

IMPROVISING THEORY

Process and Temporality in Ethnographic Fieldwork

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The University of Chicago Press
Chicago and London

hand, recognizing, of course, that a vast body of excellent work already exists in this area. It should be emphasized that my effort to think through questions of the ethics and politics of ethnographic field research are certainly not intended as a review of the literature on methods. I write in a practical spirit directly to the issues that were generated by our correspondence.

I will suggest ways of conceptualizing ethnographic field research as a complex process entailing (at least) three always co-present kinds of practice (see Moore 1978; Whitehead 1967, 90). Thus, ethnography, understood here as situated, long-term, empirical field research (as opposed to its other meaning as a genre of writing and a practice of representation), is simultaneously *a critical theoretical practice*, *a quotidian ethical practice*, and *an improvisational practice*. I conclude with reflections on process and temporality in ethnographic fieldwork.

Ethnography as a Critical Theoretical Practice

Much has been written in favor of inter- or cross-disciplinarity in anthropology and many other fields. Major funding agencies for social research are often particularly encouraging of projects demonstrating interdisciplinarity. In scholarly publishing, too, interdisciplinary work is valued. Programs and rosters of conferences organized around the world reveal a similar pattern. Graduate students in many doctoral programs are encouraged to do interdisciplinary course work, or they pursue such a route as a matter of course, as Allaine did. These contemporary trends fit ill and yet coexist with other institutional realities, and specifically this: the major and institutionally strongest departments in contemporary universities in the United States and elsewhere are still organized around long-standing disciplines. The system of awarding doctoral degrees still shapes scholarly knowledge production in disciplinary ways. When graduate students become job applicants in the academy, they are usually expected to hold a doctorate in the discipline to which they are applying. One of the most expedient mechanisms for reducing the pile of job applications facing a search committee is to weed out applicants with a doctorate in another discipline. The social and professional costs of doing interdisciplinary work are thus potentially very high for graduate students and untenured faculty. The tight market in academic jobs can become fearsome to negotiate, and there are also constant worries about the recognition and misrecognition of one's work in one's home discipline and outside it.

This raises formidable questions about advising or helping graduate students to pursue inter- or cross-disciplinary work, questions that continually preoccupied me as I worked with Allaine in her student days. This is what

I thought: is it ethical and responsible to encourage a student from outside my own department and discipline to conduct ethnographic fieldwork when her own discipline, political science, has traditionally valued and employed a very different set of research methods? What are the stakes for her, and do I have a realistic understanding of them? True, Allaine had herself pursued interdisciplinary linkages in the university long before she met me. But it is one thing to read widely and quite another to base one's entire dissertation research on an alien repertory of research techniques—techniques, moreover, that required more than a year of her time in a degree program that did not have such an extra year built into it. In deciding to pursue ethnographic research (or “real and serious fieldwork,” as I think I put it to her at the time), Allaine would be out of step with her department in a number of ways.

It was evident that she had too strong a mind to be pushed into a project not of her own choosing, and that she had trained herself well—as a political scientist and a scholar—precisely through her own intellectual nomadism. It was also clear that she was highly regarded by her own faculty in political science at the University of California at Irvine; this undoubtedly afforded her more room for maneuvering than a different student might have had.

I was very interested in her project and aware of my desire and curiosity to see how it would materialize, but this was not sufficient to absolve me of worries: was she heading for treacherous or even un-navigable waters? Indeed, her advisors in political science worried about the same thing, and reasonably so. If, upon completion of the project, she produced a creature for which there was no appropriate cage, what would happen to her? Was I in some sense treating her as an “experimental doll” (Marks 1987)? The very form of this question suggests a passive role for Allaine, and I protest: It wasn't so! Even when we first met, we were more like colleagues than like teacher and student. While Allaine was an advanced graduate student at the time, I was still an untenured Assistant Professor, not many years beyond my own Ph.D. degree. Our different disciplinary “homes” and the fact that I was not her dissertation advisor probably contributed to the informality and ease of our relationship. I was also continually learning from our conversations together. She has never been a passive doll awaiting animation or instruction. Looking hard at the situation, however, the institutional and micropolitical hierarchies were there, and Allaine was structurally vulnerable. My relief and joy were great when Allaine's completed dissertation won the American Political Science Association (APSA) award for best dissertation in the category Race, Ethnicity, and Politics (1997); these feelings were compounded when her first academic position and the transformation of her dissertation into a book (Cerwonka 2004) seemed to come so painlessly. But the outcome might have been different, and that is why the old Enlightenment

adage, "knowledge for the sake of knowledge," seems rather toothless at actually lived interdisciplinary crossroads.

Allaine's disciplinary-nomadism (or transgressions, or poaching, or auto-didacticism, depending on the angle of view) involved intellectual negotiations, but also important political and social tasks of translation. In these negotiations and translations, many different sorts of issues surfaced, and chief among them were conventional understandings of "the scientific method" and of "social science." Her advisors put a hard but productive battery of questions to her (and to me, even if more discreetly): Why do ethnography? What can it accomplish that other strategies of knowledge production cannot? Is it empirical? And even if empirical, are its findings significant and *generalizable*? To answer the misgivings of the unconvinced was a challenging and serious task that Allaine and I have each tried to honor in our different ways. My brief reply follows.

I regard ethnographic field research as an invaluable and in certain respects superior mode of knowledge production among other powerful modes of knowledge production. Ethnography has never been a failed form of empiricism, "almost a science," the thing one does if one has no head for numbers or cares more about "feelings" than "facts." The conventionalized alignment of quantification with "science" and words with "not-science" is obviously spurious but still startlingly widespread. That is why I address it here. It is one of the many guises in which "the scientific method" can haunt social research as a "default mode of knowledge production," as Allaine once described it. ("The scientific method" can be characterized as an expectation that research should ideally follow a single, generalized logic of inquiry, often described in terms of the testing, or "falsifying," of hypotheses. The idea that even the natural sciences can be described in these terms is dubious. [See, e.g., Putnam 1978; Latour 1988; cf. Whitehead 1967].)

Malinowski, Boas, and other pioneers of anthropology pursued empirical research that they never hesitated to regard both as scientific and also as interpretive and experience-based. Only a later, narrower notion of science made ethnography "soft"—a "non-science" or a "not-quite-social-science." And only an even more recent misreading allows the ethnographer's principled, situated pursuit of social and cultural understanding to be styled as a "post-modern" or "relativistic" turn away from "science." Ethnographic field research, after the very early era of armchair ethnography (see Kuklick 1997), has always been both empirical research and a form of critical theoretical practice. This is not a recent development, postmodern or otherwise. It is knitted into the very backbone of anthropology. It is our tradition. It is perhaps useful to remind ourselves of this by reviewing some much older reflections on method in anthropology.

Edmund Leach's 1967 essay "An Anthropologist's Reflections on a Social Survey" is a close examination of a particular study, *The Disintegrating Village*, by Sarkar and Tambiah (1957). Based on a statistical survey of fifty-eight villages in Pata Dambara, Ceylon (now Sri Lanka), this study was regarded by Leach as a landmark work in Ceylon social studies. While he richly praised the work, the bulk of Leach's commentary consists of a lucid, carefully argued critique of the limitations of the social survey in "field sociology." His engagement was with far-reaching "principles of method" (Leach 1967, 76) rather than simply with the Pata Dambara survey. The histories of the scholars involved here are especially interesting to me because the sociological work of young Stanley Tambiah in the 1950s little resembles his brilliant, subsequent ethnographic and theoretical work in anthropology. He was (and is still) an invaluable teacher to me, among many others of his students. And Tambiah and Leach were to become, of course, close colleagues at Cambridge in the 1960s.

This is how Leach begins to map out the consequential differences between what he calls, too simply, "anthropological" and "sociological" methods:³

In [the] area of field research, the differences between sociology and social anthropology do not lie in theory but in method. As a consequence of the fact that the principal research tool of the field sociologist is a command of statistics, it has become a necessary feature of sociological investigation that the "results" should be expressed in numbers. It follows that the units of sociological investigation must always be entities which can be expressed in numbers. [...] I would be the last to suggest that statistical investigations are necessarily mistaken in aim or application, but they are certainly limited in scope. It is my thesis that there is a wide range of sociological phenomena which are intrinsically inaccessible to statistical investigation of any kind. It is in this area of *non-statistical social fact* that the social anthropologist is professionally expert. (Leach 1967, 76–77; emphasis added)

Not all sociologists work with statistical surveys, of course—and there are anthropologists who rely on surveys—but in the present context it is analytically clarifying to foreground the premises, techniques, and products of statistical survey research. Leach's close attention to analytical premises in social anthropology and statistical sociology is also instructive: "The sociologist and the social anthropologist start with different premises about the nature of their subject matter. The sociologist with his statistical orientation presupposes that the field of observation consists of 'units of population,' 'individuals'; in contrast, the social anthropologist, with his non-statistical prejudices, thinks of his data as being made up of 'systems of relationship'" (Leach 1967,

77; emphasis added). Again, this is too simple, because some statistical techniques are themselves concerned with "systems of relationship." But the contrast Leach draws between a qualitative anthropological focus on relationships and the typical survey's focus on the attributes of randomly selected individuals is broadly accurate, and methodologically instructive.

Differences in field research technique then logically follow. The sociologist assumes that the truths he is seeking to investigate are statistical truths, and, in the normal way, he endeavours to arrive at these truths by sampling procedures. Consequently the size of the "population" which can be investigated may be large in relation to the number of investigators; and the length of the enquiry may be correspondingly short. [...] In sharp contrast, an anthropologist will normally confine his attention to a single very small geographical area and endeavour to investigate the total network of interpersonal relationships which exists within that small area. The truths which he thus discovers are particular truths, and, if he is wise, he will be extremely cautious about attempts to generalise from these particulars. (Leach 1967, 78)

It is straightforward to see that the "particular truths" produced by anthropological fieldwork are "situated knowledges" (Haraway 1991). But it is also necessary to recognize, naturally, that "statistical truths" are another form of situated knowledge—differently situated, but situated nonetheless. One of the common social characteristics of "statistical truths" is that they tend to present themselves, and to be received, as results of unsituated knowledge production and as generally representative and authoritative for the social unit being researched. Yet, all researchers are in one manner or another socially and politically situated, and the situation of the survey interview is just one among many. This obviously does not mean that their work is unreliable—only that it requires, like all social research (indeed, like all research), to be interpreted.

It is easy to see that the more obvious criticisms which anthropologists can make against sociologists are the simple converse of those which sociologists are likely to make against anthropologists. On the one hand, the sociologist with his sampling techniques and his questionnaire investigations appears to be presupposing uniformities which perhaps do not really exist. In a sense, he is forced to assume that, already, before he ever starts his questionnaire enquiries, he knows, by intuition, just what are the significant variables concerning which it is worth while making enquiries. On the other hand, the microscopic investigations of the anthropologist may well be of such a particular nature as to have no general validity at all. Both arguments carry weight, and there is a commonsense implication that, if they hope

to achieve conclusions which have a general as opposed to a particular validity, the sociologist and the anthropologist ought to act as a team. With that view I am very largely in agreement. [Here], however, I am not so much concerned with the interdependence of anthropology and sociology as with their contradictions. (Leach 1967, 78–9)

Leach then goes on to analyze and reinterpret some of the findings of the Pata Dambara survey regarding land tenure, inheritance, and stratification. He demonstrates that a random selection of households following the statistical sociological model produces results that systematically misunderstand key social facts and institutions in the region. He proceeds to examine the apparent discrepancy in the survey results and shows that an anthropological, nonrandom analysis of "systems of relationship" among persons and households, land and wealth—as opposed to "units of population" randomly sampled—produces results that better explain the cultural and social processes at work. He notes that he arrives at his conclusion "by making the typically anthropological assumption that a social field does not consist of units of population but of persons in relation to one another" (Leach 1967, 80). Survey research tends to look for statistical relations among "individuals" and households with attributes, whereas anthropologists more commonly look for systems of social relations among people and institutions.

One of the merits of Leach's concise account is that he offers an analytical vocabulary for explaining to a non-anthropologist why the random sample (with all of its possibilities for generalization) should not be the ideal or goal in many research contexts.

Let me repeat, I am arguing by negation. My purpose is not to denounce all statistical types of sociological enquiry but rather to explain just why the social anthropologist claims to be able to reveal facts which escape the observation of the statistician. Precisely because he uses statistics, the sociologist must operate with a random sample of population. This means that *by definition* the units of population must be assumed to be unrelated to one another. It follows that no characteristics of the population which emerge from the enquiry can possibly be attributed to the interrelationships existing between different units. In contrast, the anthropologist explicitly concentrates on data which are *not* random. He purposely chooses a small field within which all the observable phenomena are closely interrelated and interdependent. (Leach 1967, 87; emphases in original)

What is more, standardized and apparently neutral survey questions in fact contain embedded cultural and ideological assumptions.

This is the crux. Dr. Tambiah makes the normal statistical sociologist's assumption that his questionnaire data can be treated as *independent evidence*. Individual questionnaires may be in error but, since the material has been collected on a "random" basis, the errors will cancel out. If then mathematical analysis of the questionnaires produces results which are in accordance with "legal expectation," this shows that practice and legal theory agree. But can we really say anything of the sort? Might it not be that all the questionnaires have, from the first, been drawn up to accord more closely with legal principles than with empirical facts? (Leach 1967, 83)

Here again we are back at the basic difference of attitude adopted by sociologists and anthropologists toward their raw material. The statistical sociologist takes it for granted that the truth which he is seeking is contained in his questionnaire answers and that mathematical technique is capable of revealing that truth; in contrast the anthropologist is suspicious of questionnaire data as such. I maintain that it is in the very nature of questionnaire investigation that the "results" tend to err in the direction of ideal stereotypes. Hence any attempt to investigate, by questionnaire research, the degree of fit between an ideal stereotype and actual practice is a waste of time. (Leach 1967, 85)

A parallel problem is likely to occur when a political scientist researching, say, nationalism interviews government officials or surveys citizens on their "political culture." (See also Bourdieu's critique of the opinion poll [2000, 67ff.] and Malinowski's critique of the questionnaire [1935, 319].) Here again, the knowledge produced tends to present itself as authoritative and to leave relatively invisible the situatedness and partialness of the results. If they are articulated, the situatedness of the researcher(s) and the partial (as opposed to God's-eye) views any project affords tend to be dealt with as unfortunate weaknesses, hardly ever as *analytical strengths*.

The tension between the two traditions of research contrasted by Leach was played out in Allaine's field research and in the course of our correspondence. I have dwelt on the tension at length because Leach's treatment of it goes a long way toward answering the questions about ethnography with which we began: Why do ethnography? What can it accomplish that other modes of research cannot? Is it empirical? Generalizable? Yes, it is deeply empirical. Yes, it can do things that statistical survey research cannot do. It is a powerful form of knowledge production that is (or can be) as deeply empirical as research gets. It is also theoretical from the start, because self-questioning about the form and the "object" of knowledge, about *the categories that structure the enquiry*, and about (as we would say today) one's will to

knowledge are continually activated and reactivated by the socially situated, embodied practices of anthropological fieldwork. The generalizability of ethnographic modes of knowledge (the final element in the list of concerns given above), is, as Leach showed, a question that must be asked in the context of specific projects of research. And before this question becomes meaningful, it is necessary to ask: What is the unit of analysis at hand? And what the will to knowledge? In much classical anthropology, the unit of analysis was "a culture" or "a society" as a whole universe, and the will to knowledge was driven by holistic, totalizing research goals, that is, the desire and injunction to know "everything." Such anthropological holism was always an unattainable and wrong-headed ideal (see Gupta and Ferguson 1997; Geertz 1973, 40ff.).⁴

Long before Edmund Leach's time, Bronislaw Malinowski, in the Trobriand Islands in the 1930s, had already worked out some of the methodological ideas that came to be regarded as canonical ("traditional") in later anthropology. Malinowski never heard postmodernism spoken of, but he was very aware of the constructedness of facts and of the simultaneously theoretical and empirical nature of "fieldwork."

The observer should not function as a mere automaton; a sort of combined camera and phonographic or shorthand recorder of native statements. *While making his observations, the fieldworker must constantly construct*: he must place isolated data in relation to one another and study the manner in which they integrate. To put it paradoxically, one could say that "*facts*" do not exist in sociological any more than in physical reality; that is, they do not dwell in the spatial and temporal continuum open to the untutored eye. The principles of social organisation, of legal constitution, of economics and religion have to be constructed by the observer out of a multitude of manifestations of varying significance and relevance. It is these invisible realities, only to be discovered by inductive computation, by selection and construction, which are scientifically important in the study of culture. Land tenure is typical of such "*invisible facts*." (Malinowski 1935, 317; emphases added)

The "facts" or "data" are not, in other words, *objets trouvés* waiting to be discovered and recorded.⁵ They are *made* (see Geertz 1973, 15–16; Malinowski 1935, 322n2). They are a social product, whether expressed in numbers, words, images, or other media. This clearly does not mean that they are not empirical. It is precisely their active construction by the researcher, Malinowski insists, that is "scientifically important in the study of culture."

Malinowski goes on to consider the nature of ethnographic practice; it is obvious to him that it is an inescapably theoretical practice:

The fieldworker in collecting his material has constantly to strive after a clear idea of what he really wants to know; in this case, a clear idea of what land tenure really is. And since this idea has gradually to emerge from the evidence before him, he must constantly switch over from observation and accumulated evidence to theoretical moulding, and then back to collecting data again. [...] Your ideas, therefore, will have to be extremely plastic and adjustable, for your concrete data, of course, cannot be "adjusted." [...] *Observations are impossible without theory; [...] theories must be formed before you start to observe, but readily dropped or at least remoulded in the course of observation and construction.* (Malinowski 1935, 321, emphasis added)

Clearly, the foregoing schematic discussion touches upon very old and much-rehearsed epistemological debates. The remarkable thing about them is that they are still so actively argued in some quarters, and often in terms of very simple either/or binarisms: objective : subjective :: quantitative : qualitative :: scientific : literary :: empirical : imaginative :: hard : soft, and even nowadays :: "scientific" : "postmodernist."

Paul Willis, a central figure in British cultural studies, took up these binarisms more than twenty years ago.⁶ In a particularly clear critical essay on method in ethnographic cultural studies, "Notes on Method" (1980), his aim is to identify the "really central principles of the 'qualitative' method" (Willis 1980, 88; and see also Willis 2000). He begins by remarking that mainstream, positivistic sociology has been obliged to accord participant observation and case study work "a legitimate place in the social sciences," but that this formulation of a "methodological variety" still leaves "the heartland of the positivist terrain untouched" (Willis 1980, 88). Thus, the notion of a methodological "variety" may be institutionally convenient, but it can also paper over differences that challenge each other too fundamentally to be a part of the same universe of knowledge production, while assimilating "qualitative" ways of knowing as a junior partner in an overarching positivist project.

In its recognition of a *technical* inability to record all that is relevant—and its yielding of this zone to another technique—positivism may actually preserve its deepest loyalty: to its object of inquiry truly as an "object." The duality and mutual exclusivity of the over-neatly opposed categories, "qualitative" methods and "quantitative" methods, suggest already that the "object" is viewed in the same unitary and distanced way even if the *mode* is changed—now you measure it, now you feel it. (Willis 1980, 88; emphases in original)

But, as anthropologists have long insisted, the very "object" of knowledge may change depending on whether a statistical relation or a social system of

relations is privileged, and depending on what one's will to knowledge is. In this sense, the object of knowledge is never inert, lying somewhere out there in the landscape waiting to be "discovered"—or "felt."

Ethnography, Affect, and Quotidian Ethical Practice

This section begins with a discussion of the senses and affect, imagination and embodiment in anthropological fieldwork and then moves on to connect these with questions of ethics.

It is remarkable how often in everyday practices of knowledge production the term, *qualitative* comes to be aligned with matters of intuition and affect. "Now you measure it, now you feel it" (Willis 1980, 88; cf. Musil 1994, 134–49). What is this familiar binarism made of? The production of heteronormative gender binaries through practices of knowledge production is well known and well studied. Here, I want to pull out some other threads. First, this masculine : feminine :: hard : soft :: measurement : feeling :: objective : subjective :: quantitative : qualitative, binary apparatus assumes and even idealizes an absence of affect in the use of numbers. This is demonstrably a weak assumption. We are only too familiar with the emotive effects (and motivations) of interested numerical representations involving the exaggeration or minimization of things like civilian casualties in wars, death rates in genocides, or the measurable physiological effects of depleted uranium on people's bodies in the wake of the Gulf War. (Interestingly, in *The History of the Modern Fact* [1998], Mary Poovey has documented the early scholarly mistrust of statistics as being always too vulnerable to interested and emotive political manipulations. Statistics was considered a "soft" science on just these grounds.)

Despite the rather obvious co-presence of affective and ethical investments in even the most scientific kinds of social research, there remains in "the scientific method" in default mode a "central insistence" on the "*passivity of the participant observer*" (Willis 1980, 89; emphasis added). The idealized erasure of the human, social presence of the researcher is evident in the ways in which "observation" is often defined. The normative expectation of the passivity of the researcher depends upon a "belief that the object of the research exists in [the] external world, with knowable external characteristics which must not be disturbed" (Willis 1980, 89; cf. Whitehead 1967, 88ff.). The idealized subject position for the researcher, then, is to strive to be "a fly on the wall," a quietly transparent observer. This is, at its worst, a voyeuristic and dishonest desire. It is true that "observation" is an element in the ethnographic conventions of "participant observation." But the participant

observer is not a fly. On the one hand, one participates in good faith in the social contexts in which one is working. This presumably also entails conversations with people—conversations that make of the ethnographer, and of them, complexly social persons with social obligations. On the other hand, one observes—or tries to *see* and *hear* and use one's other senses well. To observe unobserved is usually an illusion. The actual fly on the wall shares this illusion at its own peril.

As Willis points out, human subjectivity and imagination are framed by social scientism as potential *contaminants* of research, blots:

The concern is to minimize "distortion in the field," with the underlying fear that the *object* may be *contaminated* with the subjectivity of the researcher. Too easily it becomes an assumption of different orders of reality between the researched and the researcher. The insistent, almost neurotic, technical concern with the differentiation of PO [participant observation] from reportage and Art is also a reflection of the subterranean conviction that PO belongs with the "sciences" and must, in the end, respect objectivity. There is a clear sociological fear of naked subjectivity. The novel can wallow in subjectivity—this is how it creates "colour" and "atmosphere"—but how do we know that the author did not make it all up! So the research must be for a unified object which might be expected to present itself as *the same* to many minds. The first principle of PO, the postponement of theory, compounds the dangers of this covert positivism. It strengthens the notion that the object can present itself directly to the observer. (Willis 1980, 90; emphases in original)

Willis goes on to insist, like Malinowski and Leach (and many others) before him, that "in fact, there is no truly untheoretical way in which to 'see' an 'object.' The 'object' is only perceived and understood through an internal organization of data, mediated by conceptual constructs and ways of seeing the world" (Willis 1980, 90).

This statement provokes, as Willis noted earlier, a palpable positivistic fear that "theory can only, ultimately, demonstrate its own assumptions" (Willis 1980, 89). Thus, to simplify crudely, if every practice of research is always already theoretical, how can anything new be discovered? How can one see outside of theory? How can it be ensured that researchers are not just writing about themselves? The very form of these questions rests on the homology of binarisms discussed above. Willis's answer here is as follows: "However, we must recognize the ambition of the PO [participant-observation] principle in relation to theory. It has directed its followers towards a profoundly important methodological possibility—that of *being "surprised,"* of reaching knowledge not prefigured in one's starting paradigm" (90).⁷ We

see this surprise emerging in Allaine's field experience. Her work in Melbourne was not a matter of the gradual accumulation of "data" into a stable structure, but of moments of puzzlement and sudden realization, of making and unmaking. This element of surprise often transformed her framing questions and led her to move continually back and forth between newfound understandings and older theoretical insights and questions, new methodological ideas and older, precomposed ones.

The capacity to be surprised requires imagination, which is one of the less directly "teachable" abilities that fieldwork demands (see Comaroff and Comaroff 1992; Mills 1959). The aspects of imagination at play in fieldwork are many. Theoretical imagination bleeds into social imagination and back again. Our senses provoke imagination in complex ways, as Stoler (2004); Reddy (2001); Herzfeld (2001, 2004); Seremetakis (1994); Stoller (1989), and others have shown. In the "thick processes" of fieldwork, affect and the senses (like thought) embody and gender the researcher in both predictable and unpredictable ways.

I can offer the sense of smell as a very simple example. In the refugee camp in which I worked in Tanzania, I remember noticing that I initially smelled different from many of the people there. I became aware of the smells of my own deodorant, soap, shampoo, and perfume from Southern California. On other people's bodies, I smelled sweat, smoky reminders of cooking fires, and a different kind of soap. Later, I became curious about where in the camp people got their soap, and I learned where and how it was made. In the process, I learned that it was very easy to ask people to talk about technical processes like soap-making; they knew that they were teaching me something definite and useful. This kind of conversation was much more effortless—and politically safer—than asking people outright about their lives and histories as refugees. Being taught about soap took a long time. In the process, I learned much more than I had expected about questions and themes that interested me more than soap. So, in a quite straightforward way, the sense of smell had yielded methodological and other insights, in addition to introducing me to several people I had not previously known. "Information" in a technical sense yielded "understanding" in theoretical and social senses. As Hannah Arendt (2005) wrote in another time, "Understanding, as distinguished from having correct information and scientific knowledge, is a complicated process which never produces unequivocal results. It is an unending activity by which, in constant change and variation, we come to terms with and reconcile ourselves to reality, that is, try to be at home in the world" (307).

At other times in the course of fieldwork in that refugee camp, fear forced me to think hard about my own imagination and to try to distinguish between paranoia and reasonable caution. Sitting alone at night with my

affectionate tomcat, I sometimes felt my stomach tighten into knots as I imagined possible scenarios (mostly of getting kicked out of the camp before being able to complete my fieldwork, and of getting people into trouble with the authorities somehow simply by having sought their company). I had to work hard to sort out the possible scenarios from the likely scenarios, to trace the surfaces of my fear, and then to translate that into action and inaction. These night battles with my own imagination sharpened my senses, shaped my research strategies, taught me about the kinds of social performativity involved in being a "researcher" or a "student," and, most important, they raised continual ethical concerns about anonymity, confidentiality, informed consent, and the general safety of the people with whom I worked and lived. In this case, I would not know how neatly to separate affect from thought. More, insisting on that neat compartmentalization would have been analytically counterproductive.⁸

Yet, in the ethnographic monograph I wrote on the basis of the fieldwork in Tanzania (Malkki 1995), little remains of the centrality of affect and the senses in the process of my fieldwork. Whether it was fear or pleasure, boredom or comfort, pathos or humor, I tended to erase it from the final written product, perhaps appropriately.⁹ Another thing I self-consciously avoided was writing long, narrative descriptions of the landscape, people's houses, their dress and mannerisms, and the moods or atmospheres of particular moments. I did not feel entitled to describe in that way, from the evidence of my own senses. Much later, in reading Mary Weismantel's excellent book *Cholas and Pishtacos* (2001), I realized that there might have been something quite conventionalized to my avoidance. She writes about a *Puritanism of the senses in scholarly texts*.¹⁰ "Anxious to disavow exoticist writers of the past, ethnographers these days assiduously avoid expressing sensual enjoyment of the places where we work. Indeed, anthropology, once the relentless diarist of minutiae, now avoids the systematic recording of sense impressions, beset by an almost puritanical fear of admitting that they matter to us" (100). She notes that many ethnographers prefer—almost by default—to "move to a more distanced stance; and it is there, withdrawn from sensory knowledge, that they begin their analysis" (101; and see Herzfeld 2004, and 2001, 240–53; Fisher 2002; Seremetakis 1994; Passaro 1997; Foster 1996, 203ff.). Writing about visibility and visual anthropology, Anna Grimshaw has likewise identified "a puritan spirit running through anthropology as modern project," and she characterizes anthropology's relation to images and visibility (after Lucien Taylor) as one of "iconophobia" (Grimshaw 2001, 5; cf. Schneider and Wright 2006, 4; Belting 1994; Jay 1994).¹¹

If we accept that fieldwork is an embodied, and embodying, form of knowledge production, why should we leave a considered awareness of the

senses out of the project of creating ethnographic understanding? (In some quarters, participant observation is playfully known as "soak-and-poke" research.) And it just gets messier.

In 2000–2001, during a fellowship year at the Center for Advanced Study in the Behavioral Sciences in Stanford, California, I presented the e-mail correspondence between Allaine and myself to a multidisciplinary reading group of fellows. I received many productive comments (of which more further on). For the present purposes, the most productive feedback came as follows: "Do you *have* to have all these babies and washing machines in here?" This was a very interesting and surprising reaction, one that promptly announced itself: "*Datum!*" The correspondence included a few references to babies, to be sure, but I could not find any mention of washing machines. I later concluded from this that the commentator's referent was a generalized, vaguely feminine, mundane domesticity. The fine scholar who made the comment, in some frustration at having wasted his time reading such a long document, suggested that the correspondence was too long and that we should just pull out "the main points" and discuss them "more analytically," deleting all the rest. (The relevant homology might look like this: wet : dry :: domesticity : analysis :: life : work :: the senses : critical distance.)

The correspondence, in all its deliberate length, is meant to convey an inescapably temporal *process*. That process, the critical theoretical practice of ethnography, is typically long, often meandering, inescapably social, and temporally situated. More specifically, ethnography as process demands a critical awareness of the invisible social fact that multiple, different temporalities might be at play simultaneously. The process is inextricably embedded in relationally structured social lives,¹² quotidian routines,¹³ events that become Events (see Malkki 1997), the panic time of deadlines,¹⁴ the elongated time of boredom,¹⁵ the cyclical time of the return of the expected,¹⁶ the spiral time of returns to the recognizable or the remembered,¹⁷ and so on. It engages the senses and the social and physical being of the ethnographer. It involves affect and makes demands on one's intuition. All of this is its analytical power and its messy challenge.

That you as an ethnographer work with what you are given—even as you make new things—means also that your gender, age, race, nationality, class, temperament, imagination, subjectivity, histories, and your whole social personhood are in some degree constitutive in the fieldwork process. In your field sites, you cannot be transparent, nor a fly. You take up social space as a person, and you've got to "play it as it lays" (Didion 1970). Gender matters in unexpected ways in all social research, and especially in the processes and relationships of ethnographic fieldwork (Butler 1993). In my own first year of research in Tanzania, I was socially "a student" and had no children. In a brief

summer of research in Montreal, Canada, I was socially "a professor" and "a mother" of two young children. These differences of age and status produced significant differences in the kinds of questions I was able to ask, how and when I was able to hang around with people, how our social obligations as "a family" emerged, and so on. All this "goes without saying" in anthropology.

The babies and washing machines offended because of their messy, feminized concreteness in the specific context of scholarly writing. But that same "messiness" might have taken many different forms—this is not simply a comment about gender. The broad point here is that anthropological fieldwork is not usually a straightforward matter of working. It is also a matter of living. Ethnographic research practice is a way of being in the world. All this engages the senses and emotions, and it takes time. It is in this mundane, day-to-day way that the question of ethics emerges in ethnographic research, as the correspondence shows.

Questions of ethics appear again and again in Allaine's letters and in my efforts to reply to her. She questioned herself, for example, about where the lines between a friendship and a research relationship ought to lie, and more generally about the instrumentalities and forms of desire in social relationships during fieldwork. She worried about the intellectual curiosity and social circumstances that led her to witness a strip search at the police station that was one of her field sites. She confronted the ethics of sociality and gender politics in the company of police officers. She thought about the appropriateness of asking to photograph people's homes and gardens, and about securing permission to reproduce those images in her published work. I, for my part, reminded her to send thank-you notes to her informants, and realized then that in telling her things that "go without saying" for me, I was sounding very much like Miss Manners or Emily Post. I realized later the deeply class-specific nature of reminding her about the thank-you notes. A middle-class woman, I had been taught to write them somewhere along the way. It went without saying to me that it was "polite." This was part and parcel of a whole childhood world of injunctions that had become reflex. "Say thank you." "Don't mumble." "Don't speak or laugh too loudly." "Enunciate." "Look people in the eye." "Shake hands." "Let others go first." "Elbows off the table!" "Posture!" This was the common sense knitted into my spine by my northern European, culturally Lutheran, agnostic, middle-class family—hardly universal principles of good conduct, although some of it has proved serviceable in my work of ethnography.

The quotidian ethical practice that is fieldwork brings up questions that sometimes loudly announce themselves as "ethical dilemmas," while on other occasions they present themselves as "etiquette questions." "Manners" and ethics sit on a sliding scale that involves continuous cultural and other

work of translation, in real time. Sometimes the translations go awry, and then one just has to try to go on from there.

All ethnographic field research is expected to have undergone an institutional review for the protection of "human subjects." The international, federal, and other guidelines used are meant to protect the rights and safety of those who might in any way be affected by the work. These screening processes are very important, but they do not begin to address the demands of ethnography as a quotidian ethical practice (Herzfeld 2001, 2004; Brenneis 1994; Patricia Marshall 2003; Farmer 2002; Meskell and Pels 2005).

The American Anthropological Association regularly publishes material on research ethics, and many fieldwork handbooks and textbooks also carry chapters on ethics. Thinking carefully about basic principles and textbook cases of ethical violations is important and often productive. Presumably for the sake of analytical clarity, textbook accounts and the training programs run by institutional review boards commonly present discrete ethics "cases"—sometimes very extreme cases like the Tuskegee Syphilis Experiment (Jones 1993).¹⁸ While these offer useful thought experiments (as well as striking horror into any heart), they do not address the simpler, more ubiquitous ethical quandaries that crop up as one inhabits dynamic social processes and relationships that are often unpredictable, multiply entangled, difficult to interpret, and typically beyond the control of the researcher. These ethical challenges must be negotiated in real time, as they happen, and they are not neatly bounded. Sometimes they happen so quickly and imperceptibly that there is no time to react until later. At other times, it is what did not happen that generates the ethical soul-searching. This is hardly unique to ethnographic research; it comes with living in the world (Meskell and Pels 2005; Pels 1999; Garber, Hanssen, and Walkowitz 2000; Foucault 1997). Ethnography as ethical practice is necessarily social and therefore necessarily improvisational. There is, however, a strong anti-improvisational and positivist tendency in the wider institutional world of Human Subjects review boards and some funding agencies.¹⁹

Improvisation

The third working principle has been implicit in much of what has gone before, but it is perhaps the most important general lesson of my correspondence with Allaine. It is that ethnography is, and always has been, an improvisational practice. One of the reasons for my willful focus on earlier anthropologists—rather than more contemporary writers on the science question or writing culture (Clifford and Marcus 1986)—is that I want to say

the key thing that has historically "gone without saying" in my discipline: in anthropology, as long as it has been a recognizable discipline, there has been a *tradition of improvisation*. It is not the case that there is an old, stable tradition with a fixed battery of methods, one "correct" way of doing fieldwork, and then a later movement toward postmodern fragmentation and "anything goes" improvisation. Rather, improvisation is the tradition. This comes through clearly in Clifford Geertz's classic 1973 essay, "Thick Description: Toward an Interpretive Theory of Culture," a key text in the field:

In anthropology, or anyway social anthropology, what the practitioners do is ethnography. And it is in understanding what ethnography is, or more exactly *what doing ethnography is*, that a start can be made toward grasping what anthropological analysis amounts to as a form of knowledge. Thus, it must be immediately said, it is not a matter of methods. From one point of view, that of the textbook, doing ethnography is establishing rapport, selecting informants, transcribing texts, taking genealogies, mapping fields, keeping a diary, and so on. But it is not these things, techniques and received procedures, that define the enterprise. What defines it is the kind of intellectual effort it is: an elaborate venture in, to borrow a notion from Gilbert Ryle, "thick description." (5–6)

Geertz insists here that doing ethnography is not a matter of methods. This has too often been misunderstood to mean that anthropology has "no methods." True, it does not have a well-defined set of technical methods like analytic chemistry, for example. An anthropologist cannot be assured of her competence to carry out good field research simply through mastery of a known set of methods—be they (to extend Geertz's list a bit) interview techniques, household surveys, mapmaking, photography, media analysis, symbolic analyses of ritual, structural analyses of myths, questionnaires, the extended case method, tracking of the social drama, life histories, family histories, oral histories, genealogies, kinship analysis, analysis of court records, courtroom observation, archival research, linguistic analysis of speech acts, textual analysis, institutional ethnography, longitudinal studies, or other methods. These are all possible techniques in an open, flexible, highly context-dependent, and time-sensitive *repertory* of possibilities. The intelligent use of that repertory depends on critical, always already theoretical and contextual improvisational practices that, by definition, cannot and should not be a closed set. That they are not a closed set, hypostatizable as "*a methodology*," is in part why doing ethnography is so difficult to teach in a standardized manner from a textbook. The ethnographer—specifically situated in a particular slice of space-time, and embedded in a social situation he does not control—must take on the risk and responsibility of

improvisation, the creative use and perhaps remaking of the repertory. In this more methodological sense, too, the intellectual poaching license to which Kluckhohn referred is a practical necessity.

What, then, is the relationship between anthropology as empirical research and anthropology as an "art," a creative process? Anthropology has variously been called an "art," a "craft," a "science," "a human science," and a "social science." These working definitions, each perhaps ascendant at different moments, sit in an uneasy relationship to one another. And different anthropologists' intellectual temperaments lead them into different alignments in relation to these terms. I see ethnography as a form of situated empiricism (Malkki 1997) that is simultaneously, and without contradiction, an improvisational practice. As improvisation, ethnographic research demands forms of flexible intellectual openness and principled efforts to understand, knowledge and competence, and also forms of creativity and imagination. This, too, has long gone without saying. In 1936 Raymond Firth wrote that "social anthropology does have elements of an art [...], but it is not just an effort of the constructive imagination. Its generalizations must relate at some point to evidence of *what* who said and did where, when, and how. This leads to the heart of the problem of the status of empiricism in social anthropology. It is not a philosophical standpoint, it is a working principle" (Firth 1975, 18; emphasis in original).

More recently, the possibilities opened up by linking ethnographic research practices to artists' practices have been explored by Hal Foster (1996); Quetzil Castaneda (2006); Schneider and Wright (2006); Coles (2000); and others (see also Clifford 1986; and Wolcott 2005).²⁰ Taking Walter Benjamin's 1934 essay "The Author as Producer" as his starting point, Foster suggests that "a new paradigm structurally similar to the old 'Author as Producer' model has emerged in advanced art on the left: *the artist as ethnographer*" (1996, 172; emphasis in original). The ethnographic turn among artists was preceded, according to Foster, by "artist envy" in anthropology (180).²¹ Castaneda develops a performative model of ethnographic research from theater and, specifically, from Augusto Boal's 1985 *Theatre of the Oppressed* (cf. Paulo Freire's *Pedagogy of the Oppressed* [1970]). The bridge I will make here is to music.

In the multidisciplinary reading group at the Center for Advanced Study in the Behavioral Sciences in Stanford, California, mentioned above, Mary-Louise Pratt made the observation that proved to be most enabling: "So, ethnography is really improvisational." It is thanks to her comment that we have since thought further about improvisation and fieldwork. In the same conversation, another colleague protested, "But where are the *rules*?! Are there no rules in anthropology??" In my thought-work, these two very different

"clues" led me to read about improvisation in jazz. This vast literature still remains unfamiliar territory to me, but I will present here what use I have been able to make of it for present purposes. One of the key texts for me was Paul Berliner's *Thinking in Jazz: The Infinite Art of Improvisation* (1994); it is with this work that I principally engage here.²²

First, I found in Berliner an answer to the foregoing question about "rules." Jazz musicians are often, mistakenly, thought to just stand up and make "something out of nothing," to play "out of their heads" (Berliner 1994, 2). A major dictionary states that "to improvise is to compose, or simultaneously compose and perform, on the spur of the moment and *without any preparation*" (Berliner 1994, 1; emphasis added; cf. Moten 2003, 63–64). "The popular conception of improvisation as 'performance without previous preparation' is fundamentally misleading. There is, in fact, a lifetime of preparation and knowledge behind every idea that an improviser performs" (Berliner 1994, 17). As Wynton Marsalis has said, "Jazz is not just, 'Well, man, this is what I feel like playing.' It's a very structured thing that comes down from a tradition and requires a lot of thought and study" (quoted in Berliner 1994, 63). Ethnography requires a similar commitment: to get to the point of improvising well, the ethnographer, like the jazz musician, must have devoted countless hours to practice and preparation of various kinds. (Learning field languages, reading relevant area literatures, writing theoretical essays, reading and critiquing famous ethnographies, and brief exploratory fieldwork trips are among the standard forms of preparation.) I also suggested earlier that competence for ethnographic fieldwork is not guaranteed by mastery of any one "set" of methods. In this respect, too, a comparison with learning jazz improvisation is pertinent: "Ultimately, *learning the tools and techniques of the art provides only the ground for the student's development*. To build the foundation, aspiring musicians must commit endless hours to practicing improvisation—mentally simulating the conditions of live performance events—if they are to acquire the cumulative experience upon which effective storytelling rests" (203; emphasis added). Storytelling is a key language metaphor in jazz (200–201).²³

"One conventional way for young artists to share information is through informal study sessions, a mixture of socializing, shoptalk, and demonstrations known as hanging out" (Berliner 1994, 37). Here, the "young artists" could just as well read "young anthropologists." Much of the methodological learning of ethnography occurs outside the formal, preparatory "methods seminar." Occasionally, informal, apprentice-like relationships develop between faculty and students, or among students themselves, but much of the time the learning is autodidactic and actually occurs most directly during

fieldwork (and during loosely structured "pre-fieldwork" visits of a few weeks or months to a field site).²⁴

One learns context- and time-specific knowledge about particular research contexts as one lives and works in them. And then, in a very direct way, the people with whom one works teach one continually. Sometimes, this teaching happens unwittingly as ethnographers look and listen. At other times, the teaching is more explicit and purposive. Often, it is the people we call "key informants" who really teach us in this second sense. That pedagogical and social relationship involves great investments of time and commitment for both ethnographers and "key informants." It is often challenging to find ways of working with people that do not demand too much of their time or thoroughly inconvenience them. The autodidactic dimension of fieldwork appears to be familiar in the jazz world too: "The jazz community's traditional educational system places its emphasis on learning rather than on teaching, shifting to students the responsibility for determining what they need to learn, how they will go about learning, and from whom" (Berliner 1994, 51).

I also came to think about orality in the teaching of anthropology through Berliner: "In fact, much of the jazz repertory remains part of the community's oral tradition and is not published as single music sheet items or in fake books" (Berliner 1994, 93). The professional practices of anthropology include significant oral dimensions, in which the ambiguities and mistakes of fieldwork live on, whether as rumors about colleagues, as shared reminiscences, or as instructive anecdotes to students (see Klemp et al. 2006; see also Monson 1996; Sawyer 2000).

While the discipline of anthropology, like jazz, values innovation and creative originality, a significant part of the learning and doing of fieldwork (and of jazz improvisation) consists of imitating and quoting, riffs and licks (Berliner 1994, 95, 192). "The great players always give homage to their predecessors by recalling certain things they did. They give it in appreciation and in understanding of the validity of their predecessors. Being able to quote from songs and solos is always part of a mature artist because he's aware of the contribution of others and its impact, how valid it is. Something that is really valid is timeless" (Arthur Rhames, quoted in Berliner 1994, 103–4).

It is perhaps in terms of temporality that the most useful analogies between ethnographic improvisation and jazz improvisation can be made. Neither form of making things in the world involves simple linear time alone. There is a continual "tacking back and forth" between the familiar and the unfamiliar, the plan and its execution, theoretical insights and surprising empirical discoveries. This might be conceptualized as a cyclical (or spiral)

temporality that exists among other kinds of temporal processes and rhythms in the field (cf. Kubler 1962). Likewise, there is in jazz "a perpetual cycle between improvised and precomposed components of the artists' knowledge as it pertains to the entire body of construction materials on any and every level of solo invention. [...] The proportion of precomposition to improvising is likewise subject to continual change throughout a performance" (Berliner 1994, 222, 242).

The cyclical nature of the critical theoretical process of ethnography is also manifest in the taking of risks and the making of mistakes, rethinking and reordering one's questions and priorities.

As the multiple associations of their ideas wash over improvisers, they put into operation their well-practiced skills at negotiating the many possibilities. They select some for development and tightly manage their interrelationships. Besides those unexpected transformations that periodically arise from the discrepancy between conception and execution, improvisers constantly strive to put their thoughts together in different ways, going over old ground in search of new. The activity is much like creative thinking in language, in which the routine process is largely devoted to rethinking. [...] It is in dramatic movements from formerly mastered phrases to unrehearsed patterns, from commonly transacted physical maneuvers to those outside the body's reach or hold, and from familiar frames of reference within compositional forms to uncalculated structural positions, that improvisers typically push the limits of their artistry. (Berliner 1994, 216–17)

This account of improvising rings very true in relation to my own experience of fieldwork, although I was hardly in possession of "well-practiced skills," especially at the beginning. When things were going unexpectedly well during fieldwork in Tanzania, I thought of it at the time as a heady, nervous, even blissful state of "channeling." I was "playing above my head." Something other, better, than me was generating the insights that took form and "worked." A part of it was undoubtedly that I was being *taught* by the people around me in Tanzania. They expected more of me than I did of myself. On these occasions, I felt intensely alive. Berliner cites a jazz artist as follows: "Thinking in motion and creating art on the edge of certainty and surprise, is to be 'very alive, absolutely caught up in the moment'" (220).

Descartes, in naming wonder the first of the passions, described wonder as an impassioned state that makes learning possible. [...] Wonder occurs at the horizon line of what is potentially knowable, but not known. We learn about this horizon line when we find ourselves in a state of wonder. Surprise has guided us to something where we can invest energy and time in a profitable way. (Fisher 2002, 1–2)

Another key parallel is that the jazz improviser plays in real time, often in front of a live audience or together with other musicians. The ethnographer, too, has to negotiate fieldwork in real time, in "live" social contexts. The processes and practices of fieldwork rarely follow the compositional script envisioned in the initial research proposal, and, in the course of the typical period of one year of fieldwork, it is impossible (and undesirable) for all of one's actions to be instrumental, that is, purposively directed solely toward professional goals. Life happens in the course of the work, as it should. There are false starts, adjustments of research questions, mistakes, and surprises along the way. Encounters with informants can be like live performances. You cannot go back to un-say or un-do things; you just make the best of it. As Art Farmer has said, "If an idea comes to you and you don't make it, you have to experience making something else out of it. That happens all the time when you're improvising" (Farmer, quoted in Berliner 1994, 211).

The key contribution of our e-mail correspondence in published form is, then, that it expresses something of the improvisational process of ethnographic fieldwork *as it happens in real time*. This record or working archive is therefore long and filled with quotidian or idiosyncratic details that seem unnecessary. It covers the whole period of a year of Allaine's fieldwork. Some readers of the manuscript have expressed frustration with this:

This text is so long and meandering. Couldn't you boil it down to the main points? Write a real methods book and use a couple of the e-mails to illustrate your points.

We live in an era of sound bites. Nobody has the time to read something like this anymore.

The logic of sound bites performs an authoritative mastery over the complexities of life and politics. An expert is there to pat down doubt and to plug up the little holes through which questions might seep in. Ethnography is, by its very nature, antithetical to forms of "expertise" that close down possibilities and choke off questioning. It is processual. It is as Art Farmer once remarked: "Nothing is ever fully realized, and you never say, 'Well, this is it.' You're always on your way somewhere"—or as Don Pate put it, "otherwhere" (quoted in Berliner 1994, 285, 251; cf. Moore 1978; Arendt 2005).

I began with the question of why it is so hard to say the things that "go without saying" in anthropological fieldwork. It is hard, not because there are "no methods" in fieldwork, but because it is like trying to tell a musician how to improvise. And I return to the fact that this is not a "methodology"

textbook." We hope that this record of one specific process may be useful to others (whether in anthropology or other disciplines), not by telling how to do it, but by showing how it happened, in real time. An accomplished, white saxophonist once worried to Dizzy Gillespie that "in playing jazz he was 'stealing' black people's music." Gillespie said, "You can't steal a gift." He added: "If you can hear it you can have it."²⁵

Endnotes

1. We would like to thank Michael Herzfeld (2001, 2004, and personal communication) for his insights on Bourdieu's (2000) concept of "disposition." See also Durkheim on professional ethics (2001 [1957], 14–27 ff.).

2. Many sociologists have, of course, written on ethnographic method, but generally with a different sensibility.

3. This is a serious oversimplification on Leach's part—there is a strong sociological tradition in ethnography and critical theory, while some anthropologists also use surveys.

4. Malinowski was hardly alone in this; the social construction idea is of course very old; see Kant, Weber, Berger, Luckmann, and others.

5. The idea of simply "stumbling upon" or finding facts lying around, waiting to be collected, is reminiscent of how Disney's Donald Duck cartoons portrayed Uncle Scrooge innocently "finding" pots of gold lying around in the tropics. As Dorfman and Mattelart (1991) put it, Scrooge's pots of gold are a result of systems of relationship. At issue were systems of the disempowerment of specific categories of people, and of the imperialist extraction of resources and labor, to which Disney unfailingly applied the eraser.

6. Willis's classic 1981 ethnography, *Learning to Labour*, has been very influential in anthropology; see, for example, Lave 1988.

7. "This is not to allow back an unbridled, intuitive 'naturalism' on impoverished terms. [...] We must recognize the necessarily theoretical form of what we 'discover'" (Willis 1980, 91).

8. Charles Hirschkind's work on ethics, affect, aurality, and the Islamic revival in Cairo open up new possibilities for thinking about the interrelationships between ethics and affect in ethnographic fieldwork (2006a, b).

9. I would like to thank Michael Herzfeld for his enabling insights on the challenges of representing affect in ethnographic writing (personal communication, 2005).

10. Cf. Hans Belting: "The Reformation taught the dominion of the word, which suppressed all the other religious signs. Christianity had always been a revelation through the word but now the word took an unprecedented monopoly and aura"; Belting goes on to cite Luther: "The kingdom of God is a kingdom of hearing, not of seeing" (1994, 465).

11. Arnd Schneider and Christopher Wright argue that "anthropology's iconophobia and self-imposed restriction of visual expression to text-based models needs to be overcome by a critical engagement with a range of material and sensual practices in the contemporary arts" (2006, 4). See also Coles 2000.

12. The researcher's desire to dwell in the slowed time of rest and thought may be disrupted by an informant's sense of urgency, or vice versa.

13. See, for example, Bourdieu on *habitus* (2000); and de Certeau on such matters in *The Practice of Everyday Life* (1984).

14. The researcher might be feeling the panic of the impending end of fieldwork, the "return from the field," while the people with whom she has worked may feel the yawning of time into the future. They may feel that the researcher has become a part of life as it is, while the latter may feel guilt over her all too prompt and definite departure. The different temporalities inhabited provoke ethical dilemmas and quandaries.

15. Time may seem endless during uninformative interviews, long engagements with bureaucracy, or (as happened to me) when car trouble occurs and one is stranded in the thick of a forest infested by tsetse flies with no apparent prospect of being rescued.

16. The cyclical time of the expected might take the form of the weekly laundry day, monthly rent checks, or annual taxes.

17. The ethnographer might remember or recognize suddenly, in the course of an interview, that what the person across the table is saying has been said by others in other guises in the course of other interviews. Recognition of a pattern spurs her to return to previous field notes, enabling her to understand them in a new way. In the course of this process, the ethnographer realizes that she has returned to an earlier point, but in a different key. She has in the meantime been transformed by new forms of understanding. This seems to me to be a matter of spiral time.

18. See Marshall 2003 on the histories of ethical codes of conduct in scientific research, including the Nuremberg Code of 1947, the Helsinki Declaration of the World Medical Association 2001 (originally formulated in 1964), and the Belmont Report in the United States. See also Macklin 2001.

19. Donald Brenneis, personal communication, 2002. Herzfeld (2004); and Shore and Wright (2000) have written extensively about the bureaucratization of ethics.

20. Here, Thet Shein Win has generously guided me to key contemporary texts on linkages between anthropology and art.

21. On this point, see Coles 2000; and Schneider and Wright 2006, 3.

22. I would like to thank Ray McDermott and Kevin O'Neill for their thoughtful conversations about jazz with me and for pointing me to sources. See, for example, Sharon Welch's essay on Foucault and the jazz aesthetic (2005, 79–103); Smith 1994; Klomp et al. 2006; Moten 2003.

23. See also Lave 1988; and Lave and Wenger 1991 on situated learning.

24. See Herzfeld 2004 on apprenticeship and Durkheim 2001 [1957], 22–27, on the guild, the family, and professional membership.

25. This quotation is from Gillespie's obituary in the *Economist* (February 7, 2002).