Preface

After a long American Ethnological Society board of directors session on the first day of the 1984 American Anthropological Association meetings in Denver, I met Shirley Lindenbaum, editor of *American Ethnologist* and a fellow member of the board, in the hotel lobby. We were later joined by James Clifford, a historian of anthropology. The three of us talked about current concerns in anthropology, including the growing interest in ethnographies as texts.

By eleven o'clock in the evening we were all hungry and decided to eat in the hotel. We descended several flights to the one restaurant that was still open. The service was slow and uncoordinated. As Lindenbaum and Clifford sat eating their dinner and I sat waiting for mine, Clifford brought up the subject of fieldnotes. He said that in all the recent discussion about writing ethnography and about ethnographies as writing, no one had addressed what anthropologists write *before* they write ethnographies—fieldnotes. This led our conversation to a chain of associations, comments, and ideas about fieldnotes and about why ethnographers have written so little on the subject.

When I learned at the next day's AES board meeting that I was to chair the program committee for the AES Invited Sessions at the Washington AAA meetings, in 1985, I immediately thought of doing a panel on fieldnotes. In the next two days, I discussed this with Linden-

baum and AES president-elect Judith Shapiro, both of whose ideas on themes and potential panelists were extremely helpful. Clifford also was amenable, and willing to do a paper.

The next step, early in 1985, was to write to a score or so of anthropologists, to gauge their interest in joining the Washington symposium. I approached people of substantial ethnographic accomplishments, people I believed would be able to reflect upon such work. Some I knew well; others only slightly; some not at all. My bait was a sketch of topics and issues to consider:

Unlike historians, anthropologists create their own documents. We call them fieldnotes, but we speak little about them to each other. This symposium seeks to open up discussion about fieldnotes within the profession. The aim is less to concretize what various theoretical schools think ought to go into fieldnotes than to examine what anthropologists do with fieldnotes, how they live with them, and how attitudes toward the construction and use of fieldnotes may change through individual professional careers.

We hope contributors will present a variety of perspectives. Among the topics that might be considered are these:

1. What are the relationships between fieldnotes and ethnographies? Are ethnographic writings written "from" fieldnotes, from fieldnotes plus other sources, or does one or more intermediate stage of writing follow between fieldnotes and ethnographic product? How do fieldnotes provoke and animate memory?
2. What are the different "kinds" of fieldnotes an ethnographer produces—running accounts of events, texts, reports, impressions, and other forms? How do these fit together in providing the first-stage ethnographic record?
3. When they are available, what is the impact of earlier ethnographers' fieldnotes on later researchers? Should access to such fieldnotes be a regular process of professional courtesy? If so, why, and how; if not, why not? How successfully may one ethnographer's fieldnotes be used by another in writing ethnography?
4. How does an ethnographer "live with" fieldnotes over time? What sense of responsibility to one's notes do anthropologists feel? Do fieldnotes become a burden from which one must win freedom before going on to new work? How long can fieldnotes remain useful to an anthropologist? How does the ethnographer's reading of her or his own fieldnotes change with professional development and maturity? Can ethnographic writings become "obsolete" but fieldnotes remain a source for new ethnography?
5. How do ethnographers in return visits change their conceptions of what fieldnotes should be? How do such conceptions change as anthropologists take on second or third fieldwork projects?

6. How is access to fieldnotes handled when two or more ethnographers work cooperatively—in team research, or in parallel investigations?

7. What uses may be made of fieldnotes—directly—as part of ethnographic writing? How do canons of scientific method, responsibility to informants, and desires to write persuasively and authoritatively all intersect in the use of fieldnote material?

8. Should fieldnotes become available to anyone (including non-anthropologists) other than the ethnographer? When; to whom; in what forms?

I appended a list of useful sources, including Srinivas's *Remembered Village* and Pehrson and Barth's book on the Marri Baluch; papers by Clifford, Larcom, and Marcus and Cushman; and the collection of essays on field research edited by Foster and others.

The bait worked. Fourteen contributors prepared papers, and eleven of those papers, in revised form, are included in this volume (the press of other commitments prevented Emiko Ohnuki-Tierney, Triloki Nath Pandey, and Michael Silverstein from revising their papers for inclusion here).

The topic of fieldnotes proved to be timely. A report on the symposium followed in the *Chronicle of Higher Education* immediately after the Washington meetings (see Ellen K. Coughlin, "Anthropologists' Archives: Scholars Examine the Problems and Possibilities of Field Notes," December 18, 1985, pp. 5, 7). In the next three years, several works on ethnographic writing appeared, some dealing directly and some indirectly with the uses of fieldnotes (including books by Agar, Clifford, Clifford and Marcus, Friedrich, Geertz, Marcus and Fischer, Van Maanen). Pointed and controversial pieces on ethnographic writing by Richard Shweder in the *New York Times Book Review*—"Storytelling among the Anthropologists," September 20, 1986, pp. 1, 38–39; "The How of the Word," February 28, 1988, p. 13—provoked reactions in anthropological circles and beyond.

When my work on this collection of essays began in 1985, a two-decade mix of theoretical, political, methodological, and fieldwork experiences had primed my thinking about the role of fieldnotes in doing anthropology. I teach at Queens College in Powdermaker Hall, named for Hortense Powdermaker, who taught there for many years. Soon after I arrived in 1972, I read her book *Stranger and Friend: The Way of an Anthropologist* (1966). What stayed with me most from this fascinating personal history was the sense of drudgery involved in

diligently typing up fieldnotes from handwritten notes on observations and informants' statements.

This resonated with the feelings I had during my own fieldwork in Adabraka, Ghana, in 1970–71. I had kept a small notebook in my back pocket, a suggestion made by Lambros Comitas in a field training seminar in 1965. I wrote in this notebook all kinds of things seen and heard and struggled to keep my typing from it up to date. It resulted in 397 single-spaced pages of fieldnotes covering eighteen months, although the last one hundred pages were not typed until the Watergate summer of 1973, a year after my Ph.D. thesis was completed. That thesis and my publications on Adabraka since have been based on nearly as many pages of network interviews and other systematic records, kept separate from my wide-ranging fieldnotes. The notes remain to be used, someday perhaps, in as yet unbegun ethnographic writing.

The attention to records in my Adabraka fieldwork and writing is a product of my times. I first did fieldwork in 1965 in Bahia, Brazil, as part of a Columbia University undergraduate summer program. This was preceded by a field training seminar, led by Comitas and Marvin Harris, in which the focus was on the practicalities of getting to and around in Brazil, and on establishing rapport with informants. Several students from the previous year's program spoke about their experiences. The assigned reading from Adams and Preiss's *Human Organization Research* washed over me. (*Stranger and Friend* was not yet published, nor were Epstein's *Craft of Social Anthropology* and Jongmans and Gutkind's *Anthropologists in the Field*; they would appear in 1966 and 1967, and begin the flood of fieldwork and methods literature in the 1970s.) I was more concerned about Brazilian ethnography and the ethnoscience literature to which my planned fieldwork on racial vocabulary related. Others in the Brazil group I met that year—Dan Gross, Maxine Margolis, David Epstein, Conrad Kottak, Betty Kottak—mentioned Lévi-Strauss's *Tristes Tropiques*, but I did not read it then.

In Brazil I took no fieldnotes; I tried, but had no idea of what to write. Instead, I collected records of interviews, and responses to a set of drawings of varying combinations of skin color, hair, and nose form. This led to my second published paper.¹ My first, written in the

¹Brazilian Racial Terms: Some Aspects of Meaning and Learning, *American Anthropologist* 73 (1971): 1126–43.

aftermath of the Columbia revolt of 1968, captured the intertwined concerns with ecology, underdevelopment, political engagement, science, and method which influenced me in my 1967–69 graduate student days.² It reflected Harris's teaching and writing. I was especially impressed by how his field encounter with racial inequality in Moçambique had led to his thinking about the emic-etic contrast, and how this in turn shed light on understanding the sacred role of the cow in preventing even greater immiseration in India.

Politics, science, and rigorous data-gathering were all one piece for me. My concerns about race, ethnicity, and class were crystallized in my Adabraka research on whether "tribe" or class was more important in daily life, and in my interest in testing the plural society separatist thesis that M. G. Smith and others had applied to Africa. The careful study of the daily life and interactions of Adabraka women and men which Lani Sanjek and I conducted was rooted in Harris's *Nature of Cultural Things* (1964), a theoretical book that I read as having political implications.

I was also strongly influenced by what I saw as a parallel interest in charting interaction among the Manchester anthropologists. Comitas had turned me on to British social anthropology, in which I read widely. I was fortunate also to work with Jaap van Velsen at Columbia in 1968. His *Politics of Kinship* (1964) was a demonstration of how careful fieldnotes on actual behavior could be analyzed to throw light on larger questions of process and social structure; his paper in the Epstein volume, and Epstein's own 1961 paper on network analysis, I saw as a next step from Harris's theoretical approach. Thus, detailed attention to daily activity marked both my Adabraka network records and my fieldnotes. George Bond and Allen Johnson, who came to Columbia in 1968 and served on my dissertation committee, reinforced this combination of intellectual elements for me. The importance of dedicated perseverance in fieldwork was also impressed upon me by Simon Ottenberg, who was in Ghana while we were.

The mix of political concern, respect for systematic data, and methodical attention to detail, which I have tried to make evident in my Adabraka publications,³ has continued to be important to me. This

²Radical Anthropology: Values, Theory, and Content, *Anthropology UCLA* 1 (1969): 21–32.

³What Is Network Analysis, and What Is It Good For? *Reviews in Anthropology* 1 (1974): 588–97; Roger Sanjek and Lani Sanjek, Notes on Women and Work in Adabraka, *African Urban Notes* 2, no. 2 (1976): 1–25; New Perspectives on West African

mix also marked my writing in the 1970s about the employment of women in anthropology.⁴ The victory achieved on this issue, however, had as much to do with the political experience I gained in 1976–78 at the Gray Panthers' Over 60 Health Clinic in Berkeley⁵ as with methodological and quantitative skills. My two years as an applied and advocacy anthropologist in Berkeley, however, produced few written fieldnotes, though they did result in a large file of other documents.

I continued as a Gray Panther activist on health, housing, ageism, and economic justice through the 1980s after I returned to New York.⁶ In 1981 I decided to write a book about the Berkeley Gray Panthers and their health clinic. I discovered, however, a treasure of documents on the origins and history of the Gray Panther movement at the Presbyterian Historical Archives in Philadelphia, and my plan shifted to a study of the national organization, with the local Berkeley story as one chapter. In working on this project in 1981–82 and the summer of 1985, I reflected often that the documents were my fieldnotes. Though I had not been present at the formative 1970–76 events detailed in them, I knew all the major actors, had seen the places where events

Women, *Reviews in Anthropology* 3 (1976): 115–34; Cognitive Maps of the Ethnic Domain in Urban Ghana: Reflections on Variability and Change, *American Ethnologist* 4 (1977): 603–22; A Network Method and Its Uses in Urban Ethnography, *Human Organization* 37 (1978): 257–68; Who Are “the Folk” in Folk Taxonomies? Cognitive Diversity and the State, *Kroeber Anthropological Society Papers* 53/54 (1978): 32–43; The Organization of Households in Adabraka: Toward a Wider Comparative Perspective, *Comparative Studies in Society and History* 23 (1982): 57–103; Female and Male Domestic Cycles in Urban Africa: The Adabraka Case, in *Female and Male in West Africa*, ed. Christine Oppong, 330–43 (London: Allen & Unwin, 1983); Maid Servants and Market Women's Apprentices in Adabraka, in *At Work in Homes: Household Workers in World Perspective*, ed. Roger Sanjek and Shellee Colen (Washington, D.C.: American Ethnological Society, 1990).

⁴The Position of Women in the Major Departments of Anthropology, 1967–1976, *American Anthropologist* 80 (1978): 894–904; Roger Sanjek, Sylvia H. Forman, and Chad McDaniel, Employment and Hiring of Women in American Departments of Anthropology: The Five-Year Record, 1972–1977, *Anthropology Newsletter* 20, no. 1 (1979): 6–19; The American Anthropological Association Resolution on the Employment of Women: Genesis, Implementation, Disavowal, and Resurrection, *Signs: Journal of Women in Culture and Society* 7 (1982): 845–68.

⁵Anthropological Work at a Gray Panther Health Clinic: Academic, Applied, and Advocacy Goals, in *Cities of the United States: Studies in Urban Anthropology*, ed. Leith Mullings, 148–75 (New York: Columbia University Press, 1987).

⁶*Crowded Out: Homelessness and the Elderly Poor in New York City* (New York: Coalition for the Homeless and Gray Panthers of New York City, 1984).

occurred, and had participated in similar events in Berkeley, in New York, and at national Gray Panther meetings. To me, the process of building ethnographic description and analysis from these documents was similar to van Velsen's extended case method and to the account of Adabraka life which I had built more quantitatively in my network analysis dissertation and in papers.

My thinking about fieldnotes was stimulated, as well, by the part-time, long-term fieldwork I began in Elmhurst-Corona, Queens, in late 1983, which continues at present. In this incredibly diverse neighborhood of established white Americans of several ethnic backgrounds, newcomers since the late 1960s have included Latin American and Asian immigrants of many nationalities, Black Americans, and white not-quite yuppies. With a team of researchers as diverse as the local population, I have been studying changing relations among these varied groups. My fieldnotes cover mainly meetings of political bodies and associations, public festivals and ceremonies, and services and social occasions at three Protestant churches, with scores of descriptive accounts of events in each of these three categories. My chronological fieldnotes to date amount to 930 single-spaced pages, with more notes from ethnographic interviews. My analyses of these three domains begin with the fieldnotes. They have more in common with van Velsen's extended case approach than with the quantitative analysis of behavioral records of my Adabraka network study. Political concerns about the future of racial, ethnic, and class differences continue to give meaning and purpose to this work.

Most of the revised essays for this volume reached me during 1986. In June of that year my father died, and in the following two years several responsibilities overtook me. It was not until 1988–89 that I was able to return to the introduction for *Fieldnotes: The Makings of Anthropology*. Once started, the introduction seemed to take on a life of its own; it is now divided into essays that address the issues raised in each of the book's five sections.⁷ The eleven other authors are not entirely blameless for this extended "introduction." They raised so many compelling issues that adequate treatment of the wider literature

⁷For reading and commenting on sections of my contribution to this book I thank Lani Sanjek, David Plath, Robert J. Smith, Simon Ottenberg, Peter Agree, Linda Wentworth, James Clifford, Nancy Lutkehaus, Rena Lederman, Carol Greenhouse, David Holmberg, Judith Goldstein, Moshe Shokeid, and Jean Jackson, who always sent just the right signal at just the right time.

and context—the proper job of an introduction to an edited collection—proved a formidable task. With their joint examination of anthropology from fieldnotes “up” rather than from theory “down,” the whole history of the discipline looked different. Theoretical concerns were very much present, but they were extended to include questions of “when theory,” “where theory,” “why theory,” in addition to “which theory.”

Several writings have been extremely helpful to my work on this book. They include Clifford Geertz’s “Blurred Genres: The Refiguration of Social Thought,” in *Local Knowledge: Further Essays in Interpretive Anthropology*, pp. 19–35 (New York: Basic Books, 1983); Peter C. W. Gutkind and Gillian Sankoff’s “Annotated Bibliography on Anthropological Field Work Methods,” in *Anthropologists in the Field*, ed. D. G. Jongmans and P. C. W. Gutkind, pp. 214–72 (New York: Humanities Press, 1967); Nancy McDowell’s aptly titled “The Oceanic Ethnography of Margaret Mead,” *American Anthropologist* 82 (1980): 278–303; and several essays by the historian George W. Stocking, Jr. —anthropology is blessed that he has devoted his professional attention to our discipline.

But most valuable of all are the essays in this book by George C. Bond, James Clifford, Jean Jackson, Allen and Orna Johnson, Rena Lederman, Nancy Lutkehaus, Christine Obbo, Simon Ottenberg, David Plath, Robert J. Smith, and Margery Wolf. Every one of the authors surprised me, doing much more than I expected, revealing sides of themselves to the world, or dealing with themes and issues far beyond what I imagined back in 1984. As editor, I am honored. As reader, you are in for a treat.

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