



# REFLECTIONS ON ORDERLY AND DISORDERLY ETHNOGRAPHY

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This paper discusses the eroding conceptual and geographic distance between research and daily life, between the home and the field. I focus in particular on the important role played by new tools and devices for recording, transforming, keeping and communicating an ever-growing corpus of information, references, images, sounds and ideas. In the process of changing the way we imagine the process of doing ethnography, and the technologies we use to manufacture ethnographies, we have inadvertently become much more like journalists, expatriates and even tourists, which requires us to engage in new kinds of boundary maintenance.

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This paper addresses the changing practice of ethnography, and the porous, sometimes tenuous boundaries between professional life and the everyday business of being human. Other social scientists have written about the shifting and uncertain ground on which “field” researchers stand in a world where geography is uncertain and positionality is rapidly changing (e.g. Gupta & Ferguson 1997; Till 2001; Voloder 2008). In this article I focus in particular on the way that recording media, computers and ubiquitous telecommunications have helped move us from a situation when the division between “the field” and “home” was clear and comfortable, into a state of uncertainty where geographic categories are arbitrary and artificial. I end in a strange place, where ethnography has almost disappeared as a separate professional practice, and has instead become part of the daily practice of living in a complex world full of information. Instead of being something akin to searching or asking questions, to exploring new

territories and nosing about, my ethnography now seems more like the constant continuous process of digestion, the rumbling, mostly unconscious and sometimes uncomfortable activity which takes material, extracts something, and gets rid of the rest. I suggest that this change in the nature of ethnography is partially due to my own growth as a person and a scholar, at the same time that it also reflects profound changes in the nature of the world itself, and the technologies of practicing ethnography.

My own engagement as an anthropologist started with an archaeological field school in Spain in 1970, when the whole attraction was *getting away* from the mundane world of home to a warm and exotic location, from a troubled present into an exotic past. I continued to pursue archaeology to Israel, the southwestern US deserts, and Belize right up through graduate school, comfortable with the idea of “the field” and “the lab” as separate locations for particular and specialized activities, spaces where par-

ticular kinds of knowledge were extracted. But in the political tumult of the time, as my own commitment to environmental and social justice issues firmed and deepened, I became unhappy with the lack of connection between the anthropological archaeology I was learning to practice and the obvious and fundamental problems of the world outside the tents of the field camp. The fracture took place in 1976, while I sat in the mud at the site of an ancient Maya city, watching my trenches fill up with the torrential downpours of the rainy season. A farmer from a nearby village sat with me, and innocently asked why, if I was interested in helping people, I was digging holes in the ground instead of walking over to his village where there were plenty of problems.

When I went back three years later to do my dissertation research on the problems of development and culture change, I had made the emotional and political connection between my life and my work, but I still had a very clear idea that there was one world of “the field” and another world which was home. This was an easy distinction to make because I found getting from one place to the other so difficult – one of the first villages I worked in required a long drive down muddy and rutted roads to a small town, hours of terrifying riding on the ocean in a dugout canoe, and a half a day’s arduous hike on muddy trails over jagged hills. I worked in a foreign language, lived in a thatched hut, and once a month I went to town to collect mail and make a few phone calls through a noisy line, which did more to emphasize the distance from home, than to bridge it. I definitely felt like I was living a different life, and this was reflected in the way I took notes on everything, both formally in the form of field notes and informally in a series of field journals. My biggest problem was that there never seemed to be any time when I was *not* doing ethnography. I woke up with curious little faces of children peering through the sticks that formed the walls of my house. The only time I was “off duty” were the quiet hours after the rest of the village was asleep, when I sat up typing my field notes, and swatting the flies attracted to my kerosene lantern, while listening to the BBC on a shortwave radio. Even leaving the village for a trip

to town was no relief – I was still observing, taking notes, talking with officials, gathering grist for the dissertation mill. Fieldwork only ended when I left Belize and went back to graduate school in Arizona, to begin the mysterious period called “writing up.” The re-entry process was marked by a liminal feeling of culture shock, as my body, emotions and daily habits readjusted to a completely different pace and routine.

There was a direct parallel between my ethnographic experience and my prior archaeological fieldwork. We went to another place to generate “data” which we brought back home to a workplace, where we did “analysis” leading to publication. Ethnographic field notes were like a corpus of excavated artifacts, drawings of section walls, photos of objects in situ, upon which a kind of scientific magic was to be performed, in order to transform them into a form of knowledge acceptable for an audience entirely separate from the field. I expected that once out of the field, I was completely cut off from the people I worked with in the field, most of whom were illiterate and did not get mail – they might as well have been living in an ancient city in ruins. Still, they were living in my mind’s eye, and I felt a kind of responsibility and debt to these communities and individuals, which transcended space and separation, something more than the very abstract and tenuous sense of connection I had previously experienced when rooting through the remains of the dead. The sense of political commitment and obligation was furthered by close friendships I had made in Belize, particularly with two Belizean anthropologists, Joseph Palacio and Harriot Topsey. A decade before the anthropologist Lila Abu-Lughod declared herself a “halfie,” a person who shared both the professional identity of an anthropologist and a cultural affiliation with the subjects of anthropological research (1991), I was already learning that the division between “home” and “the field” was more complicated and politically charged than any of my teachers had led me to expect.

The conceptual division between home and field allowed for a neat progression which mirrored industrial production: gathering raw materials,

processing them, then dividing them into chapters, like little retail packages which could then be sent off to journals, the shopping malls of academia, where they would hopefully find “buyers.” In practice the stages were never as neat as the conceptual model which generated them, so I wrote several papers in the field, and throughout the analysis and write-up I was gathering more information all the time from reading, from sending letters back and forth to friends in Belize, and from conversations with other anthropologists. Publication took many years, and overlapped with additional short periods of fieldwork in Belize.

Just as in any other culture or kinship system, however, deviations from the norm never led me to question received wisdom. My generation of fieldworkers was sent off with no formal training in methods, beyond the casual advice we could pry from our teachers and older colleagues, and the archaic colonial-era advice contained in the pages of “Notes and Queries in Anthropology” (published in many editions by the British Association for the Advancement of Science and the Royal Anthropological Institute). We actually believed the legendary accounts of people like Malinowski and Evans-Pritchard, who portrayed fieldwork as a mystical process akin to travelling to the heart of darkness. Each of us resurrected and reinvented ethnography from the ground up, from an imaginary template. But we were going out into a different world from that experienced by our elders, one where some of our informants had lived in the United States, or were listening to American baseball games on the radio, where we could no longer fool ourselves into thinking we were finding vestiges of an “untouched” precolonial world. Instead, we were thrust unprepared into all the complexity of an advancing stage of globalization where Coca-Cola was no longer an exotic import, but a part of daily life. Some friends from a British Army camp dropped into the village on my birthday, carrying gifts of ice-cold Heineken, a barbecue grill, and hot dogs. When I went to town I could sit in the Chinese restaurant and watch fuzzy videotaped American television programs on the set behind the bar.

My fellow graduate students, and the other ethnographers I met in the field, all had very different kinds of field experiences.<sup>1</sup> Some seemed to spend their time drinking in bars, or sitting calmly around comfortable kitchen tables, while I was struggling up and down steep jungle slopes chopping trees with a sweaty machete and trying to keep my notebook from getting soaked in the tropical rain. But we all knew that we went “away” to a place called “the field” to do ethnography, and then we came back and we wrote an ethnography based on our fieldwork. To a remarkable extent, graduate programs in North American universities still instill this idea in their sociocultural anthropology students, preserving the essential ritual of separation, fieldwork, and reincorporation through the ritual of writing the dissertation as a personal narrative of voyage, learning and enlightenment. Even though an increasing number of graduate students are *from* “away” places, and despite the fact that more and more North American students choose to do research at “home,” the normative model still tells them that they have to go “away” in order to be welcomed back into the fold as professionals. Students whose projects are far from the ideal can have a very difficult time getting funding, and many of them feel like they live under a dark cloud of stigma, as if they are breaking unwritten rules.

### Readjusting to Reality

I have continued to do a good deal of fieldwork in Belize, and took a job there as an applied anthropologist for about a year and a half in the 1980s. But like most of my university colleagues, I have also developed a series of research projects on current issues in the communities in the USA where I live and teach. Some of these have started as class projects, teaching exercises on topics as diverse as the culture of lawn cultivation in Indiana, multiple-roommate consensual households in Northern California, and an “authenticity field trip” to a tourist town to see how anonymous commodities get indigenized and authenticated by shopkeepers. As the fashion for “service-learning” has developed at Indiana University, my classes have also done applied research

projects with a local food-bank, and with an environmental organization working for more sustainable waste disposal. As the lines between academic and applied research become more indistinct, and I find that the problems and issues I study in Belize are mirrored and replicated in the part of Indiana where I spend most of the year, the very basis of ethnographic practice has warped out of all recognition, as in a fun-house mirror.

Today I cannot tell you where ethnography begins and my daily life ends. I am always taking notes, writing papers, talking to and e-mailing people I might consider part of my “research,” including my spouse and my offspring; I interview “informants” who are also friends, some of whom are also colleagues. My e-mails are part of my record of fieldwork, and my notes can no longer be divided into field and home, since they include reminders to buy groceries, pasted sections of articles from the *New York Times*, and snatches of television jingles. One young man from a Belizean village came to Indiana and lived with us for half a year while attending classes and getting his high-school equivalency diploma. His niece was our first goddaughter in the village, and later one of my graduate students married his first cousin. Does this make us relatives? It feels like it. Another of my graduate students did her dissertation fieldwork among Belizeans living in Chicago, some of whom came from the same village where I was working at the time. Because the opportunity for an interesting conversation is unpredictable, I have recorded several field interviews on my cell phone, where they are linked to my Google calendar, which has my teaching schedule, reminders to call my doctor, lectures and workshops at the university, and deadlines for papers like this one.

I now own a house in Belize near the place where I once lived in a thatched shack, and the house has high-speed Internet (most of the time), cable television, and a cell phone. This leads me to ask if I am doing “fieldwork” while I am lying on the verandah drinking iced tea and writing a draft of this paper? What about the day I took notes on the food I ate in a local restaurant at lunch, and snapped a photo in the grocery store which may turn up in my next

book? What is happening when my student from Belize, who grew up just a few kilometers from where I did my dissertation research, comes in to my office at Indiana University for a discussion of the ethnographic field school we are planning to run in Belize? These hybrid experiences lead me to constantly ask myself if I am just slowly and belatedly recognizing the artificiality of the boundaries between life and work, the indeterminacy of what is near and far, home and away. Or has the world really changed in fundamental ways during the course of my research career, compressed in time and space as in Harvey’s description of the geography of postmodernity (1991)?

A number of anthropologists have been arguing that globalization, new forms of media, and accelerated travel and migration have both dissolved and complicated the separation between “the field” and “home.” We can also blame the blurring of the boundary on the rising number of indigenous and “halfie” anthropologists, who no longer respect the difference between home and field, since their home is our field. The classifications which once made ethnography and sociocultural anthropology into identifiable genres of writing and activity have also lost their clarity, with the rise of ethnographic-like travel writing, geography and film. Popular television programs on food, science and travel often follow the same plots and conventions as ethnography. In the USA we have Anthony Bourdain engaging in food ethnography every week in a different exotic location, learning the local dishes and explaining how they are embedded in indigenous and national cultures, rooted in history and nature. Our own efforts to popularize and demystify ethnography and anthropology have had similar effects. Other disciplines have adopted ethnography as a legitimate method, even if what they do under this banner would hardly be recognizable to Malinowski. Corporations can buy ethnographic services to help them understand their own workplace culture, their markets, the people who consume their products, and to hunt for new “cool” innovations and styles.

I would argue, however, that changes in the technology of doing fieldwork and managing informa-



tion have had a more dramatic effect in blurring the distinction between when we are “doing ethnography” and when we are doing something else. When I did my dissertation research, I had several distinct ways of recording and communicating information. I typed my formal field notes every night on a manual typewriter, keeping carbon copies and mailing the originals back to the USA every time I went to town. I used a cassette recorder to record stories, long meetings and complicated conversations which I could translate and transcribe later, and I usually carried a pen and a small pad for notes. I had a notebook for my maps and notes when I was mapping fields and villages, a card file where I kept everyone’s names and kinship relationships, a sheaf of survey forms for my household census, and my field notebooks where I sketched and kept my personal reflections. A calendar on the wall recorded major events, rainfall, floods and my trips in and out of the village, and I wrote long letters to my advisor and to friends describing my fieldwork experiences. I kept notes on all the articles and books I had taken with me to the

field as well, notes which seemed somehow out of place in my fieldnotes, but I did not have anywhere else to put them.

All of this record-keeping stopped when I got back to the University of Arizona to write my dissertation. I started a new set of notes and notebooks for the records of my analysis and write-up, most of which took the form of files, which went in my file cabinets, along with notes I took on the things I was reading, ideas for analysis and drafts of chapters, and the copies I made of relevant articles and publications. I organized my field census forms into notebooks, indexed them with colored tabs, and appended hand-tabulated data summaries, mostly in the form of tables. Each step created a further conceptual distance between “raw” field information and the processed and digested ingredients which could become ethnography. Each layer of interpretation pushed the immediacy of field experience further into the distance, denaturing the informality of my actual experience, increasing the sense of separation between the routine of



everyday life and the exceptional state of engaging in fieldwork.

I finished and defended my dissertation in 1981, and over the next few years new technologies infiltrated my fieldwork practices, and I began new projects, all of which had the effect of blurring the divisions between home and field. My next ethnographic research involved interviews with families in the town where I lived in California. It was confusing, leaving “work” after a lecture or a faculty meeting to go to “the field” at a neighbor’s house and record an interview, but the results still went in separate physical and computer files, even if the actual practice of fieldwork was thoroughly jumbled up with everyday life. In other words, despite the complications of practice, I was still able to impose an artificial order based on my own cultural classifications. This order was premised on the absolute separation between an extraordinary disciplined process of inquiry called fieldwork, ideally supported by grants, and my everyday working life which at the time consisted of teaching for a wage. In this scheme, the intermediate stages where fieldwork was analyzed and written up for publication occupied a liminal position, which corresponded quite well to the actual fact that my teaching duties were an absorbing full-time job, leaving me only evenings and weekends for my research and writing, and precious little time for anything else.

I did not notice the boundaries of these categories actually crumbling till my next fieldwork trip to Belize in 1990. Instead of an isolated rainforest village, I lived in cities and towns, studying the consumer culture of the middle-class majority, as it was expressed in daily practices of shopping, entertainment and meals, and in the public culture of restaurants, music, and public festivities like beauty pageants, fairs, and holidays. I carried a notebook all the time, and again found that fieldwork encompassed every single activity of the day. I actively eavesdropped on shoppers in stores, and quizzed the clerks when I did my own shopping. I interviewed the woman whom we hired to take care of our baby daughter, and visited her family. I intercepted my neighbors on the way to the shop, followed them, and recorded our conversa-

tion as we moved around the store picking up different goods. The people across the street invited me to dinner, which I described in an article in the *American Anthropologist*. I always picked up hitchhikers when I drove anywhere, stopping after I dropped them off to furiously write notes, and at parties I would run to the bathroom to jot down things people said about their drinks, the food we were eating, and the taste of their cigarettes. When I read the newspaper I tore out and scanned hundreds of articles, and when I listened to the radio I found myself rushing around for a pen and pad. At night I talked things over with my partner and then sat down at the keyboard to get the thoughts down before going to bed. Within a few years, constant e-mail was added to the task of fieldwork – I was the first person in the country with an e-mail account, and figured out how to get a modem working through a 5-watt mobile phone about the size of a car battery.

This time when I got “home” to the USA, there was no single box or place to put the “field,” since most of my notes now resided on the same computer where I had e-mails, personal and professional correspondence, lectures and classroom assignments, and games. But more dramatically, I could no longer maintain the conceptual or symbolic division between my personal life and my professional existence, between the things I wrote and recorded as ethnography, or teaching, or personal affairs. I still kept things in separate boxes, files and categories, but this was gradually revealed to be no more than an arbitrary attempt to impose order on a heterogeneous jumble. Rather than reflecting an ontological quality of the world, I started to recognize that classifying and sorting the artifacts of work and leisure is no more than *housekeeping*, the same thing we are always trying to do when we sort our socks, put clothes away in a closet, keep receipts and financial records, and organize our books on shelves.

The increasingly arbitrary nature of the way I was classifying my own activities were evident in a whole series of small dilemmas. Was I reading a particular book for pleasure, or as part of my work? Was not my work supposed to be a pleasure at least some of the time? At the extreme ends of the spectrum it was

easy to make distinctions between keeping books by Derrida in my office, and a stack of pulpy science-fiction novels on my bedside table. But then there are moments like a recent night, when the novel I was reading had a wonderfully clear description of a gift economy. I folded the page, and in the morning remembered to use my pen scanner to enter it into *Evernote* (an online database file organizer), creating a file in the notebook where I keep all the bits and pieces which I hope to use in the next edition of an economic anthropology text.

Then there are blogs, newspapers, journal articles, e-mails and all the other grab bags of information and opinions, some of which may find their way into a lecture, a paper, an interview with a journalist or a conversation, each of which may be recorded, archived, found and used, appearing in an infinite number of possible works in the future. I use the same bibliographic software and indexing programs for all of my writing, including interview transcripts and field notes as well as poems, lectures, diaries, angry letters to the local newspaper, my wedding vows, articles about headache cures, and the receipts for repair work done on our house foundations. It is all just digital grist. The division between the personal and the professional is just as tenuous as the difference between public and private; my Facebook page is a tablet open to friends who know nothing about anthropology, family members who do not admire my profession, as well as colleagues who are friends and vice versa. The way fieldwork and life have converged and interdigitated no longer seems terribly uncomfortable, but it makes some of the things which worried anthropologists in the past seem kind of silly – what will happen when the people we do fieldwork with read what we write? What is the difference between writing for academics and writing for a “popular” audience? How does our writing objectify and isolate culture, fencing it off in an artificial time and place? E-mailing back and forth with the children of the Q’eqchi’ farmers I did dissertation research with 35 years ago, I do not even pause to think about what particular writing genre I am contributing to, or the obstacles to intersubjectivity. For my last book I engaged with members of a

Belizean bulletin board, and quoted some of the responses to my questions and provocations. Was this fieldwork, requiring a signed consent form, or was it public discourse which can be used without permission? The nineteenth-century classification of the world into discrete spaces separated by boundaries does not conform to a twenty-first century reality.

One of Bourdieu’s enduring insights was that every time we classify and sort objects in the real world, we are also classifying and reifying categories of people, defining our own identities (1984). When the conceptual boundaries which define the site of our own research are undermined, it therefore challenges the legitimacy of the ethnographer. This fundamental reflexivity means that we are no longer so easily defined by what we do in the world, or by our specialized knowledge and expertise, particularly in places where the label “anthropologist” has no particular referent (or is mistaken to mean someone who studies insects or static native cultures).

In Belize I find myself being lumped together with other American expatriates, some of whom have been living and working in the country for 30 or 40 years. What do I have in common with this heterogeneous band of real-estate sellers, travel agents, dive instructors and organic farmers? This begs the following question: what makes anthropology different from learning a place and people well enough to do thick description, to write “ethnographically?” I have recently interviewed some of these expatriates, prompted by reading three different locally-published books written by foreigners about their experiences of long-term living in Belize, which explain many things they have learned about its people and places. This has sharpened my questions, since it is clear that many expatriates know many aspects of the country better than I ever will. They have married into local families, operated businesses, tried to make a living through farming or fishing, and never had the safety net of a university job, while I have just dropped in like a privileged observer from time to time.

I cannot say that some particular methodology or scientific sensibility distinguishes what I do from the activities of some expatriates. One of my expatri-



ate friends is a journalist who has done thousands of interviews, and another has spent thousands of hours collecting recipes, watching people cook, and recording the vocabulary and variety of indigenous cuisine. They keep notes, read books, clip things from newspapers, participate in online discussions, follow the work of archaeologists and other scholars working in their country, and even collect historical documents. Some have archives that rival my own huge collection of Belize trivia, cookbooks, government documents, postcards, reprints, and yellowing newspapers. I find myself sometimes thinking of these people as my peers, people who share my obsession with understanding the country, rather than subjects or objects of research. And as the number of Belizeans trained in anthropology continues to rise, along with the general educational standards throughout the country, it is easy to adopt the same relationship with many of the people I meet in the course of my research/life in Belize. In one memorable encounter, I visited a small village in northern Belize in 1984 to do a survey as part of a project to anticipate the economic and cultural effects of building and improving rural roads and bridges. I sat down with the Mayan chairman of the village council, and identified myself as an anthropologist, to which he responded: "Oh. Are you a structural-functionalist, a Marxist or a Boasian?" We chatted about the relative merits of different anthropology graduate programs, and he was happy to answer my questions once he had accepted my credentials.

### Redefining Ethnography

If we cannot define ourselves by our long residence in, or attachment to a particular place; if our methodology no longer distinguishes us from other academics, or a horde of market researchers, government officials and social workers; if we are no longer students who go "off to the field" to study exotic people, bringing home the spoils for a small specialist audience, can we at least be defined by our product, the *ethnography* itself? There is no question that some of us still write those canonical and syncretic portraits of a people and a place, with titles like "The Nuer" and "The Gypsies." But if you look at the cur-

riculum vitae of the average practicing anthropologist or ethnographer in the USA today you will find that very few have ever written a synthetic ethnographic monograph about a culture or ethnic group. That means that most of us are doing ethnography without ever actually writing an ethnography, to the point where the noun and the verb have only a loose connection to each other, so the noun has become an adjective: ethnographic. Most scholars these days publish a series of topical and theoretical papers and chapters, and if they write a monograph it tends to be topical or oriented towards an issue, rather than the depiction of a particular time or place. We write with multiple co-authors for an often multidisciplinary audience, and some who fancy themselves "public anthropologists" (as opposed to what?) address popular venues and professional audiences where their work can have an impact beyond small groups of specialists.

A generation of anthropologists have been working to expand the genres of legitimate writing to include autobiography, dance, music, films, and novels, so surely the way we write, or the places we publish do not determine the nature of our enterprise. As I have just explained, those methods have now become so diffuse and various, everything from eavesdropping to taking snapshots, that they have no necessary relationship to any particular way of seeing or describing the world.

The uncertainty about home and field has broken down the difference between ethnographers and ethnographees, and the balance of power between professional social scientists and amateurs, tourists and journalists. At the same time the influx of new technologies for information storage and retrieval has turned "ethnographic data" into an arbitrary subset of all the things we know and record. Is there anything about the practice of ethnography itself which remains the same, which we can still point to as something which is not commonplace to intellectual reflective life in the twenty-first century?

Looking back from my own individual perspective, the persistent characteristic of ethnography seems to have become invisible, receding into an Oz-like place behind-the-curtains. Ethnography

cannot be found in the practice of fieldwork, but in the attitude and values one brings to many different kinds of work. The visible techniques of asking, answering, recording are much less important than the hidden technologies of distancing, re-learning, seeing the commonplace as unusual, finding telling details and gaps, an awareness of qualities and categories in addition to numbers and things. Ethnography cannot rightfully be contained by methodology, but its unique quality informs methodology at every turn. The tools allow us to combine distance with intimacy, and reflection with participation, in ways that make the experience and product dramatically different from tourism and guidebooks.

For these reasons, seeking the essence of ethnography in methods or practices is futile. Instead, the complex passage of knowledge to and from our encounters with other people are the essence of ethnography, the things which make it a special category of experience. We bring with us the voices of other scholars, the concerns and thoughts and theories which create the context in which we can open ourselves to experience. And we process and interpret all our interactions with other people through this same “anthropological lens” (Peacock 1986). So rather than being in a place, ethnography is a directional flow, a series of movements which involve filters, modes of storage and connection, pattern recognition, and juxtaposition. As Löfgren and I have argued, metaphors are central to the process, structuring both what we see, and how we interpret and represent what we have seen (2006).

Understanding ethnography in this way should bring renewed attention to the parts of the anthropological project which are less dramatic than fieldwork itself. These “missing pieces” are evident in the prospective designs which many of us write for the purpose of getting money to support our research. Having read hundreds of grant proposals, it is clear that they are a genre of fictional narrative reifying the artificial stages of knowledge production, the sequence of question–research–answer, enshrining the separation of fieldwork from home which I have criticized above. In the formal world of the proposal, the section called “literature review” is the

place where intellectual preparation appears, where the authors describe the questions which motivate them, and enumerate the ancestors and peers who they wish their work to converse with. Nevertheless, this section rarely states the explicit epistemological assumptions which inform the project – we often have to read between the lines and look for subtle cues of language and reference in order to divine the authors’ approach at its broadest level. How has their ethnographic vision been shaped by their reading and training? What kinds of people, experiences, and knowledge are they going to find significant and worth recording?

If we can usually figure out what the ethnographers are taking with them to their project from a proposal, it is always much harder to understand how they plan to bring their experience back. To be sure, there is always a “methodology” section which describes a series of activities, settings and people. We usually learn that there will be some combination of formal methods, interviews and “participant observation.” But this increasingly elaborate semi-fictional narrative describing the discrete stages of fieldwork often ends with a strangely curtailed sentence or short paragraph to the effect that “I will analyze my data and write it up (as a report, dissertation, book).” I would suggest that this unnamed movement between recording and writing is really where the most interesting things are going on, though it is rarely documented or discussed.

This explains a puzzle which has bothered me in twenty years of teaching courses on ethnographic research methods. No matter how clear the explanation, or explicit the cookbook, students still find ethnographic methods disturbing, mysterious and difficult. I now believe this is because we are only teaching them the equivalent of growing and gathering food, and expecting them to figure out for themselves how to cook it into a delicious meal. We give them “field methods” and let them read finished ethnographies and articles, but we never really explain how one thing became another. Ethnographers may, in their monographs, tell us where they lived, whom they talked to, and what questions they asked, but they rarely tell us what happened when they sat

down to write, and how they made a narrative out of their mess of notes, charts, photographs and sound recordings. We train people for “the field,” that antiquated construction, and not for the process of creating ethnography, which is so much more than methodology.

I began this essay with the metaphor of *digestion*, for this stage, but digestion is an autonomous process which requires no volition or knowledge. Cooking is a better metaphor for the mixture of intention and accident which moves us from experience to performance, for the core of what makes ethnography different from travelling or studying and makes me different from my expatriate neighbors. It takes the unruly business of life through a series of operations which produce an ordered narrative. It is how we make sense of our own words and questions. It is not so much a stage as a process, and in reality it is always going on, because we are never simply recording what we see like cameras or voice recorders. We are interpretive instruments, and we are engaging with ethnography when we move any experience from our senses to our pen or keyboard. This is not quite what has been written about so grandly and ambitiously as the translation (Rosaldo 1989), description and interpretation (Geertz 1973), or writing (Clifford & Marcus 1986) of an entity called “culture.” Instead I see the heart of the enterprise as something more prosaic and continuous, a semidisciplined process of what Löfgren calls “managing overflow,” channeling the overabundance of information, events, and impressions into something which appears to make sense.

As the division between ethnography and everyday life is revealed as a cultural creation, we have to recognize the universality of many of these ordering processes. In many ways, what we do as ethnographers is exactly the same thing everyone is doing in a mass-mediated, information-saturated and highly textual environment. We have to engage in a constant triage, managing the overflow by sorting things out and putting them in places where we can find them, developing means of searching, prioritizing, and linking. Scholars are like many other kinds of information workers in this respect, and the flow

of messages and potentially relevant information shows no sign of slowing down.

This recognition gives us the grounds for rethinking what is special about the ethnographic process, even when it lacks the arbitrary ordering of discrete stages and bounded spaces. Ethnography is in this sense very much like cooking, a skilled process of taking many ingredients – some intentional and some fortuitous – and turning out something palatable and pleasing. The alchemy of the kitchen is a good metaphor for the wellspring of ethnography, for it combines art and science, self-expression with the desire to please an audience. Like cooking, ethnography also always brings us into direct contact with broader and larger issues, the political economy of production, the ethics of practice, and the constraints both of our ambitions and the circumstances of time and location.

### Note

- 1 I had the strange experience of doing my dissertation at a time when southern Belize seemed to be crawling with ethnographers. A student of Napoleon Chagnon worked in a village about 10 km to the north, and a student of Fredrik Barth was about 12 km to the south, while a medical anthropologist and a semiotic linguist were based in the coastal town about 30 km to the east.

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