Prifysgol Cymru
Y Drindod Dewi Sant
University of Wales
Trinity Saint David

edited by James D. Faubion and George E. Marcus ; foreword by Michael M. J. Fischer.

Introduction - Notes toward an ethnographic memoir of supervising graduate research through anthropology's decades of transformation

pp. 1-34

edited by James D. Faubion and George E. Marcus ; foreword by Michael M. J. Fischer., (2009) Fieldwork is not what it used to be : learning anthropology's method in a time of transition Cornell University Press

Atgoffir staff a myfyrwyr Prifysgol Cymru Y Drindod Dewi Sant bod hawlfraint ar y darn hwn ac ar y gwaith y cafodd ei gymryd ohono. Gwnaed y Copi Digidol hwn o dan delerau trwydded CLA sy'n caniatáu i chi:

- fynd at gopi a'i lwytho i lawr;
- argraffu copi;

Sylwch bod y deunydd hwn yn UNIG at ddefnydd myfyrwyr sydd wedi cofrestru ar y cwrs astudio fel y nodir yn yr adran isod. Caniateir i holl staff a myfyrwyr eraill ddarllen y deunydd yn unig ac ni ddylent ei lwytho i lawr ac/neu argraffu copi

Mae'r Copi Digidol hwn ac unrhyw gopi digidol neu brintiedig a ddarparwyd i chi, neu a wnaed gennych, o dan delerau'r Drwydded hon, i'w ddefnyddio gyda'r Cwrs Astudio hwn. Gellwch gadw copïau o'r fath ar ôl i'r cwrs ddod i ben, ond at eich defnydd personol eich hun yn unig. Bydd pob copi (yn cynnwys copïau electronig) yn cynnwys y Rhybudd Hawlfraint hwn a bydd yn rhaid eu dinistrio ac/neu eu dileu pan fydd Prifysgol Cymru Y Drindod Dewi Sant yn galw am hynny.

Ac eithrio'r hyn a ganiateir gan gyfraith hawlfraint, ni chaniateir copïo, cadw neu ddosbarthu pellach (yn cynnwys drwy e-bost) heb ganiatâd deiliad yr hawlfraint.

Mae gan yr awdur (ac mae'r term hwnnw'n cynnwys arlunwyr a chrewyr gweledol eraill) hawliau moesol dros y gwaith ac ni all staff na myfyrwyr achosi, na chaniatáu, i'r gwaith gael ei aflunio, ei lurgunio neu ei addasu mewn unrhyw ffordd arall, neu ei drin yn ddiraddiol mewn unrhyw ffordd, a fyddai'n niweidiol i anrhydedd neu enw da'r awdur.

Mae hon yn fersiwn ddigidol o ddeunydd hawlfraint a wnaed dan drwydded gan ddeiliad yr hawl, ac ni ellir sicrhau ei chywirdeb. Cyfeiriwch at yr argraffiad cyhoeddedig gwreiddiol os gwelwch yn dda.

Trwyddedwyd i'w ddefnyddio ar gyfer y cwrs HPAN5012 - Theory, Methods and Practice: Critical Perspectives in Anthropology

Teitl: Fieldwork is not what it used to be : learning anthropology's method in a time of transition

Enw’r Awdur: edited by James D. Faubion and George E. Marcus ; foreword by Michael M. J. Fischer.

Enw'r Cyhoeddwr: Cornell University Press

Staff and students of the University of Wales, Trinity Saint David are reminded that copyright subsists in this extract and the work from which it was taken. This Digital Copy has been made under the terms of a CLA licence which allows you to:

- access and download a copy;
- print out a copy;

Please note that this material is for use ONLY by students registered on the course of study as stated in the section below. All other staff and students are only entitled to browse the material and should not download and/or print out a copy.

This Digital Copy and any digital or printed copy supplied to or made by you under the terms of this Licence are for use in connection with this Course of Study. You may retain such copies after the end of the course, but strictly for your own personal use.

All copies (including electronic copies) shall include this Copyright Notice and shall be destroyed and/or deleted if and when required by University of Wales, Trinity Saint David.

Except as provided for by copyright law, no further copying, storage or distribution (including by e-mail) is permitted without the consent of the copyright holder

The author (which term includes artists and other visual creators) has moral rights in the work and neither staff nor students may cause, or permit, the distortion, mutilation or other modification of the work, or any other derogatory treatment of it, which would be prejudicial to the honour or reputation of the author. This is a digital version of copyright material made under licence from the rightsholder, and its accuracy cannot be guaranteed. Please refer to the original published edition.

Course of Study: HPAN5012 - Theory, Methods and Practice: Critical Perspectives in Anthropology

Title: Fieldwork is not what it used to be : learning anthropology's method in a time of transition

Name of Author: edited by James D. Faubion and George E. Marcus ; foreword by Michael M. J. Fischer.

Name of Publisher: Cornell University Press

Introduction

NOTES TOWARD AN ETHNOGRAPHIC MEMOIR OF SUPERVISING GRADUATE RESEARCH THROUGH ANTHROPOLOGY'S DECADES OF TRANSFORMATION

George E. Marcus

Fieldwork projects in anthropology are not what they used to be—at least as they have been imagined in an aesthetics of practice and evaluation that define anthropology's highly distinctive disciplinary culture of both method and career-making. In recent years, there have been a number of stimulating volumes that have reflected on the changing nature of fieldwork practices challenged by contemporary conditions, informing the concepts and ambitions of anthropological research (see, e.g., Amit 2000; Gupta and Ferguson 1997a; Marcus 1999; Rabinow 2005; Strathern 2004). After the so-called reflexive turn of the 1980s, virtually all ethnographies themselves are interesting sources of such reflection, since they include, almost as a requirement, meditations on their conditions of production. Indeed, accounts of fieldwork within ethnographies constitute a primary form of evidence for arguments, and often *are* the primary form of argument.[1] But these sources are mostly written from within this professional, disciplinary culture of anthropology, rather than making this culture and its shaping effects on

1. Anna Tsing's *In the Realm of the Diamond Queen* (1993) comes to mind as one of the most influential and intricate ethnographic texts that builds a strategy of theoretical argument around a narrative account of fieldwork. This narrative character remains an important dimension of how she develops argument in her more recent work, *Friction* (2005), which is exemplary of how ethnographic accounts might develop arguments about globalization. More generally, the volume *Women Writing Culture* (Behar and Gordon 1995), a response to, but quite independent of, *Writing Culture* (Clifford and Marcus 1986), constitutes a kind of primer on how fieldwork narratives within ethnographies have become integral to their core strategies of description and interpretation—at least in one of the most influential contemporary styles of ethnography.

research visible; they think of the broadening horizons of research projects and deliver critical assessments of fieldwork while still holding the aesthetics and the regulative ideals of the Malinowskian paradigm of research—more crucial than ever to the signature identity of anthropology—centrally in place.[2]

What we believe is distinctive about this volume is that it begins to relativize the role of fieldwork, as classically and normatively conceived, within a broader view of the production of anthropological research today. So emblematic of disciplinary identity, fieldwork remains the focus of the accounts offered here. Yet they unfold from varying situations and retrospective degrees of distance from career-defining initiatory projects of ethnographic research. They are studies of the variable role of the classic practices, images, and expectations of fieldwork, with which anthropologists are so familiar and of which they are so proud, in the playing out of complex, often ad hoc and opportunistic designs of research for which the classic aesthetic of distinctive method alone is no longer an adequate imaginary. Indeed, two of the essays (by Fortun and by Kelty) are accounts of personal efforts in teaching fieldwork to conserve the traditional imaginary of fieldwork even while reconceiving and remapping it onto altogether different topologies or scales of research design.

The essays of this volume represent two perspectives: that of the graduate supervisor and that of the former student. They offer the longer view of how initiatory fieldwork that earns degrees turns into finished first projects of research, and then into later projects. In doing so, they open a new discussion of fieldwork

2. "Malinowskian" is repeatedly used as a shorthand in my introductory essay to stand for a complex history of variation in fieldwork practices in the modern history of anthropology (including in the pre-Malinowski, British tradition, something as vogue-ish in the contemporary fashion for collaborations as the Torres Strait expedition of 1898 [see Herle and Rouse 1998]). Still, despite interesting efforts today to revive alternative styles to the mythos of Malinowski (see Bunzl 2004, in his plea for a Neo-Boasian practice of anthropological research), the basic training model of first fieldwork in anthropology graduate programs preserves the substance and outline of Malinowski's fieldwork in the Trobriands, reported effectively in the introductory chapter of *The Argonauts of the Western Pacific* (entitled "The Subject, Method, and Scope of This Ethnography" [Malinowski 1961 (1922), 1–26]). This text, along with a few influential later essays by Clifford Geertz, instantiates in many programs the ethos of fieldwork practice and especially the metamethodological practices that define it as central to the professional craft culture of anthropology. The central tendency in the craft culture of fieldwork is deserving of this particular shorthand, not just because Malinowski wrote effectively about fieldwork, but because he established legendary pedagogical practices that were effectively passed on generationally (see Firth 1957).

While many different theoretical brands of anthropology have been incorporated within the basic model (e.g., during the 1960s, fieldwork in cognitive anthropology had a distinctively different feel from fieldwork, say, in economic anthropology), the critiques of the 1980s (signaled in *Writing Culture* and *Anthropology as Cultural Critique* among other texts) had the effect, in my view, of reinstantiating the basic Malinowskian processual model of fieldwork in training, in new guises and with new licenses like "the reflexive turn," even though the works of Malinowski and those of Clifford Geertz are more and more consigned to courses on the history of the discipline.

and its changing functions in the complex courses of anthropological research careers. In short, the variable ways that the defining practices of fieldwork (participant observation and sustained encounters in stable sites of occupation by the anthropologist) function in the under-conceptualized diversity of the research processes that engulf them are the concerns of these essays. In effect, we argue that the transformation of classic research practices is already well under way within the full array of apprentice to mature projects of research today, but that the deeply committed disciplinary culture of method and symbolic capital of disciplinary identity does not provide an articulate understanding of that transformation on which any collective rethinking of and innovations in how ethnographic research is imagined, taught, and undertaken would depend.[3]

So, we proceed from four basic, connected propositions:

(1) The conduct and outcome of fieldwork are less a matter of training in method, or specific techniques of inquiry and reporting, than of participating in a culture of craftsmanship that anthropologists embrace.

Mentorship (the importance of the supervisor), expectations and strong shared images of ideal practice (young "Malinowskis" engaged with key informants and telling incidents), exemplary performance (the published ethnography that attracts attention beyond specialization), and an aesthetics of evaluation of results (what is "good" ethnography, effective material, rich translation) define collective standards of fieldwork in this culture of craftsmanship. For us, then, fieldwork is already well understood as a method through the many formal and informal accounts of it that exist (see fns. 1 and 2). There are always more stories

---

3. For a very recent account that exposes the workings of the basic training model that I term Malinowskian, see Allaine Cerwonka and Liisa H. Malkki's *Improvising Theory: Process and Temporality in Ethnographic Fieldwork* (2007). This work, which documents the exchanges between adviser/mentor and student while the latter is doing fieldwork, is of special value here not only because it exposes the pedagogical context of first fieldwork in progress, as no other work does, but also because it involves a student from another discipline (political science) and thus the anthropologist mentor (Malkki) displays, perhaps more didactically, her own personal assimilation of the craft of Malinowskian anthropology which she is passing on in her advising of Cerwonka's fieldwork.

Suggesting a reform (rather than a replacement) of the basic model of research long at the core and self-identity of anthropology as a discipline, as this volume does, is a tricky exercise, precisely because of the flexibility of this model and its realization in the experience of each and every person's becoming an anthropologist, and then in his or her experience of passing on this method to students and others. "How is this different from what I do?" "How is this different from what was always done (in fact)?" or "This may be what you do, but it is not what I do" are expected reactions to generally intended discussions of shifts in the way that conditions of research refigure key ideological aspects of research practice (see my debate with Judith Okley [Marcus and Okley 2008]). It is both the glory and the weakness of anthropology's professional culture of method, in whatever shorthand it is characterized, that it depends on this profoundly individualistic form of training, flourishing, and reproduction. We intend in this volume the difficult task of working both with and against this tendency of anthropology's craft culture.

to be told, new reflections on familiar norms and forms, but these are a reinforcement of the same, which I suppose is important for the solidarity of a profession that understands itself as a craft and prides itself on a certain mystique of practice.

What is not well understood are the metamethodological aspects of the anthropological research process, that is, fieldwork's production as a valued object of a professional culture. These aspects both exert a highly restrictive influence on the range of discourses that anthropologists can have about their distinctive research process, and encourage a flexible covering over of the variety of activities, of what is done as research "out there" in fieldwork's name, whether or not they resemble the classic mise-en-scène of fieldwork.

So one sense of "metamethod" in which we are interested concerns the norms of professional culture that shape the actual form of research. Other metamethodological issues define the conditions for research today that exceed the discourse of fieldwork that exists. These define unrecognized predicaments or possibilities of research about which anthropologists are either frustrated or are finding ready-made solutions, revealed perhaps in the changing character of the stories of fieldwork that they tell each other informally, but are otherwise unregistered in the regulative training model of fieldwork (Marcus 2006).[4] We believe these issues of professional culture and research practice, once articulated, would define the bases for certain reforms in the way that anthropologists think about and teach their classic research practice as a craft. Most of the essays in this volume are not explicitly reformist in this way, but a number are just this side, so to speak, of suggesting alternative practices that accommodate the critiques or ready-made adjustments of fieldwork that they have undertaken. This brings

4. In my impressionistic hearing, corridor chat and stories among graduate students whom I have known are about routes, connections, mapping, knowledge distributions—in short, about networking with keen ethnographic sensibilities—rather than about sites, what goes on in a situated community, or location of observation. I am sure that all fieldwork now and in the past has included movement in networks and within sites, and there have always been stories about both milieus, but it is my impression at least that the milieu of the network outweighs the milieu of the site in many current conversational fieldwork stories. This could be a shift in style of self-presentation, but I think it has to do more with changing objects of study, the influence of "theory" (critical cultural and social theories and their ideologies), and real changes in the tempo of research, in access to sites, and in investments in sticking with particular specialized sites of classic participant observation (see Marcus, ed. 1999a). As one raised on "peoples-and-places" research (a year and a half in Tongan villages), I hear too little in fieldwork stories today of conversations with informants, anecdotes of observations and situations, but instead more about findings, juxtapositions, and events familiar through media reportage. These students could produce interesting and in-depth stories of the more traditional kind if asked, but it seems as if their preference and concern in informally reporting their fieldwork are of a different quality. If there is in fact a tendency that could be documented here, I am not prepared to judge whether it is good or bad, but in any case, it would be significant.

us to the need to shape a discourse that escapes the restrictive constructions of fieldwork while preserving its valued characteristics.

(2) The exercise of the decoupling of fieldwork in its enduring classic construction as an essential and defining characteristic of doing anthropology is crucial in being able to think differently about fieldwork in broader contexts of inquiry today, to which the essays of this volume are committed.

In his essay, James Faubion both performs and argues for this exercise of freeing fieldwork as we have known it from the heavy symbolic, identity-defining load that it has carried. For Faubion, what is distinctively anthropological are ways of problematizing inquiry and conceptually defining its objects, rather than the practices of a particular conduct of inquiry and the professional ideological functions that have historically come to bear. To quote him:

> If anthropological (field)work today thus looms as the specter of a Sisyphean labor, endless and never redeemed, it does at least permit of functionally distinct divisions that give it a rather different look from fieldwork of the past. Neither the extended sojourn nor the serial return to the same or closely related physical sites yields a correct model of its physiognomy. Good anthropology will always take time. Yet I can see no reason for concluding that the time it takes must in every case be spent in its bulk in a physical fieldsite.... The ethical profile of the good anthropologist, in short, yields no methodological a priori concerning the appropriate duration of a project. Everything hinges on the terms and requirement of the question of research itself.

Just so.... The turning of an anthropologically conceived question into research requires different ways of thinking about the actual design or practices of research that are free to roam from the deeply inculcated aesthetics of fieldwork by a professional culture of craft. Faubion continues:

> Given the marked inertia of granting agencies, the anthropological (field) project of the future... may well end up looking in fact much as it has looked for several decades: one roughly year-long, more or less continuous encampment at a primary physical site, a few satellite trips here and there, and probably a two- or three-month mop-up before the dissertation or monograph is complete. Yet, in a less conventional understanding of what constitutes an anthropological project, an understanding more in accord with the practices of our contributors, a seriality comes to light that is not merely that of the repeated return to the same physical site (classically, in order to develop an ever richer and deeper comprehension of the people inhabiting that site), but more

> frequently that of a concatenation of legs—some passed in what we still customarily expect a site to be, but others, no less integral a part of the project itself, passed at the library or in conversation with students and colleagues, legs in which the primary but still altogether integral activity is not that of encounter but instead that of the evaluation, articulation, thinking, and rethinking of what one has already encountered and what one is likely to encounter on the next go.

Faubion makes it possible to see classic fieldwork as a variable component of a broader process of research, and thus to see the need for distinctively anthropological understandings of this broader process. In his essay, he ventures a fascinating scheme of his own to conceive of this broader process, which develops seriality (rather than reflexivity or recursivity), connectivity, and a topology of conceptual innovations as its key ideas. He understands anthropological research as playing out on different scales required by its contemporary ambitions to pursue questions that fieldwork itself in its conventional aesthetics cannot answer. We need many more such schemes, proposals, and inventions to think about at this juncture.[5] The essays of this volume suggest where in the professional culture of research careers the rethinking of fieldwork as we have conceived it might most cogently emerge—which brings us to our third proposition.

(3) Thinking about how anthropology's culture of research and professionalization shapes what is usually thought of as method, the process of dissertation research, of apprentice research, and, by extension, how first research gets "finished," and then how one moves on to other projects that should share something of the same character as first fieldwork is a strategic focus of attention for us.

The aesthetics of craft in a culture of knowledge production that takes mandatory courses on method insouciantly, while highly valuing tales of fieldwork that recount surprise, discovery, and unanticipated findings,[6] are much more

---

5. At present, such schemes sometimes emerge as novel experiments or projects, especially in the orchestration and management of collaborations (see Lassiter 2005a). In the context of training and pedagogy, Burawoy's *Global Ethnography* (2000), representing a collaborative project among him and his students, is an example of one such effort to revise the standard practices of ethnographic research to produce coordinated fieldwork on processes that take place on a global scale. Such examples of multi-sited ethnography, as I have defined it (Marcus 1998), would seem to be the natural terrain on which to conduct self-conscious experiments with the Malinowskian parameters of fieldwork, but as the essays of this volume demonstrate, there are as many conceptual varieties of recentering the classic fieldwork process in broader domains of research activity as there are dissertation projects revised by later reflection.

6. The rhetorical frame that corresponds to this special contribution of ethnography is the common indication at the beginning of texts that the research fortuitously took different turns from the way it had been conceived before fieldwork. This is one key way of establishing authority for an ethnography through the venerable aesthetic in anthropology of knowing by discovering. Thus, fieldwork habitually is reported as having taken often quite abrupt shifts or turns in direction.

defining of fieldwork than training in particular skills or techniques of inquiry in the conventional sense of method. But still, we argue in this volume that this arena of metamethod, as we have termed it, needs conceptualizations and experiments with alternative practices just as much, and even regimes of training in terms of them, that would substitute for what is now imagined by regulative ideals about the doing of research as fieldwork. These latter assimilate at their limit what anthropologists—both apprentice and mature ones—are actually doing in their research these days, without explicitly undergoing redefinition themselves.

This rethinking of the craft, and its regulative ideals or aesthetics, can best be done by looking closely—as autoethnographic objects themselves—at the experience and process of first fieldwork in a broader context that most anthropologists define initially as students, then as mature researchers looking back, and continually as mentors/supervisors. First fieldwork is thus a common object of reflection by which the entire culture of research craft in anthropology can be rethought. That is the spirit in which the contributors to this volume think about fieldwork today—not to celebrate its virtues, despise its flaws, or to admire its adaptability—but to look squarely at how it plays variable roles in larger designs of research that the present training model can only vaguely accommodate.

There are at least two reasons why this focus on first fieldwork is strategic for us rather than being merely convenient or interesting. As training model, first fieldwork is where what is most authoritative and regulative in the Malinowskian tradition of fieldwork most visibly meets the complex array of research questions and concepts that challenges the parameters of this tradition. These questions are imported into anthropology through its interdisciplinary engagements and are what have primarily drawn many students to the discipline.[7] The emphasis in some graduate programs may well be to limit quickly the ambition in apprentice inquiry that some of these questions encourage so that the training function of that first project can be successfully, effectively, and efficiently achieved. There are also ready-made narratives of research conception available on various current

7. This is an appropriate point to indicate that we make no effort in this volume to explain why, in recent disciplinary history, ethnographic practice plays such a large role not only in the symbolic identity of anthropology and anthropologists, to judge whether this should be so (that indeed anthropology, social and cultural, should be more than ethnography), or to suggest the major sources, internal or external, which have altered the forms and norms of the practice of fieldwork and the writing of ethnography in recent times. One of the press readers for this volume posed the issues in this way: "Was it new modes of fieldwork that produced recent changes in anthropology, or did new theoretical questions in anthropology bring about and necessitate new forms of fieldwork?" or, "Are we discussing 'new reflections on fieldwork' or 'reflections on new fieldwork?" These are indeed big questions that deserve their own treatment, but we sidestep them for the sake of a more strategic (but we believe just as important) critique and intervention that focus on a neglected side of the many discussions concerning fieldwork and ethnography in recent years. For my own preliminary and fragmented attempts to address the big questions, see Marcus 1998, 1999, 2005, 2007, 2008b; Rabinow, Marcus, et al. 2008.

problem arenas within which the Malinowskian ideal can be easily followed.[8] But, more frequently in my experience and observation, first fieldwork projects in many departments today are more like experiments managed by students and their supervisors in negotiating the limits of the norms and forms of the traditional paradigm to take on dimensions of problems that "fieldwork as usual" has not been designed to address.[9] It would certainly be preferable not to have to conduct such training as de facto experiments under the burden of conserving the valued norms of a culture of craft. We believe there are others means by which this very same culture can be preserved, but it requires thinking differently about first fieldwork, more in line with what the ongoing mentor-student negotiations tell us than what the long-standing regulative ideals in play impose.

So, the value of innovation, originality, discovering something conceptually new through fieldwork trumps the countervailing pressure to work within the available anthropological "takes" on the problem arenas that it has recently entered. In particular, exemplary ethnographies by younger scholars, which are of key pedagogical use, are carriers of this value, provide models of what to strive for, and also indicate where the self-esteem of anthropology lies today. What is interesting to us, then, in first projects are their edges, the negotiations by which they satisfy the traditional norms and gesture beyond them. It is in these negotiations and what is settled for in the production of dissertations—and thus, perhaps most crucially, in what remains to be done afterwards to finish or complete

8. These are discussed in a volume of conversations mainly between Paul Rabinow and me (Rabinow, Marcus, et al. 2008). These are the scripts that situate locales, communities, or groups, usually of subaltern subjects, at the vortex of contemporary complex changes involving global-scale processes, events, and multiple agents and forces (for influential recent examples see Petryna 2002; Tsing 2005). Ethnography preserves its Malinowskian aesthetics by staying with human-scale lives of accessible subjects. Some would say this is what ethnography does best and is also its limit. The spirit of this volume is to renegotiate this limit by rethinking the formal dimensions of the research process, held in place by powerful craft aesthetics, so that the role of fieldwork in specific projects can be conceived beyond the conventionally situating current scripts for it.

9. Simply incorporating experts as ethnographic subjects, or the production of a particular kind of rational, bureaucratic knowledge as an ethnographic object, into a field of research that traditionally focuses on sites of everyday and ordinary life stimulates such negotiations in graduate training (see Boyer, in press). This negotiation is not about simply adding sites of elites to sites of subaltern subjects; rather, it is about encompassing in fieldwork something of the complexity of contemporary processes, which the habit of setting up ethnography by distinguishing between the global and the local belies. The ethnography of policy processes provides exemplars for defining this more complex terrain in the conception of fieldwork projects. Greenhalgh's magisterial account of China's One-Child policy (2008) derives from a complex path of research, rooted in fieldwork, over twenty years. The challenge of negotiating dissertation projects today is how to incorporate such scale—or not—into the design of career-defining ethnographies of apprentice anthropologists. Such a design involves not just doing fieldwork in the Malinowskian pattern, but a different compass in which fieldwork itself can have variable functions and forms.

research—that the glimmer of an alternative and coherent conception of the present research process can be perceived.

Second, while I have written about the way in which later projects of research operate on very different principles than first projects, even though they share the same ideology of craft (Marcus 1998, 233–45), it is first projects more than later ones that are responsible for guaranteeing the reproduction of the entire research enterprise of anthropology in its traditional form, even though it appears sometimes that second projects are "running away" from the training model. This frequent discomfort or at least ambivalence with how first projects end, at least at the dissertation stage, is a major and ironically valuable encouragement in the production of finished first work, and later projects as well; it is indeed a symptom to note in the imagining of the alternative, remodeled practices to which I will return.

Indeed, whether first fieldwork was done in the classic era of sustained discipline-developed problematics for it (e.g., problems of kinship, ritual, classification, language, the essential character of cultures) or now, when problems of inquiry often exceed the imagined and literal places in which first fieldwork in the classic mode can take place, first fieldwork nonetheless provides in almost every case a personal, even intimate constitution, carrying in an embodied way the culture of craft that anthropology so much values into highly diverse careers of research. Many of these are in the pursuit of projects very far from the training model, while still hanging on to the influence of its imaginary.

Thus, through first fieldwork the authority of the discipline as craft comes to live on within careers of diverse research activity. It is the anchor of lore in practice, through the affect of memory—positive or negative. It tells one what the "classic" model is in one's own experience and later trajectory; it is a compass, however much one's research and circumstances vary later. In this volume, we depend on the power of that "looking back" on first fieldwork in a sampling of today's projects of research, as represented in the essays of former recent students at different degrees of distance and in various subsequent professional situations. Their accounts think of first fieldwork not as a professional legitimation, bittersweet in some cases perhaps, for what one does later, but as a key means to begin to imagine alternative visions of craft that respond to the "symptoms" encouraging them in such distanced reflection on experiences of first fieldwork. This leads us more explicitly to the state of the pedagogy of first projects of anthropological research and to our fourth guiding proposition.

(4) Finally, and more briefly, the object of the exercise undertaken collectively by the essays of this volume and the critiques of anthropology's professional culture of method, centered on fieldwork, is to encourage experimentation with alternative pedagogical strategies that might enhance graduate training

consistent with a long view of how fieldwork fits practically, ideologically, and variantly into anthropological research careers today.

What is called for are more debates arising from better ethnographic understandings of the contemporary research process itself. This is a very large task that we can only hope to evoke and provoke in this volume. But the common concerns of these essays with first fieldwork, across the perspectives of the supervisors of the dissertation projects in question in the various essays, of former recent students, and of two teachers of fieldwork with a decidedly experimental orientation to pedagogy, are an effective way to begin.

The Essays

The essays by Faubion and me are reflections by long-time dissertation supervisors, who worked together over the years in this capacity. Indeed, we were co-supervisors of the first fieldwork projects of the six former recent students who contribute essays to this volume. Appropriately, our essays bracket and frame their accounts, mine by overview, Faubion by theoretical implication. Taken together, our reflections are not about specific projects, but of the changing nature and function of fieldwork of the classic sort over the past two decades in the kinds of contemporary research careers that are exemplary and emerging among younger anthropologists.

Faubion goes much further than I do in venturing a theoretical framework for an alternative understanding of what fieldwork might be about these days. We both, however, see the need, from the shared supervisions that we have done, to evolve apprentice projects differently. Both of us work with an ethnographer's eye for the metamethodological habits that currently shape the production of ethnography—the subtle affective and attitudinal factors that characterize anthropology's explicit and core culture of method as it is challenged today: its sense of tempo, of patience, slowness, in the presence of rapidly changing events; its anxieties about scale, about being pulled away from the Malinowskian scene of encounter; and its multiple accountabilities, with the professional community being only one. I have tried to capture some of these characteristics below in a set of notes and observations about predicaments of the training process. Faubion uses ethics as a conceptual frame to evoke some of the same issues.

About the six core essays by former recent students variously situated now and at various degrees of distance from the first fieldwork process in graduate degree programs: the temptation, given the dominant genre in existence for such accounts, would be to treat these as simply more "tales of the field" that, however bittersweet and even critical, wind up validating the professional ideology of

fieldwork as worthy "trial and tribulation." I would encourage readers not to do so, but to see them as fieldwork accounts that begin to challenge, from different angles of distance and current situation, the adequacy of the classic paradigm of fieldwork to account for their research experiences, which overflow it. Each case of near or more distant "looking back" and the intellectual value and function of first fieldwork performed, within the canonical environment of training as I described, is quite different from every other. If there is a single overweening issue that enters, more or less prominently, into each account, it is about scale and scaling: how to make the phenomenological intimacies of fieldwork, made even more canonical by the so-called reflexive turn of the 1980s, speak to larger theoretical, more abstract, but nonetheless empirical engagements with systems, institutions, networks, and global processes. How research in each case after the dissertation develops in this larger arena of both conceptual and literal spaces determines what kind of value first fieldwork has in a relative sense.

First fieldwork barely allowed Kristin Peterson and Jae Chung to grasp, or even operate in, the problem arenas in which they are now dealing. Their essays most directly address issues of the scale of ethnographic work versus the scale of the anthropological object in question. They continue to do anthropological research, but whether it has any significant resemblance to the Malinowskian scheme is a question on which the working out of new metamethodological practices, in which this volume is interested, depends. In each case, first fieldwork was valuable for doing a certain kind of messy conceptual labor of extraordinary importance to what has come later.

The essays by Jennifer Hamilton, Deepa Reddy, Nahal Naficy, and Lisa Breglia are portraits of the situational constraints of fieldwork, the juggling of independence against several different sources of blockage, and imposition of research norms and terms of conduct not their own. In both first and later projects, they wrestle under contemporary conditions with the absence of the space that Malinowski and later fieldworkers (even if primarily in a psychological sense) had. Many fieldworkers today are simply not free in a practical sense to impose the classic conditions of fieldwork, or the difficulties of so doing are quite different from those related in classic accounts. Thus, in such accounts as these four essays especially present, there can be read the glimmer of alternative understandings of fieldwork that are being formed within essays that in a hybrid sense are still within the conventions of genre-writing ("tales of fieldwork").

Hamilton's and Reddy's essays overlap substantively: after different first fieldwork experiences they have until recently been working within the same environment of medical research that has made places for ethnography. While also explicitly addressing problems of scale, Hamilton's is a discussion of the "recovery" from the apparent failure of the first project (but see the book that was

later derived from that experience [Hamilton 2008a]) to the establishment of a certain elbow room in the course of taking on the second project with Reddy (see also the book that Reddy generated around her first fieldwork [Reddy 2006]).

The essays by Reddy, Naficy, and Breglia are less about the problems of scale than the problems of association with one's subjects and the ecology of the contemporary ethnographic encounter, which redefine the "site" or mise-en-scène in which fieldwork is classically imagined to take place. Of course, this "site" has always been instead a "field" in which the ethnographer moves for a period. These movements accumulate an inventive recursivity, composed of the de facto mix of design, serendipity, and choices that creates in turn a distinctive coherence and imaginary to fieldwork these days, as Faubion discusses. The problem is that this recursivity spins research out of the traditionally imagined confines of fieldwork (where "good ethnography" can be done) and suggests the need for practices that redefine these "confines" of fieldwork to keep up with the expanding and moving ground of research.

Breglia makes perhaps the strongest argument for the marginalization of the Malinowskian standard in favor of found concepts and circuits of inquiry in a very complex ecology of movement. Indeed, fieldwork for Breglia is at once an identity, a foil, and, as she says, a phantasm of heightened "common sense," against which to think creatively. All of these essays suggest the need for different training models, worked out, rather than the revised Malinowksian model of craft that is proudly open to what the student might find.

So, as a dissertation supervisor, I ask myself how many of the retrospective insights in these six essays about what fieldwork in its classic modality means in larger frames and longer views of particular research problems could have been thought through proactively before "going into the field"? And if many could have been (as I believe), what would the capacity to think through ethnographically or imagine the field more thoroughly before "going there" suggest about different preparations and conceptual boundaries of research projects, even in the performance of first fieldwork with its symbolic load as "rite of passage"? More on this later.

This volume ends appropriately with two experiments in teaching research that are alive to the issues that we are raising. They provocatively conduct these experiments on first fieldwork at the very beginning of careers, where, as argued, metamethod is the symbolic heart of the discipline's identity. Kim Fortun was trained in anthropology at Rice and is author of *Advocacy After Bhopal* (2001). She teaches now in a science studies program at Rensselaer Polytechnic Institute. Christopher Kelty was trained in science studies at MIT and is author of *Two Bits: Free Software and the Social Imagination After the Internet* (2008b). Until recently, he taught anthropology at Rice. There is a sense of license in both

papers in experimenting with anthropology's method that would not be found commonly in anthropology programs. And this is significant. They emerge from the borderlands of the crossover between the anthropological project of ethnographic research and the interdisciplinary field of science and technology studies (sometimes known as STS) in which the focus is on the conception of objects of study and how to articulate research problems that will stimulate collective work on particular topics (in this, STS tends to mimic the concern of research in the sciences themselves for operating within limited, vetted, and collective research programs) rather than on methods, about which STS is eclectic.[10]

From the perspective of anthropology programs, science studies is clearly a vibrant arena in the current orientation of anthropological inquiry toward problems and objects of study novel to it, which inevitably challenges very settled ideas of what research as fieldwork should be like. This arena is a veritable laboratory of new ideas about alternative conceptions of fieldwork. But of course, not all anthropologists do science studies, and science studies programs in their eclecticism care little about staying true to the classic norms and forms of anthropology. The trick is how the experimental spirit and license of these two papers in rethinking first fieldwork might migrate suggestively and more generically into the diverse range of problem areas in which anthropologists conduct research today. This is indeed already happening on a large scale in contemporary anthropology and as represented here in the crossovers that structure the essays by Fortun and Kelty. Both provide useful ways for teaching fieldwork relevant to any contemporary subject of interest and for addressing issues, which run through this volume, of scale, research environments of collaboration, and how fieldwork can be conceived flexibly to participate in research on questions for which anthropological fieldwork has little past experience in providing answers.

Finally, it will not be lost on the reader familiar with the recent history of anthropology that the contributors to this volume are connected by more than common interests—they have all been associated with the Rice department of anthropology in various ways: Faubion a current member of the faculty and Kelty (who was also a student of Michael Fischer's at MIT, a former longtime faculty member at Rice who reflects on the history of the Rice department in his foreword to this volume) nearly current; a former faculty member now at the University of the California, Irvine (myself); six former recent graduate students; and Fortun, a former graduate student at Rice in the late 1980s and early 1990s.

The Rice department collectively was a leader, among others, and a mildly controversial one at that, in the seminal critiques of the 1980s that contributed

10. For exemplary recent works that reflect the vibrant crossover between anthropology and sciences studies, see Fischer 2003; Downey and Dumit 1997; and Franklin and Lock 2003.

to the broader trends that have reshaped the research agendas of contemporary anthropology and its styles of ethnography over the past two decades. But I would ask readers not to move quickly to the conclusion that the concerns of this volume thus represent the concerns of a smallish faction within anthropology, either in the guise of a survival of past fashion or of an avant-garde. While the spirit and intellectual capital of the critiques of the 1980s perhaps endured somewhat more strongly in the Rice department than in others, on the whole, the legacies of those critiques have become quite mainstream. At least a large number of anthropologists, and many students who are entering the discipline, think of research today in terms that would be quite familiar to and compatible with ideas and arguments circulating especially through the 1980s and early 1990s.

In any case, we would claim that much of what we are arguing for now encourages anthropology to move on from the influences of that period and the points where the discipline has become stuck, so to speak. The way to do this, we think, is to concentrate on remaking what is least up for debate, but which is deeply shaped by once critical ideas becoming a kind of orthodoxy, or at least an authority for preserving very old and ideologically highly valued practices of a culture of craft. Accordingly, we believe that we are addressing significant current tendencies of change at the heart of the discipline—its culture of method. Further, we think the Rice connections are a distinct advantage in so doing. They provide a context of working within an important, if not decisive, recent history of the discipline, and they provide a coherence and rationale in purpose at a level that few edited volumes achieve.

My own recent move to the department at the University of California, Irvine, one that has been prominent in contributing to, even in defining, current research trends, confirms for me the importance of rethinking our culture of method, especially in graduate pedagogy. The student predicaments at UCI and the culture of method here impress me as not markedly different from their counterparts at Rice in recent times, or from the situation of graduate training in many programs that I have visited in recent years, both in the United States and abroad.

I would make a broad and relative distinction about the level of current awareness among anthropology graduate programs, or even projects case by case, regarding the state of the discipline's culture of method and their motivation to remodel the pedagogy of training. On the one hand, there are those programs or projects that are undertaken within strong facilities and traditions of area studies (e.g., universities where anthropology develops alongside substantial institutional resources for the study of particular regions)—even though area studies as a concept and program has itself undergone searching

critiques since the 1990s. On the other hand, there are those programs and projects that are driven by "problems of the contemporary" (phrased often in terms of modernity, alternative modernities, globalization, or identified with various formulations of public anthropology or activism), wherever they may be situated geographically. This distinction is of course relative, and projects so distinguished are pursued side-by-side in the same departments. But it seems to me that fieldwork projects backed by strong area studies traditions are not as troubled, or as stimulated, by problems of metamethod that we have been discussing here. The motivation to remodel pedagogy is likely to be strongest among those teachers and students who are most vexed by problems of scale in what they do, for which the apparatus of culture/geographical areas is not an adequate or available framework.

So today we often deal with first fieldwork projects in which students situate the complex things that they do (made complex by transcultural, transareal structures that fieldworkers have the ambition to get "inside" as they once desired to get inside structures of kinship, for example) in specific cultural zones nonetheless (which the Malinowksian model, especially in training, requires—"go far away and work on ordinary people"). With the aid of mentorship and advising, they are left to figure out a research path that will satisfy these parameters: what culture is in the vortex of changes at different scales; what the relation of micro to macro is; how power is constituted in systems of many agencies. Without the supporting context of organized area studies, which often has specific, usually historicist narratives for the ethnographer to write within and the fieldworker to structure her experience in terms of, many projects today, though still deeply invested in understanding other places, require a different kind of working through of a problem that matches the complexity of its recognition and encounter in a pathway of fieldwork that neither begins nor ends with the gathering of material for a dissertation.[11]

---

11. Xiang Biao's *Global "Body Shopping"* (2007) provides, for me, an ideal example of how a dissertation project evolved into a transnational, multi-sited and manageable ethnography of a truly global object of study: an Indian labor system on which the computer industry has depended. Granted, Xiang is dealing with subjects quite congenial to a tradition of fieldwork research: the study of workers and working conditions. Yet I am impressed by the preservation of "the cultural" within the ethnography without it being the dominant frame of the study (he is not primarily asking about the "Indian-ness" of this system, although he captures it), and I am equally impressed by how appropriate the depth of fieldwork is in the different sites explored. Malinowskian detail is there; but the account of the operation of a system is foregrounded. It is as if Malinowski actually made the kula voyages.

Xiang's project accomplishes conventional fieldwork on a scale and in terms of a complex object about which supervisors are often hesitant, if not skeptical, when they negotiate dissertation projects with students.

Revisions of the Culture of Training and Observations from Graduate Supervision: A Brief Exercise in Ethnographic Memoir

I want to use the remainder of this essay to present in note form a collection of observations and suggestions about graduate training in the metamethod of anthropology's craft that addresses the concerns of this volume. Perhaps this should be thought of as a contribution to pedagogy rather than methodology. These notes are developed from the perspective of my role as a graduate supervisor in a single program over more than two decades during which the actual nature and content of initiatory fieldwork across the discipline have changed markedly, though the pedagogical routine with its considerable virtues has not. I do not argue for a revolution in or reformation of pedagogy, but rather a morphing of it to catch up with what research, and fieldwork within it, have become in many apprentice situations. The ethnographer's eye for the subtlety, detail, and contradictions or ironies of process is what I hope to have turned, however briefly, upon our culture of craft, in which I have participated and which I have observed during a time of its transition, the nature of which, I would underscore again, has not yet been effectively articulated.

I preface this exercise with two personal observations, impressions really, about shifts in the graduate training process that have affected my experience and thinking about supervision. First, for many years after the 1980s critiques and the interdisciplinary ferment around the study of culture of which they were a part, at least until the late 1990s, the best candidates for graduate training in social-cultural anthropology were motivated primarily by the excitement and complexity of those largely academic debates. Critique, critical theories, and changing conceptions of culture, society, and identity drove students from a variety of academic trainings in these interdisciplinary movements into anthropology which, for its legendary method of engagement in the world, its self-critical acuity, its long-standing identification with conceptions of culture, and the objects that its broad historic ambitions had evoked for inquiry had considerable cachet in these interdisciplinary movements. So, I had come to presume at least at Rice that the students with whom I would be working came with this theoretical, essentially academic interest and image of 1980s anthropology, and that fieldwork, while desired as a trope of practice, would be the major challenge—how to operationalize the theory of that period and its habits for the shocks, cultural and otherwise, messiness, and ad hoc qualities of fieldwork's distinctive intellectual labor. After the turn of the century, I came to realize that the priorities of the best candidates had shifted (even at Rice). The excitement of theory and academic debate about changing social and cultural orders had receded among students

in favor of activism, driven by a healthy combination of pragmatics and idealism. The "typical" highly motivated candidate today comes with experience from work in the world of NGOs and activist organizations. One can no longer count on a background in the knowledge of, or at least the desire for, the theories and debates that brought students into anthropology previously and about which they were better informed, even though academic training in anthropology still heavily depends on working within the debates and theories of the earlier period. It is just that students are unfamiliar with—and thus have to be taught in a more elementary way (which, by the way, is a very great problem for the economy of effort in graduate teaching in many programs)—this still active apparatus.[12]

So, if my impression is at all correct, students enter anthropology today for what fieldwork can do in sites of the world with which they already have considerable experience or affinity. In a sense, today's students are ahead of the discipline in thinking about the terrains of the "real world" in which fieldwork-based projects of research must be forged. We could simply adapt the pedagogy we have to these NGO-experienced students, which is the state of play in many departments, but in my opinion it is much better for anthropology and its own debates and ideas to rethink and recast its pedagogy to where younger generations are pulling it in the present era. One might say that the 1980s were a congenial intellectual preparation in less engaged times for the shape and inclinations of research careers being formed now. Rethinking graduate pedagogy is a crucial middle term and disciplinary task in this transition.

Second, in participating in the pedagogy of first fieldwork over the years, I have long felt that the period of the defining of the dissertation, through fieldwork, to the dissertation writing-up process encompasses the most obvious and interesting activities in which to intervene with new thinking and experiments in the remodeling of practices. Certainly, they remain the most critical activities by which to observe the negotiation of anthropology's aesthetics of craft and in

12. In my own current department, there is a brilliantly conceived and effective three-term pro-course designed in part to instill this "apparatus"—this tradition and influence of theory from the 1980s and 1990s—in new students. In my former department at Rice, we at one point hired Kathryn Milun, a scholar in comparative literature, to teach students preparing to do fieldwork this synergy, so to speak, of the mastery of contemporary theory (Michel Foucault, Gilles Deleuze, Jacques Derrida, Jürgen Habermas, Hans-Georg Gadamer, Gayatri Spivak, Julia Kristeva, etc.) and its innovative applications in text-based cultural analyses, for it was then in comparative literature that such synergy was most effectively realized. Milun had to work yearly with students who arrived (for better or worse!) with considerable conversancy with the range of theoretical works. Now, in both departments, there is the need to begin at the beginning in teaching these sources (or those whose currency has survived, such as Foucault), even though the role that such theory plays in constructing ethnography is still decisive (for a critique of the kind of ethnography that the era of theory produced, see Marcus 2007).

relation to which to suggest changes in practice. But under current constraints, I have come to see the little-reflected-upon postdoctoral process as the part of first research that most accurately and explicitly manifests the real predicaments of contemporary anthropological research. Of course, this process is often beyond formal supervision and pedagogy, but probably most supervisors, and I myself, remain involved with this key part of the research projects of former students. For me, it has become more fun and personally satisfying than trying to forge alternative pedagogy in the current process of dissertation research. And for my interest in the remodeling of pedagogy, it has become a very instructive and inevitable phase in the present training model. I find the kind of "second-wind" rethinking and renewed initiatives in inquiry that sometimes go on in this phase fascinating and a source of inspiration for my rethinking of the pedagogy that precedes it.

In many cases, former students substantially revise their dissertations or put them aside altogether in the postdoc period. Certainly they functionally relativize the dominant role of first fieldwork in the dissertation process, which shares much with the enterprise of this volume. Increasingly, almost as a standard practice nowadays, jobs are awarded to candidates who have remade their research in postdoc periods of one to two years, however well supported by fellowships or temporary jobs they are—or are not—and have completed ethnographic volumes to show for it. So the following notes take the present process as it is, but they are inspired by the reinvention of research that often goes on in the postdoc period today.

Observations

In the spirit of the previous discussion about what of consequence for rethinking the paradigm of training these days occurs off the radar, so to speak, in the postdoc period, the following observations are mere gestures, of varying degrees of elaboration, from a contemplated memoir of a career of supervising first research projects. They are about where to look—where there are "seams" between present practices and alternative possibilities legible within them. This legibility itself depends on the ethnographer's art, and each one deserves an article or a chapter in an ethnographic memoir of a career of supervision that savors the subtleties and subversions of the informalities of a culture of method central to a discipline that itself savors the subtle and critical edges of its research productions.

Tales of Fieldwork Now

Fieldwork stories—not so much those written into ethnographies almost habitually since the 1980s as a consequence of the so-called reflexive turn of that

period, but the kinds of narratives that are the currency of courses, conferences, and informal occasions of all kinds in anthropology departments—have been a key modality in the instilling of method and its culture in anthropological training and beyond. They are the medium by which anthropologists reveal to one another what really goes on in fieldwork. While some older anthropologists may have tired of listening to such stories, even given the sense of professional solidarity that they provide, I observe that the shape and frequency of occasionally told fieldwork stories today, coming especially from research in arenas where Malinowskian conditions of fieldwork are most challenged, are key loci of access to the seams, revealing expected and alternative practices that I mentioned. In my hearing, I find that such informal accounts of research by younger fieldworkers—apprentice and later—are less indulgent of the canonical "trial-and-tribulation" form of reporting on an initiate's testing and less naturally about Malinowskian, situated encounters than about the kind of recursivity that Faubion discusses—the moving around various kinds of sites and sources and the problems of the coherence of informing concept and research problem to which this movement—defining a cumulative density—gives rise.

Several of the essays begin to tell, or suggest the telling of, different such stories. For example, Jae Chung does this by beginning with "a feint," luring us into a comfortable tale of fieldwork (hanging out with subjects in relaxed company after hours), only then to tell us that such settings do not inform the kinds of questions with which she was centrally concerned. Her research was very much like canonical first fieldwork, but stories of this sort were not central to what her research was about. Further, the teaching experiments of Fortun and Kelty each suggest the telling of different sorts of modified traditional stories of the field. In Kelty's case, fieldwork stories would be organized around the problem of "composition" that he discusses and would not be reducible to the Malinowskian mise-en-scène.

Fieldwork stories today are thus less about a fieldwork experience bounded by the Malinowskian scene of encounter (these can always be elicited on demand, but they are not so naturally offered in corridor talk) and more about what I will describe below as the design of anthropological research for which anthropology does not yet have an adequate articulation. So increasingly, this de facto design gets articulated along the way of research and careers in changing genres of talk derived from fieldwork stories that serve professional solidarity. These are worth listening to in a different register.

The Pedagogical Role of Reading Exemplary Ethnographies

Reading ethnographies as another way of learning what the signature method of anthropology is and what it should produce as a discursive result has long been

of pedagogical importance. In a sense, students are to learn both the aesthetic standards and the procedures of fieldwork from reading their published results; in many departments, obligatory fieldwork courses have for their textbooks exemplary ethnographies. Ethnographies have served classically as the basis of thought experiments, providing materials to be "worked through," augmenting conceptual debates over description, and crucially showing what fieldwork is to be about, what is expected of it in a discipline that has been remarkably silent in a formal way about research design. After all, who else would read ethnographies with any care—no matter how appealing their romantic origins in travel?

Before the 1980s, there were classics and models of ethnography that circulated in such an exemplary, pedagogical way. After the 1980s, it has no longer been so much the classics that have circulated for their pedagogical influence, except perhaps symbolically, as texts of self-consciously experimental ethnography, calling attention to their critical, innovative aspects. In student culture, for example, one reads Michael Taussig rather than, or at least more carefully than, Malinowski. And contra the older more stable system of pedagogy based on classics, for a time these experimental ethnographies circulate in an inflationary manner, turning over every year or so, emphasizing the first or second works of younger scholars, and very much defining the marketplace of reputation on which secure careers are established (see Marcus 2007). Indeed, the considerable demand for innovation and revival of ethnography determines the primary readership for such ethnographies. Significantly, this pattern of circulation and influence continues to the present. It still creates the pedagogical models, fashions, markets, and perhaps most crucially the form of knowledge for ethnography, especially for students, who learn in a back-loaded way what fieldwork is to accomplish through strong images of how it is to do so as portrayed in exemplary ethnographies since the reflexive turn.

While I appreciate that exemplary ethnographies are powerful purveyors of what we have called in this volume the metamethods of the discipline, and that their use in teaching as vehicles for thought experiments about the conduct of fieldwork is invaluable, I also believe that they bear too much of the burden of conceptualizing research in pre-fieldwork training, since as a genre they are still written as if the fieldwork experiences that they canonically evoke are the core and limit of the process of inquiry—a presumption that we are calling into question in this volume. Also, given a broader imaginary for the research process that we are advocating, the ethnographic form itself in its current exemplars is no longer necessarily the ideal textual outcome of such a reimagined research process. In some current exemplary ethnographic works, the limits of the classic form are clearly pushed, but in my observation, this is not the lesson that is clearly drawn from them by their use in teaching within the culture of method.

In any case, how certain exemplary ethnographies become reread during the course of a project before and after fieldwork would make for an interesting topic for elaboration within a fully realized ethnographic memoir of supervision.

The Private Data Sets of Fieldwork

The "stuff" of fieldwork, its raw materials, has always been primarily between the fieldworker and her notebooks, but formerly the traditional topics of anthropology (kinship, ritual, religion, and so on) offered more exposure to this material and to its "working through" in the scholarly community in its near-to-primary form than they do now at almost any stage of processing (see Sanjek 1990): supervisor-student interactions, seminar presentations, write-up, and the production of published texts. It is much more uncertain what the data sets of various fieldwork projects are now, and there is very little discussion of them in these various contexts of training. In fact, the acuteness of the challenges to what the objects of ethnography are today makes the raw form of fieldwork material a crucial subject for direct attention and analysis in common. For example, in my experience, supervisor-student discussions are mostly about positioning moves, conceptual fixes, and how to make and support an argument; there is rarely the working from an inventory of material, but rather the negotiation of selectivity from a corpus of material, largely off-stage to these discussions. Given that training keeps the finished ethnography in view, and the fact that the ethnography is supposed to do more theoretical work of an abstract, or at least more interdisciplinary, sort than the classic ethnography as analytic-descriptive report was ever expected to perform, it is not surprising that primary materials get very little direct attention in training or in processing for publication or, for that matter, in professional reception.

Still, what are ethnography and the labors of fieldwork all about if not basic materials that have specific forms and processes of presentation in which they can be collectively discussed and reviewed at some phase of research? It is perhaps ironic that outside the culture of method of anthropology, those intimate materials of the field are probably more public in character than they have ever been. In their raw form, they exist as documents among overlapping documents and discourses of a parallel nature being produced by others—subjects, media, parallel inquiries—in the field of research (this is especially the case with the emergence of blogs as a "form" of anthropological writing; see Saka 2008 and the e-seminar based on it). As such they are subject to constant change and engagement with their counterparts even as they reside in fieldwork notebooks (or whatever other media of recording are in play today). As such, research, including fieldwork, conceived of as a design process or design studio—to be suggested

below—might allow for the capturing of this dynamic character of data through the staged presentation of basic materials at different points of development, along with continual revision. Such an opportunity is lacking now.

Greater exposure of the stuff of fieldwork by design would mean at this juncture a challenge to present practices that tend to encourage the production of the dissertation as the first draft of a book. It might be better to think instead of ways to deal more directly and processually with the personal archives from which the dissertation is produced. Perhaps the dissertation is the one phase in the current production of research where the working through of material is most exposed for review by a specific (departmental) community. How to compose materials for the purposes of scholarly communication is at the core of Kelty's innovations in teaching. The challenge is to develop forms and ways of processing, conceptualizing, and paying attention to raw material in common as pedagogy, without being overwhelmed by it. If the raw material, the data sets of fieldwork, are increasingly composed with public dimensions, amid various active collaborations and receptions in play, then again, a design process incorporating staged occasions of broad review might best serve this present absence of what otherwise gives anthropological research with fieldwork at its core, more than ever, its distinction.

Surprise and Discovery: The Expected Derailment of Original Research Plans

There is not only a tolerance for, but even an expectation of, a shift in plans in fieldwork. This has the standing of a trope in ethnographic writing, a story of "correction" as I call it: the anthropologist starts out with the idea of researching one thing, but good, promising fieldwork often leads to something completely different, unexpected, and more interesting. This is as much a part of the rhetoric of ethnographic authority as the well-known tropes establishing "being there." It is most eloquently articulated by Faubion in his essay:

> The worthiest of questions are not at all guaranteed to remain stable through the empirical course of their resolution, and what instability and mutation they exhibit make unstable and liable to mutation every one of their epistemological and ontological fortifications.

Certainly, these accounts of how what one intended to study becomes something else during fieldwork are one common currency of discussion between supervisors and students. Finding something new, a sense of discovery and surprise, and a surrogate for the traditional affinity for the exotic, no longer respectable but still traceable within the romantic side of anthropology—these are all aspects of the regulative aesthetics of ethnography that are performed in the

"turning course" stories of fieldwork. Such stories are also at the core of the sense of double agent-cy in the production of first research, discussed below.

In one sense, what these stories communicate is the essential unpredictability of fieldwork, its virtuous unruliness, and its resistance to standard ideas about research design and methodology in the social sciences. But is this deep orientation of anthropological metamethod a reason for indulging it entirely? It is worth considering how this valorization of changing the question in midcourse can be further thought through and better understood with the aim of changing, if not method, then the metamethod of training to accommodate it.

For example, I have begun to make a collection of these stories and expressions and to think in reverse about them: Could the projects in question have been conceived differently to have accommodated earlier on, before fieldwork, the turns that they cast in the rhetoric of surprise, discovery, and of what was unanticipated? In what kind of process of organized research preparation could such turns at least be conceived as satisfying and worth approving while also explicitly or implicitly not calling that process into question? In this, I suppose I contest Marilyn Strathern's recent celebration of the surplus capacity of the uncertain turns of traditional anthropological research, especially in new environments. As she says:

> Social anthropology has one trick up its sleeve: the deliberate attempt to generate more data than the investigator is aware of at the time of collection...a participatory exercise which yields materials for which analytical protocols are often devised after the fact...[Ethnography allows] one to recover the antecedents of future crises from material not collected for the purpose...to anticipate a future need to know something that cannot be defined in the present. (Strathern 2004, 5–7)

Well, yes, but there is method (or metamethod) to this restatement of the traditional aesthetic of the distinction of anthropological inquiry in new research environments. It is worth exploring rather than valorizing the virtues of established practices.

So, are there ways to reorganize the current metamethod of anthropological training around this aesthetic and enhance it? This question presages my discussion below about imagining a broader process of design for anthropological research.

Double Agent-cy

Anthropology's thriving, distinctive culture of research, composed of a cluster of informal practices and standards such as those just described, has an uncertain, often ill-fitting relationship to the demands of the larger institutional structure

and ecology of research in terms of which it must define and shape itself, for the sake both of such quite tangible "goods" as research funding and disciplinary recognition and of public and academic conversations in which anthropology would like to count as participating. The deeply regulative norms of metamethod often conflict with the larger contexts in which anthropology must be successful—and, I observe, this tension is felt and most consequentially plays out in the current pedagogical process itself. At least part of the solidarity and identity of anthropologists today is based on a premise of their own disciplinary "cultural intimacy" (see Herzfeld 1997), a shared understanding that they are playing a game of double-ness, or fancifully, double agent-cy, on the level of individual project development. There is the sense in training projects of producing research for both "us" and "them" at the same time, in different registers.

Though widely shared among anthropologists, the playing of this game is learned in training, and primarily in the crucial funding application process, which has a critical shaping or enhancing effect on some of the characteristics of the current informal training norms (e.g., it enhances the previously discussed expectation, and pridefully so, that proposed research will change course after the formal proposal has ceded center stage, as it usually does; it also tends conceptually to reify fieldwork as coterminous with "research" more than we think should be the case, because fieldwork is what methodologically stands for anthropology in its reception as a social science). The labor and importance of producing funding proposals thus often exclude a distinct marking of this double-ness in pedagogical process in favor of collusion. Such a marking would be more desirable, I believe, under present conditions.

The formulation of research within the culture of method, as described above, differs from its formulation for a funding proposal that responds to the authoritative norms, forms, and categories of the social sciences, with which the former fits badly. Under current conditions, the former deserves its own expression before or aside from translation into the latter. Such translation is possible, and anthropology has in fact been very successful in fitting into funding structures, but there is neither the time nor the resources in the training process to give to the parallel development of a proposal that more fully reflects anthropology's culture of metamethods and to perform this double-ness in a satisfactory, balanced way.

As it now stands, the formal proposal requirements push the expression of the alternative discussion of research—how it really works—to the realm of the informal and the marginal and tend to make it a bit of the outlaw as well. This has consequences for all of the issues that I have been raising about how anthropology's metamethods affect its research projects. These issues exist in the shadows of more formal procedures that are in tension with them. Better would

be a transcending of this awkward situation of double-ness by a developed expression, a remodeling, of its metamethods, drawn from an auto-ethnographic appreciation of how they work at present, and the eventual offering of this formulation or model to the institutional ecology of support for anthropology as the appropriate standard by, and form in which, its proposals should be assessed. (Actually, there is a commission under way to advise organizations like the NSF about how to evaluate "qualitative research" in the social sciences on its own terms; my suggestion here is in line with this promising development.) It would mean substituting for fieldwork, as the emblem that indeed stands for anthropological method in its present institutional environment of support, a broader conception, such as a design process, as discussed below. In so doing, the awkward aspects of the double agent-cy game, especially visible in training, might be reduced.

Suggestions

Finally, here are some specific ideas for reshaping the crucial training process at the beginning of careers through which anthropologists learn practices as well as an aesthetics or embodied ideology of method to think about them. They are about strategy (metamethod, directed toward the pedagogical process), rather than tactics of inquiry (research techniques—e.g., how to do interviews, how to count things). They are presented in broad strokes and emerge from memoirist observations such as the preceding as well as from my engagement with the essays in this volume.

The first suggestion is a large one, about an alternative concept of form or framework as distinctive of the anthropological research process. The other three suggestions can be viewed either as aspects of this alternative form or as potentialities, as under-realized features, within current pedagogy. Though advocating more than mere "tweaking," these suggestions follow what I have called the seams of alternatives already present in current practices under the challenge of changing conditions of research.

A Design Process

We come to the conviction that some broader, elaborated view and model of the anthropological research process is needed today, rather than just "fieldwork," to serve especially the task of training ethnographers-in-the-making. While acknowledging that anthropological research is mostly composed of projects of individual conception and execution and is about defined cases—this much

is guaranteed by the discipline's mythic construction of fieldwork—virtually everything else about research these days pulls a project into collaborations, collectivities, institutional arrangements, and networks of various kinds that are not simply its objects, but are integral to the process of making knowledge out of the traditional individual, case-bounded project of fieldwork.

Once again, to quote Faubion:

> [Anthropologists] offer us anthropology as a topology that refuses to abandon the particularity of its various cases even as it manages to pursue and indeed to grant pride of place to a topologically modulated enterprise of comparison. [Research today is] executing a practice in and beyond the literal site of the field that has no need of an ethnographic totality or of any contribution to some generalist's gamut of human types in order to assert its anthropological credentials.

My candidate for a concept that conceives of research practice in a way that provides the long view, encompassing the phases of research today in a coherent way, retaining the focus on individual research while incorporating and making visible and accessible to the professional community the complex relations that compose it, is that of the design process. I am not thinking of the idea of formal research design, which is a standard category in the implementation of social science methods, but of design as it is defined in studio fields like art, graphic and industrial/product design, and architecture (the latter of which I have experienced as a process, and think of as a model in making this suggestion).[13] In design processes with which I am familiar, the individual and collective as agents of knowledge

13. There is much diverse writing on the design process and its pedagogy in these fields that can generate models with which one can experiment in deriving analogous design frames for developing an alternative research process in anthropology that both encompasses Malinowksian fieldwork and allows for systematic treatment of the ways that it is getting stretched, redefined, and contextualized in current projects. One work on the pedagogy of teaching graphic design that I recently discovered through a (graphic) presentation of it at a conference in Oslo is by Theodor Barth and Maziar Raein (Barth and Raein 2007). Barth is an anthropologist who works closely with Raein in developing the pedagogy of design development in Oslo. With appropriate translation, I found aspects of it particularly suggestive for how the teaching of ethnographic research might be thought through in terms of design process. There is constant feedback in the development of a design; research is built into the conceptual work at the core of this development (the idea of "a holding pattern," a theme of their cited article, conveys how the conceptual work from beginning to end in ethnography "hovers" in the operations of fieldwork); and key ethical issues have cogency, not so much in the process of data collection itself as in the transaction that makes the materials of ethnography public, comparable to the stage when a design is turned over to a client with whom one has had a relationship during the entire process. In my view, some of what they have written fits, some not, but I am finding that the frame of design pedagogy is "good to think" in reconceiving the research process in anthropology. Earlier, I had found inspiration in the work process of conceptual, site-specific artists (Marcus 2008a), whose conduct of fieldwork-like research within the production of installations or

production are constantly in play. There is conceptual and practical rigor in applying ideas. There is reporting and constant feedback by diversely composed audiences from beginning to end. The final result has multiple accountabilities which are thought about through the entire project, and so the final result is not final, at least conceptually—there is an ideology of open-ended design and of a work being a solution that is subject to revision by later and other work.

It seems to me that anthropological research today rethought as a design process would encompass and preserve classic fieldwork perhaps still as a core modality. It would, however, both relativize its functions and blur its beginning and end in conceiving it within the broader contexts and operations that so much research now entails, as the essays of this volume illustrate. This would lead immediately to the three other issues that I take up briefly below: incompleteness as a norm, at least of the dissertation phase of a project; the more complex role of collaborations in producing individual projects; and the more formal or conceptualized incorporation of the receptions of the project into its design and doing.

Further, a model of a design process would map easily onto the research process that we have today and would give it an articulation for which we have been calling. At least such a model would serve as the framework for a practical discussion of many of the issues that we have raised here about the workings of the present informal culture of metamethod in anthropology, most visible in the pedagogy of first research leading to the production of dissertations, but not ending there. Moving to understand this venerable disciplinary process according to one or another model of a design studio would bring into the open—for anthropologists, students, anthropology's publics, and for institutional supporters demanding accounts of its methods in return for funding—the longstanding distinctiveness of the experimental ways that anthropologists have produced ethnographic knowledge. It would usefully displace the mythos of fieldwork and the informal professional culture that supports it, which no longer offers sufficient clarity about the research process and its dimensions, either to anthropologists themselves or to their subjects and publics. It would finally give this experimental dimension of anthropological research full expression as a framework of practices rather than remaining just a professional ethos and set of regulating aesthetics.

I cannot actually lay out a blueprint for this idea here; there are many possibilities. Such a proposal deserves its own full account (in my case it probably would depart from the procedures of the architectural studio to model similar

---

works of conceptual art, resembles that of ethnography, but their practices lacked both the coherence of process and the concern with pedagogy that I have found in the literatures on design.

practices for anthropology) and certainly lots of discussion in response. Because a design process would map readily onto what we already do traditionally as anthropologists, discussion of its feasibility alone would provide the framework to systematically question the process that we have now, as registered in my above observations. A design process should be open-ended. It should incorporate scenarios of anticipation and changing course. It requires the presentation for review of an ethnographically sensitive research imaginary before the undertaking of fieldwork that overreaches it and is revisable in terms of it. Research conceived as a design process keeps attention focused on material—data sets—all along the way and insists on results that are closely accountable to it. Thus, it encourages theoretical work at the level of material—the "stuff" of fieldwork as I called it—and privileges found concepts that emerge from it. It also looks beyond the confines of its own production to response and revision. While still preserving the responsibility of individual work, it recognizes collaboration as a normative principle, incorporates broad receptions, and finds a place for the anthropological community in this. What could be better, given how anthropological research is moving today anyhow?

A Norm of Incompleteness

Any traditional fieldwork project defines a massive task compared to what a researcher can practically hope to do with the time and resources available. Thus, a rhetoric of incompleteness is very common in finished ethnography. It is sometimes a hedge for inadequate evidence or analysis; it is sometimes a pro forma apology. It sometimes reflects a certain edge of anxiety or tension about the way an individual researcher handles both the limitations and the possibilities of the discipline's regulative culture of metamethod. We are most interested in the latter aspect motivating this rhetoric. Both under present conditions and in research imagined as a design process as just discussed, incompleteness would be a positive norm of practice, even a theorem of practice, expected of kinds of inquiry that remain open-ended even when they are "finished." Incompleteness is a dimension of thinking about what can be said about one has done. It is not about incompleteness in relation to the general and future unknown, but in relation to a design or research imaginary that has been thought through ethnographically but investigated only in part (e.g., the dissertation phase of research that produces first projects). That partial knowledge, so to speak, which is the product of first fieldwork, is partial in relation not to some unknown or vaguely conceived larger whole (in my view, this leads all too often to the justifying, dominating moral discourses of so many ethnographies today), but to a known and carefully conceived incompleteness, a ground or terrain of possible ethnography that is deeply imagined as such and in terms of which the partial results of fieldwork

are specifically argued. Incompleteness thus defines a norm for contextualizing conditions of fieldwork research today at a thoroughly imagined ethnographic level for which the researcher should be responsible.

For example, the state or economy is not the context for a bounded site of fieldwork; certain ethnographically imagined processes and their connections to the foci of fieldwork are. The anthropologist may not do fieldwork in these contextualizing realms, but she projects an ethnographic imagination upon them as if she had. I would say most if not all objects of interest today can be known in this speculative way. And some degree of so doing could be part of a norm of incompleteness and the pursuit of research projects as they are today. The processes of a design studio would simply make this operation of incompleteness as a practice visible, indeed normative, and metamethodological in a formal way.

Design processes thus call attention to such edges of a project and develop contexts of discussion for them in the same terms and styles that characterize the work of the focused individual effort of fieldwork and its defined objects of inquiry. But even without the imaginary of research today as a design process, a norm of incompleteness under current practices could provide context, ground theory, and calm anxieties in researchers who move recursively around a field of inquiry and are uneasy or hedging about the partiality of what they are doing. They might otherwise embrace incompleteness as defining knowledge of something also in a speculative way. Scenarios are the instruments for dealing with the specificities of incompleteness by the informed imagination, and they can be implemented to good effect now or within some future regime of anthropological research as design studios.

Collaborations

Collaborations have always been integral to the pursuit of individual fieldwork projects, and their importance has been widely acknowledged at least since the *Writing Culture* critiques of the 1980s (see Lassiter 2005a). They never have been, however, an explicit aspect or norm of anthropology's culture of metamethod. The fieldworker, for example, is not held accountable or judged by the quality of his collaborations and his ability to manage them. Yet, today, collaborations of various kinds are increasingly both the medium and objects of fieldwork, quite aside from the long-established collaborative character of the relationships between fieldworkers and their once labeled "key informants" in the Malinowskian scene of encounter.[14] Virtually every observation made in this

14. In a recent paper (Marcus, in press), I attempt to describe some of the distinctive current predicaments and imperatives that move anthropologists to experiment with forms of collaboration in their research projects. One major ongoing concern of the Center for Ethnography that I founded

volume about relativized fieldwork in broader contexts of research practices suggests the incorporation of explicit norms and forms of collaborations into the culture of metamethod. If this were to happen, then anthropology would have to develop far more varied ideas about the ethics and the nature of fieldwork collaborations and the significance of what gets transacted in them.

For example, nowadays relationships with experts or counterparts (and not just colleagues or consultants) very often provide the intellectual capital for conceptually defining the bounds of fieldwork. Such relationships are neither outside fieldwork, nor are they one-dimensional in purpose. They define contexts of mutual appropriation when the anthropological project itself sometimes undergoes the "turning of course" that I have described as one of the common expectations for it. So, collaborations with what I call epistemic partners, those who come to inform the very conceptual frame of the research, push at the boundaries that keep the "scholastic" space (Bourdieu 1990) distinct and distant from the situated intellectual work of subjects. I suppose this might be seen to constitute a latter-day threat of "going native," made more threatening perhaps when the practices studied overlap with the intellectual apparatus and terms of the observing analyst. This movement of a project's intellectual center of gravity into the bounds of fieldwork probably can't be helped, nor can it be contained by keeping this operation normatively invisible in the culture of metamethod.

Under the present conditions of this culture, I see no way for collaborations to gain explicit identity as a form with normative status in terms of the practice of metamethod, since the latter so resolutely focuses on individual research and its achievements. The promise of remodeling the culture of metamethod as a design process is a different story, since such a process constantly incorporates collaborations of different sorts as part of its fabric—within the studio, out there in the field—as very visible and explicit parts of producing work, transacted in the constant feedback and revision by which a design process is characterized. Within the framework of design, such collaborations would be bound to become a much more explicit concern for anthropology's remodeled culture of metamethod, as

---

when I moved to UCI is "Ethnography In/Of Collaboration" (http://www.socsci.uci.edu/~ethnog/). Pedagogical experiments are part of this project as well, the most notable example of which so far has been the sponsorship of "para-site" exercises within the frame and process of dissertation projects that otherwise are conceived and conducted in the individual voice of the lone, responsible ethnographer. The para-site is a surrogate for collaborative design in that at appropriate phases of a project it injects carefully staged (and designed) events like seminars in which ethnographers, research subjects, and select others do conceptual work critical to the development of the research (this is similar to the reviews that punctuate design projects in all phases). This operation introduces a collaborative dimension into fieldwork as conventionally conceived and in so doing suggests alternative models for the fieldwork research paradigm itself that can be explored through design thinking that I am suggesting here.

eventually something that the assessment of "good" ethnography depends on by the aesthetic judgment of craft.

Receptions

The widespread call today for a public anthropology (Marcus 2005) already signals the intense interest of anthropologists in the responses to their work by the publics of varying composition and scale that it is able to touch. These responses matter more to many anthropologists, at least affectively, than professional responses to their work within the discipline, which I believe are weaker in intensity, and often less substantive, than sources of broader reception—both academic and nonacademic. The question for us here, again, is how this interest in receptions can be built into the fabric of research projects themselves, that is, how it can become part of the culture of metamethod. The question especially, given this volume's focus on pedagogy, is how such an interest can become a dimension of the process of first research. This is not just a question of what the subjects think of what the anthropologist has written about them—this sort of exercise began to be developed after the critiques of the 1980s (Brettell 1996)—but how diverse responses to a project as it develops become part of its integral data-sets, so to speak, the raw materials that need exposure somewhere in the process of research, certainly in dissertation research. Folding receptions into the metamethods of anthropological research at this stage would surely influence the habits and aesthetics of more mature research no longer subject to the specific constraints of training, but still shaped by its regulative ideologies.

As with collaborations, building norms and forms for reception into current research practices is difficult to accomplish, except piecemeal, where there are such tendencies anyhow as legible seams of particular current projects. The systematic integration of these tendencies depends on the implementation of a larger frame for research that, again, I have argued for here as a design process. In the manner of design studios, through the constant phased critiques and discussions of research by diverse audiences, including subjects, receptions would find an integral place in the production of anthropological research.[15]

15. This final suggestion on incorporating "reception" as an integral dimension (and site) of contemporary fieldwork prompts a final source of experiment where alternative forms of the classic research process of anthropology are emerging. This concerns blogging as a medium of ethnographic research and opens for consideration the whole arena of new forms of digital communication with the introduction and expansion of the Internet. I avoided this domain in my essay (though it is taken up in the essay by Kelty, who is a scholar of emerging forms of communication) because I am not sufficiently familiar with it and because the state of development of this medium in research is too inchoate at present, although I am sure that it is likely to be the eventual basis for the reconfiguration

Envoi

At its very best, in the supervising of ethnography-in-the-making, students gradually become one's teachers. In my passion for crafting new practices of training out of anthropology's old ones, this has especially been the case with the essays collected here, in their distanced insights of what their authors have made of fieldwork as they have developed contexts of work beyond it. In this, they have provided me with new ideas for inhabiting this venerable set of practices that stand for as well as perform the discipline, and thus, they have renewed my career-long romance with anthropology in a different and optimistic register.

Appendix: A Note on the Literature Relating to Fieldwork and Ethnography

Of the voluminous, long-standing, recent and accumulating literature on fieldwork in anthropology and related disciplines, the influential volumes by Vered Amit (1999) and Akhil Gupta and James Ferguson (1997a) are the two sources with which this volume is perhaps most closely engaged. We share with these volumes the sense of the altered circumstances in which fieldwork is undertaken today as well as the necessity of rethinking it conceptually. Both of these volumes are indeed concerned with what we term metamethodological aspects of fieldwork (that is, fieldwork as shaped by certain expectations of form and practice encountering changing objects of study and conditions of implementation), but neither develops a framework or a set of questions that delve into the professional culture of research and, within that, the pedagogy of first research, to focus on these aspects. We believe that this is the original contribution and advance of this volume.

Still, there are hints, clues, and discussions embedded here and there in the huge literature on fieldwork, primarily as method, that touch on the meta-

---

of conventional research practices, about which I am currently thinking in terms of design. Still, I want to draw the reader's attention to an "e-seminar" (May 20–June 2, 2008) occurring as I write these notes, administered by the EASA Media Anthropology Network (http://www.media-anthropology.net/workingpapers.htm), that in my view is the most stimulating ongoing source from which to learn of ethnographic research conducted on and through alternative and emerging media. This seminar concerns a paper on blogging as a research tool by Erkan Saka (Saka 2008). Saka is a Turkish graduate student at Rice, about to complete his dissertation on journalism, Turkey, and the European Union. He has produced a highly successful website and blog since he returned to Turkey three years ago that serves multiple functions in his fieldwork. The interesting exchanges produced by the e-seminar suggest that Saka's work is a kind of design studio itself for the revision of the conventional forms of ethnographic research practice. The e-seminar on Saka's blogging provides stimulating discussions that cover each of the topics that I have been describing in the final section of this essay as central to making over the conventional terms by which fieldwork is produced in training.

methodological concerns of this volume. This literature is located primarily in the disciplines of anthropology and sociology, but has developed markedly in recent years, as the popularity of and demand for ethnography has grown, into an industry of manuals and topically specialized treatments of more generically conceived "qualitative methods." Publishers, most prominently Sage, AltaMira, and Routledge, have developed extensive lists in this area. As a bibliographic link to my introductory essay and this volume, I cite a range of "methods" books in which discussion of issues regarding the craft of fieldwork as a distinctive professional culture of inquiry is more or less legible: the comprehensive seven volumes of *The Ethnographer's Toolkit* (LeCompte, Schensul, and Schensul 1999) and the three editions of the massive *Sage Handbook of Qualitative Research* (Denzin and Lincoln 2005) can aptly stand for the large manual-style literature on fieldwork and its various topics (interviewing, participant observation, and so forth). In anthropology, many of the best known and most effective texts on fieldwork address undergraduates or graduate school novices (see especially Agar 1995; Crane and Angrosino 1984; Spradley 1979, 1980; Watson 1999). Recent overviews of and readers on anthropological fieldwork show clearly the impact the analysis and critique of ethnography as texts and a genre of writing from the 1980s have had on treatments of fieldwork practice as method (see Lucas 2000; Robben and Sluka 2006). Also useful for getting at a kind of implied culture of method in terms of what is expected from fieldwork as written result are two important works on fieldnotes (Sanjek 1990; Emerson, Fretz, and Shaw 1995).

Two works that parallel my conceptual but not methodological apprehension of the details of the ethnographic (or qualitative) research process are Howard Becker's *Tricks of the Trade* (Becker 1998) and John Law's *After Method* (Law 2004), although neither is explicitly concerned critically with a professional or pedagogical culture (sociological or anthropological), as I am. In particular, Law, known for developing Bruno Latour's actor-network theory as a sociological method, shares my sense of how methodological traditions (he is not specifically concerned with the ethnographic tradition) do not serve well the experience of trying to do research in the present, but he tends to emphasize coping with factors like fluidity, multiplicity, and messiness more than I do. Perhaps, this is because he critiques established methodological practices in sociology that abhor these factors, while I am dealing with a disciplinary tradition that has long embraced them—to a fault perhaps.

Finally, while this volume addresses contemporary developments in the distinctively anthropological tradition of method, it is also concerned more with the comprehensive process that produces ethnography rather than with what ethnography produces—that is, more with the fieldwork process in the broader

research contexts of specific projects than with ethnography itself as a distinctive kind of writing or style of analysis. But, in terms of separate literatures, this is not a hard and fast distinction, since I have often found more attention to and insight regarding the metamethodological issues of interest here in the literature on ethnography as genre and research product than in the usually more didactic and less critically acute literature on fieldwork. This is particularly the case with sociological studies of ethnography (e.g., see Denzin 1997) and works of sociological ethnography that include interesting metamethodological reflections on the process of inquiry and its requisite conceptual artifice (from the myriad examples, the following have been among the most important for me: Burawoy 2000; Glaeser 2000; Waquant 2003; Willis 1981). These reflections, in my view, are different from typical examples of the "reflexive turn" in anthropological ethnographies from the 1980s forward (see fn. 1). Yet, in anthropology, there are also many examples of recent ethnographies that have within them specific suggestions about rethinking the traditional compass of fieldwork practice in a metamethodological frame (Holmes 2000; Maurer 2005; Riles 2000).