a valuable account of William Partridge's fieldwork in Colombia, the precise points at which systematic records emerged from fieldnotes 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 Partridge's fieldnotes (Kimball and Partridge 1979: 131, 172).

The balance between fieldnotes and records is unique in each research project, and most if not all anthropologists produce both kinds of documents. Many ethnographers would probably feel uncomfortable 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 record 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 Honigsmann (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 Suyé 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 catharsis. . . . 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, diatribes 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 revived 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).<sup>3</sup> 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

<sup>3</sup>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 publish 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 biography (1984) for an understanding of Mead's career in anthropology. "Letters written and received in the field have a very special significance. 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 circulating 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 fieldnotes 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 "occasionally 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 roneoed 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<sup>5</sup> 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.

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# *Examples of Fieldnotes*

AND COLLEGE LIBRARY

Canoe magic

(5)

Most of the magic associated with building canoes belongs to Zabebia - is that concerned with making the canoe light to pull & launching the canoe. Tala also knows the coconut magic for cooking the canoe. (Magawa). A/ + Tenua says Ababa knows the mwabwa cooking magic, wh if I (a ♀) were to hear I I shd die (I think to really believe this - the threat of death is not a conscious means merely of keeping ♀ in the dark).

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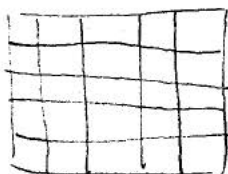
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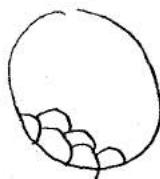
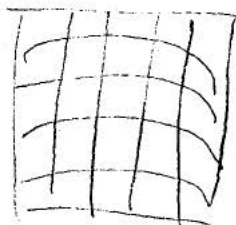
There are two roof making groups in the village: one in Imamura consisting of Kitagawa old man, Sakaguchi and two Kurohiji (Chokichi's brother and Kumaichi).

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 <sup>horizontal</sup> ~~vertical~~ 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

Sept.30.

Today is 5th of Aug. o.c. - jugoya ~~kayax(thexfall)xxxx~~.

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. Kōda 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 young 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 <sup>(house)</sup> 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 incanted 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; Ikaru, etc.). Here list under General only items that do not seem to fit well elsewhere.

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Reincarnation (See Ikaru, especially genealogies, obanji, <sup>Phoebe</sup>diviners, etc.)

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

# VILLAGE-GROUP AGE GRADES

See also VILLAGE-GROUP

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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; I 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:

ñiwó nómohoko pakho ~~kat~~ Estribina

ñe wadego pakho ~~kat~~ ko / ñeno wadega eahani

G said entity of mohoko 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.

katunihy  
note: ~~kunike~~ 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 Maku. The girl, Isiria, danced. Others looked at them. Girls giggled that old women'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 ~~xxx~~ 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 -- ~~ye~~ ya tia.

~~xxxxPundiaep~~

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
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

Figs: 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 va 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

Timben 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 Bang). 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 ~~to~~ 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. Timben was angry because Nare (M's 2'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. Wage 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? Tue (who's been sitting around the fire with Wage + me this morn) says two here are known for it: Kilawa 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...)

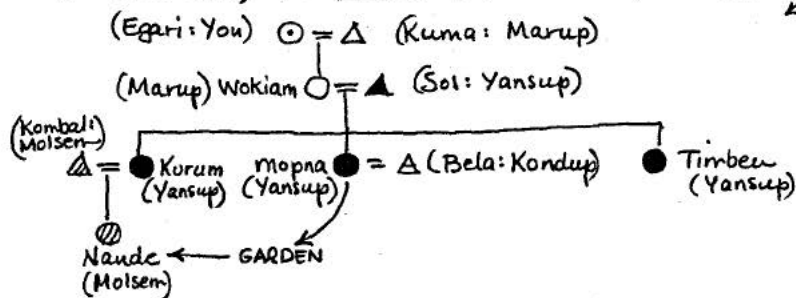
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Wage reported that when his 2D in Pimanda



got married yesterday, she was given K1700 of nopao (i - 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 it's 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 it's a Yansup garden (MF's group): effectively, 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 it's Kuma! ↘



See typed pp., Poza interview for this date on Keyosem's changed affiliation.

Nande wanted Alin to hear her konaen which

G. n Ankrab's mother was visiting M/M Q today. He introduced 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 works at the Atomic Energy 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 brought shorts to be fixed by an itinerant tailor who set up his machine in front of the Q house.

T gave me data on the family and funeral affairs.

Mrs. Selo 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 because he says the Ghana matches are no good. He said they would not sell to him because they thought he was a cop. Ben did not get any either. Alex said because 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 discussed 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 to kin and friends studying.

Mr. A came with a friend who is from Ewodze and works for the VRA at Akosombo; he is visiting in Accra. He works in the fisheries and studied fish farming 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 misses most from US was pie, esp. lemon pie. He said Accra is too fast and expensive for him.

He said you can't tell tribe of women by dress in Ghana because the Ga and Akan and Ewe women all dress alike (also true in Lome.) "Even by the face you can't 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 border, and they go to each others markets. Some of the border speak Ewe. He said their names are either Ewe or Ga. The Ewes he said have very distinctive names.

"Our names are very different."

We talked about dress differences in men's traditional clothes. The Akans don't 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 Ewes along the Volta river, eg. Sogakofe, have a very difficult dialect for other Ewes to speak. Mrs. A said Adama, head of the opposition speaks this dialect, from Sogakofe.

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 neighbor 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 Hispnic 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.)