Abstract (Document Summary)

A few years ago, the American Sociological Association was prompted to adopt a Code of Ethics in the wake of several cases in which sociologists admitted to deceiving their informants in order to gain or maintain access to information. (The cases were profiled in 1997 in Lingua Franca but played out in mainstream professional association journals.) All of these involved U.S. based participant-observation research. In one case, the sociologist Carolyn Ellis published an elaborate mea culpa concerning (among other things) deceptions she had perpetrated on her research subjects over a several year period. Ellis had returned year after year to her host community on the Chesapeake Bay, allowing her subjects to view her as "just Carolyn coming to visit." Even during the most intensive phase of her field research -- when she was an undergraduate and then doing her dissertation fieldwork -- she let her hosts know only generally that she was a sociology student, writing a paper about "fishing" (not letting on about her central interest in the dynamics of cultural conservatism and isolation). Ellis never imagined that her subjects would become aware of her writings and had no intention of showing or discussing her sociological study with them. Indeed they only learned about her book, Fisher Folk, from another sociologist with (Ellis claimed) competing interests in the community. Even in her confessional mea culpa Ellis reasserted, "we can never tell subjects everything...or reveal the total character of our research goals." Her solution to what she called, quite generally, the ethical "quagmire" of fieldwork was, firstly, to assert the value of reclaiming her "personal relationships" with her former informants, and then to devote herself to autobiographical projects: a strikingly subjectivist strategy for obviating the distinction between the knower and the known. Nancy McDowell's book, entitled The Mundugumor: From the Field Notes of [Margaret Mead] and Reo Fortune, is particularly interesting for the light it sheds on certain differences between historical and anthropological presuppositions concerning sources. The book is an ethnography of the Mundugumor, a Papua New Guinea people among whom Margaret Mead and her then husband, Reo Fortune, did about three months of research in 1932, and who figure in Mead's 1935 Sex and Temperament, as well as in a famous social structural analysis by Claude Lévi-Strauss. The Mundugumor are a people whose cultural norms, according to Mead, encouraged both men and women to act in ways that Americans might consider "male." Partly because of Lévi-Strauss's attention, Mead returned to her notes in the early 1970s intending to write a book on Mundugumor kinship. She asked McDowell, then embarking on fieldwork just upstream from the Mundugumor villages, to complete the project, should she fail to do so. Indeed, Mead died without having written the book. So, after doing her own fieldwork and enough writing to gain a tenured teaching position, McDowell made good on her promise to Mead, using Mead's fieldnotes and Mead's transcript of Fortune's otherwise indecipherable notes, most of which are archived at the Smithsonian.

(2) Anthropology is peculiar in several ways that I don't have space to detail here. I'll just mention that we ourselves (and certainly also folks in other fields) often call anthropology "interdisciplinary." Anthropologists get borrowed from a lot by practitioners in other disciplines; we also borrow a lot. The field is itself an uneasy alliance among "subfields" that span the humanities-social science-natural science divisions: interpretive cultural anthropology on one hand and evolutionary paleoanthropology on the other. So one of the key challenges of the book on which I am working is to articulate what's "disciplinary" about anthropology. At the same time, it is absolutely true that anthropology is a friendly place -- both at the undergraduate and graduate levels -- for people who want a certain intellectual flexibility and playfulness while, at the same time, wanting an unambivalently grounded, out-in-the-world subject matter. Anthropologists have always studied people "everywhere." (That is, it is a myth that, in the past, anthropologists used to only work in places like highland Papua New Guinea. In fact, we were always in New York and Ibadan too.) While our topics have shifted and even expanded in
recent years, there’s a basic continuity in our discipline-wide, transnational commitment to face-to-face ethnographic research. Unlike what I take to be the current situation in literary studies, contemporary anthropology is not in inner turmoil about the loss of that basic substantive core.

This brief paper provides a suggestion of the topics covered in two chapters of a book -- tentatively entitled Anthropology Among the Disciplines -- that I am writing on “disciplinary practices” (by which I mean something broader than research methods: this is also the topic of a graduate seminar I teach every other year in Princeton’s Anthropology Department). The hub of the book is anthropology, but it is equally concerned with disciplinarity and interdisciplinarity. I’ve been tracking current rethinking of how anthropology is done and how its relations have been shifting with respect to other knowledge practices. But to do this “anthropologically” I have approached the discipline in terms of its internal differences and also comparatively. Seeking out a reticulum of contrasts and overlaps among neighboring disciplines has helped me get a grip on backgrounded assumptions concerning how different research practices are evaluated -- good and bad, reliable and unreliable -- in contiguous disciplines.

Background: How I Got Here From There

When I first began in anthropology -- as a graduate student in the mid/late 1970s at Columbia University and as an assistant professor at Princeton in the 1980s -- my work focused on Papua New Guinea. In one sense, I did absolutely archetypical, classic ethnographic research: long-term vernacular fieldwork in a rural community, in one of my discipline's key ethnographic areas, and on a topic steeped in disciplinary traditions. I was interested in understanding the inner workings of a notoriously complex system of "ceremonial" (gift) exchange, which required that I gain the kind of local linguistic competence that would enable me to make sense of metaphorically-loaded political oratory and become fit enough to hike about in highland mountain valleys, talking with pig farmers about their socio-political relationships, not to mention attending spectacular ceremonies from time to time.

But in another sense, my work was not strictly classic. Along with many other young anthropologists in the 1970s and 80s, I was busy working feminism into the weave of anthropological research and theory. "The anthropology of women," then "gender studies," was a presupposition, explicit at first, tacit thereafter. Consequently, my study of gift exchange was very much about detailing the engagement of women in exchanges (something that hadn't been much reported in the New Guinea highlands, an area infamous for male dominance). In addition to exploring conflicts between women and men -- as well as among men and among women -- over the value and purpose of events, I was interested in gender meanings: e.g., how attributing maleness/femaleness to things or activities was one way people expressed evaluations. My cohort was also working its ways through Marxian and structural theory to something "post-": anthropology and history students formed study groups together, reading Fernand Braudel and Claude Lévi-Strauss, and arguing about all sorts of issues. So, working in Papua New Guinea, I was attentive to the historical (that is, most definitely not "timeless," static, or unreflective) character of the ceremonial events I was documenting. That meant, among other things, that I tracked the ways in which Papua New Guinea national currency -- money -- worked its way from rural cash cropping projects and wages into gift exchange: how global and local relationships interpenetrated.

I did fieldwork in Papua New Guinea between 1977-79 and again in 1983 and spent many years writing and publishing that work. But my interests were also shifting. I became interested in pursuing the implications of a certain irony: on one hand, Pacific area anthropology has an iconic status in the discipline as the locus of foundational work and as the continuing scene of disciplinary innovation; on the other hand, stereotypes about Pacific Islanders as so-called "primitive people" -- pervasive and stubborn in American culture at large -- stigmatize not only folks like those I lived with in P.N.G. but also anthropology as a whole. I began to think of writing about the dilemma this posed for the discipline and for Pacific studies. Similarly, my research interest in gender and in anthropology/history
connections -- especially after I began teaching at Princeton in 1981 -- placed me in interdisciplinary spaces. I had many opportunities to think about the peculiar position of anthropology in these spaces. Over the past decade or so, my research interests have centered, in effect, on questions of (inter)disciplinarity. But I view this turn of events as very much connected with my earlier Pacific area work.

I began my explorations into disciplinarity with forays over the border into history (which I knew best and which nowadays anyhow -- we anthropologists like to think of as akin to what we do). But I soon got drawn to other borders: into sociology, literary studies, and also journalism. This paper sketches some of the corners glimpsed from a comparativist vantage.

Disciplinary Practices and the Value of a Focus on Ethics

I am interested in a number of "suggestive boundary problems" between anthropology and its neighbors made evident if we consider their respective "ethical" presuppositions -- their respective senses of proper or improper research practices. My thesis is that, whatever else they are, disciplines are moral orders. Disciplines constitute themselves in implicit and explicit dialogue, or contest, with one another (as well as with extra-disciplinary modes of knowing the world) not simply as substantively distinctive but also as better or worse, even proper or improper -- that is, morally weighted -- knowledges. This moral weighting may account for some of the power of disciplinary boundaries -- their insistent if somewhat subterranean reproduction even in face of their apparent irrationality and arbitrariness.

As moral orders, disciplines are taught and assimilated, or policed, reformed and subverted, at several junctures. Over the past thirty years or so, anthropologists have made it their business to understand some of the world historical conditions for the making and reworking of particular subdisciplinary emphases. For example, the colonial-metropolitan relationship is nowadays understood as a properly "anthropological" topic of investigation whereas, a generation ago, it was excluded even by many critical scholars: it was a context for -- not the content of -- ethnographic work. In autobiographical, critical, and other reflexive ethnographic modes, anthropologists have also invested much effort over the past number of years in rethinking their conventional choices of topics and places: the increasing importance and influence of comparative gender and sexuality studies is an example of a topical shift, while increasingly ambitious, deeper studies in Euro-American places (whether of kinship or biotechnology, often both in one stroke!) is an example of a change of place. A focus on shifting topics and places has led many anthropologists to conclude that their traditional disciplinary boundaries are "blurring" or dissolving. After all, historians, anthropologists, sociologists, literary scholars, and others appear to be converging on these same interests and locations, and apparently by any means necessary.

However, while we're no longer so naïve about the sociopolitical factors shaping what and where we study, there's been relatively little attention to how we do our work: to what we might call disciplinary practices. By disciplinary practices I mean the largely implicit legitimizing relations of knowledge-production: those everyday (although by no means necessarily routinized) negotiations over what will constitute "real or good anthropology" (or whatever); those relations that enable and enact the very shifts in the field-defining boundaries and values -- notably among them the embrace of gender and sexuality studies -- that we variously applaud or deny. Insight into disciplinary practices may be gleaned by attending, for example, to what goes on in graduate training, job interviews and tenure reviews, funding and publication evaluations, and in fieldwork and other research or work venues. The constraints and double binds entailed in these key "disciplining" arenas are facts of which junior scholars, including graduate students, in all fields are acutely aware.

Compared with the attention that anthropologists have paid to large-scale institutional frames, our exploration of the everyday experience of disciplinarity has been mostly anecdotal and unsystematic. In effect (to close the circle) we haven't yet treated disciplinary practices as properly ethnographic objects. While their analysis is not yet "real anthropology," it should be. Something like an ethnography of anthropology, in the context of a broader comparative ethnography of knowledges (disciplinary and otherwise) holds promise to contribute powerfully to our understanding of contemporary experience by
training critical attention on the implicit meanings and values defining kinds of "expertise," and ordering different ways of knowing the world.

As it has in other domains, one might expect this type of attention to sit askew of commonsense understanding, which nowadays is (as I've noted) all about interdisciplinary convergences: even about the transcendence of disciplinarity. While I don't think that our desire for something like disciplinarity will be undermined when we take it apart concretely, I do think that we'll acquire a deeper understanding of the difficulties many have experienced in cross-disciplinary engagements. Ethnographic attention may bring to light the dynamics of differences between anthropology and neighboring historical, literary, and social scientific fields. In short, comparative and ethnographic attention to research practice -- and particularly to the ways in which ethical presuppositions underwrite disciplinary truth claims -- can deepen our understanding of the ongoing "refiguration of social thought" in which we are all, one way or another, taking part.(4)

Anthropology and Sociology: Positioning the Researcher

I've been pursuing these ideas by looking at the research practices of anthropology in relation to history, and also sociology, literary studies, and journalism -- for example, reading out from ethical controversies within some of these areas to interdisciplinary disputes over proper research, and talking with the principals and observers in some of the cases. To illustrate the issues, I'll say a few words about anthropology and sociology, disciplines that my undergraduates typically have trouble distinguishing on topical grounds. After that, I'll consider anthropology and history briefly.

A few years ago, the American Sociological Association was prompted to adopt a Code of Ethics in the wake of several cases in which sociologists admitted to deceiving their informants in order to gain or maintain access to information. (The cases were profiled in 1997 in Lingua Franca but played out in mainstream professional association journals.) All of these involved U.S. based participant-observation research. In one case, the sociologist Carolyn Ellis published an elaborate mea culpa concerning (among other things) deceptions she had perpetrated on her research subjects over a several year period. Ellis had returned year after year to her host community on the Chesapeake Bay, allowing her subjects to view her as "just Carolyn coming to visit." Even during the most intensive phase of her field research -- when she was an undergraduate and then doing her dissertation fieldwork -- she let her hosts know only generally that she was a sociology student, writing a paper about "fishing" (not letting on about her central interest in the dynamics of cultural conservatism and isolation). Ellis never imagined that her subjects would become aware of her writings and had no intention of showing or discussing her sociological study with them. Indeed they only learned about her book, Fisher Folk, from another sociologist with (Ellis claimed) competing interests in the community. Even in her confessional mea culpa Ellis reasserted, "we can never tell subjects everything...or reveal the total character of our research goals." Her solution to what she called, quite generally, the ethical "quagmire" of fieldwork was, firstly, to assert the value of reclaiming her "personal relationships" with her former informants, and then to devote herself to autobiographical projects: a strikingly subjectivist strategy for obviating the distinction between the knower and the known.

In this and other cases, sociologists have explained their research stances as necessary to ensure reliable observations. What made their cases problematic to colleagues was not deception per se, but its management. They and their colleagues were in agreement that if one's research subjects were to know the guiding hypotheses of one's research, then they would either try too hard to help the researcher attain his or her ends, or else try to foil them. Either way, they would not act "natural." Sociological analytic "authority" depends on convincing readers that the data has been inoculated against such contaminating "observer effects." The legitimacy of this stance may be gauged by the persistent marginality, in sociology (and even more so in social psychology, political science and economics), of participant-observation fieldwork (together, more generally, with qualitative research and, within that, interpretive and reflexive approaches), all of which are understood to be quite vulnerable to contamination relative to other research strategies.(5)

Ellis and other sociologists whose work has come under scrutiny attempted to fade into the background, so to speak. Their deceptions were surely facilitated by the fact that they were working in
their "own" countries (albeit with subcultures to which they did not themselves belong), where they could in some sense "pass." But those deceptions were authorized by disciplined presuppositions about good, authoritative sociological knowledge. The point is that properly sociological truth claims have distinctive ethical entailments.

There are multiple ways of comparing these sociological cases with anthropological ones that we could review. For present purposes these cases help illustrate the place of deception in sociological fieldwork by defining its outer limits: the point is that deception is normalized. The scandal in these cases inhered in an exaggeration or over-reaching of convention, and debate centered on where exactly to locate the border between legitimate and illegitimate obfuscation of the investigator's identity or interests.

If one thinks about major anthropological scandals, or more mundane discussions of ethical conundrums, hypothetical or actual, published in the American Anthropological Association's monthly newsletter over the years, posted on the Association's website, available in edited collections and, by-the-by, in our hefty literature on fieldwork experiences, the deception of informants is not a prominent issue. Instead, our imbroglios have revolved, for example, around questions of intervention (the provision of medical or other aid, taking sides in a conflict). And, in especial contrast with the sociological cases I have referred to, they have involved concerns over the adequacy of an anthropologist's immersion in and engagement with the lives he or she writes about. Consider Derek Freeman's claims that Margaret Mead had not actually lived with her Samoan hosts, and that her adolescent female interlocutors had "put her on" about their sexual activities. Given how prominent Mead had been from the 1930s through the next half-century or so, the story broke on the front page of The New York Times!

What was scandalous -- or at least authority undermining -- was the suggestion that Mead's methods had left her so ethnographically naïve that a gaggle of Samoan teenage girls could deceive her. The flip side of this blunder is the figure of the fieldworker who overreaches immersion and "goes native" (still illegitimate despite the discipline's on-going rethinking of the ethnographic value of being native). There are still a host of assumptions to unpack here, but the point for present purposes is that anthropology and sociology are, as moral orders, interestingly different even when we consider an area of overlap (qualitative field research).

My sense of a difference between anthropological research practices on the one hand and sociological ones on the other derives partly from discussion with sociologists about these and other cases, but also from personal experience on Princeton's own Institutional Review Board (what many refer to as the Human Subjects Committee) as well as wider research I have undertaken on IRBs. The mandate of IRBs is to protect the interests of research subjects. But IRB protocols were originally designed with medical research practices in mind; even nowadays, these protocols presume one generalized social science standard, in which deception is a given in authoritative research. Deception is not, in itself, considered a violation of human subject interests in medical or social and behavioral science fields. There's nothing new here: it is an obvious entailment of any proper "clinical trial" that research subjects cannot know whether or not they are in a control group (receiving a placebo instead of the drug being tested, for example). What is at issue for IRBs is the evaluation of research design so as to limit deception to that which is strictly necessary and to ensure that investigators have plans for mitigating its negative effects on research subjects after the research is over.

Given anthropological research ethics, there is nothing particularly remarkable about an IRB insisting that investigators make efforts to ensure the confidentiality of sources. However, a Board's demand that researchers detail their provisions for adequately "debriefing" their research subjects when the research is finished is rather more notable. It contrasts with the ethnographic ideal, evident in numerous autobiographical accounts of field experiences, published or not, of explaining our research to our interlocutors at the outset of fieldwork (and indeed, more often, all along).(6)

I am not saying that sociologists and behavioral scientists are nasty liars and that anthropologists are angelically honest and true. Most generally, of course, life in society involves normalized deceptions of all sorts; more particularly, individual sociologists and anthropologists do not act in accordance with
their disciplinary norms any more or less than anyone else does. But just as for anyone else, conventions are "real" insofar as they constrain the meanings attributed to individuals' actions. What I'm saying is that deception of informants is located or "situated" in sociological styles of research differently from how it is "situated" in anthropological research. What one field finds ethically compromised and intellectually unconvincing -- bad and not even wrong -- the other normalizes as good, and a legitimate condition for truth. These are the sorts of disciplinary differences I've found interesting to uncover, even while also appreciating the important overlaps and convergences that exist all over the place in our work.

Anthropology and History: An Inadvertent Border Crossing

The anthropology/history relationship is probably of greater interest for most anthropologists than that with sociology. Being historical is such a taken-for-granted "good" in anthropology that it might seem odd for me to look into disciplinary distinctions between the two fields. However, it's clear that there is much unexplored territory on that frontier. While sociologists and anthropologists authorize their texts by reference to different constructions of their relations with their interlocutors, historians and anthropologists also diverge in their manners of construing their sources. In both cases, the ethical presuppositions undergirding research practices in each field are distinctive and may produce some dissonance when historians and anthropologists read one another's work or otherwise engage one another.

I'll mention just one case. Consider the situation of those few anthropologists who have taken on the responsibility of writing up another anthropologist's fieldnotes. I'm not thinking of the more usual situation in which an ethnographer uses another's notes simply as a reference aiding their own field research with the same or related community. Rather, in the cases with which I'm concerned, a writer may have had extensive field experience somewhere nearby, but construed their task to be the writing of an ethnography -- that is, something like what the original note-taker had planned to write had he or she lived long enough.

Nancy McDowell's book, entitled The Mundugumor: From the Field Notes of Margaret Mead and Reo Fortune, is particularly interesting for the light it sheds on certain differences between historical and anthropological presuppositions concerning sources. The book is an ethnography of the Mundugumor, a Papua New Guinea people among whom Margaret Mead and her then husband, Reo Fortune, did about three months of research in 1932, and who figure in Mead's 1935 Sex and Temperament, as well as in a famous social structural analysis by Claude Lévi-Strauss. The Mundugumor are a people whose cultural norms, according to Mead, encouraged both men and women to act in ways that Americans might consider "male." Partly because of Lévi-Strauss's attention, Mead returned to her notes in the early 1970s intending to write a book on Mundugumor kinship. She asked McDowell, then embarking on fieldwork just upstream from the Mundugumor villages, to complete the project, should she fail to do so. Indeed, Mead died without having written the book. So, after doing her own fieldwork and enough writing to gain a tenured teaching position, McDowell made good on her promise to Mead, using Mead's fieldnotes and Mead's transcript of Fortune's otherwise indecipherable notes, most of which are archived at the Smithsonian.

With the exception of one chapter in which she offers a thoughtful reanalysis of Mundugumor kinship, McDowell attempted to write the sort of book Mead intended to write. That is, she wrote an ethnographic description of aspects of Mundugumor culture, not (for example) an analysis of Mead's fieldwork or critique of Mead's published ethnographic writings. True to Mead's explicit concern "to incorporate as much ethnographic detail as possible," the book included descriptions of the local environment, demography, religion, and social organization.

Only the introductory and concluding chapters considered the notes and their authors directly in a sustained manner. McDowell pointed out that, while Mead's observational skills were remarkable, at the same time, Mead's and Fortune's research suffered important limitations. McDowell also briefly discussed the interpretive challenges posed by using another's fieldnotes. Nevertheless, McDowell asserted that these materials make possible an ethnography of Mundugumor and insisted that we judge the book as such. After all, if it's true that we write our own ethnographies "from our field notes,"
we might reasonably expect to be able to do the same "from the field notes" of someone else.

The problem, as it turns out, is that anthropologists don't actually write ethnographies this way. On the one hand (as I've suggested), McDowell treated her own interpretations of Mead's notes and Mead's interpretive work in the field as if they might have the same, classically ethnographic object: Mundugumor culture. The difference she recognized between Mead and herself was one of degree, not kind: their relative distance from that object and, consequently, the potential reliability of their respective descriptions. Consistent with this approach, McDowell made Mead's notes her own by a process of re-reading that she described in her introduction. In subsequent chapters, then, she treated Mead's notes more or less as she and most other anthropologists conventionally treat their own fieldnotes. That is, in contrast to the manner in which historians reference archival sources, McDowell did not footnote or otherwise cite the notes themselves as texts to "document" the analysis -- not even when she overtly struggled with biases, ambiguities, and contradictions in these sources.

That McDowell is careful to quote from and to reference Mead's published work when she questions Mead's interpretations suggests that the "problem" here is not idiosyncratic (that is, an instance of poor scholarship, as it would likely appear to a reader with methodologically historical expectations). It's rather what I'd call disciplinary.

There are many issues here, but I'll mention just one. Understanding the differences between ethnographic and historiographic conventions of building or "supporting" their arguments and interpretations requires consideration of how anthropologists and historians each think about "primary sources" -- that is, the primary worldly constraints on an author's imagination (or, perhaps, the ways they each distinguish their own truthful constructions from those of novelists). It requires consideration of how those primary sources are "located" in the text to establish both its scholarly authority and proper grounds for skepticism, doubt, and critique.

There have been some extremely interesting discussions, recently, of the history and critical functions of footnoting and annotation in historiography, and quite a bit of attention to the problem of "ethnographic authority" in and around anthropology. These literatures need to be brought into dialogue with one another to develop the point; which is, most simply, that the common equation of ethnographic "fieldnotes" and "archival documents" is misplaced. Rather than being comparable, the two are apples and oranges in our respective practices.

Thinking about McDowell's book helped me to understand why reading fieldnotes second-hand does not itself give one a clear view of Mundugumor culture (the way we imagine that reading a good ethnography or our own fieldnotes might). McDowell herself pointed out the poverty of even the best set of notes, given the rich background knowledge anthropologists never write down. For example, Mead's notes contain very little indeed on the topics for which Mead's published account of Mundugumor in Sex and Temperament is famous: gender relations and ethos. In this context, McDowell drew on her own two-week visit to Mundugumor country and also occasionally succumbed to the temptation to extrapolate from her own extensive field experience in neighboring Bun, just upriver from the Mundugumor villages.

In effect, McDowell demonstrated that Mead (and, by extension, many or most other anthropologists) don't strictly write ethnographies "from fieldnotes." Anthropologists' ability to make other worlds "visible" in a conventionally convincing fashion has complex, largely unscribed sources effectively unavailable to secondary readers of their notes. Anthropologists treat fieldnotes as an adjunct to this process. As such, they construe their notes as private records or aides mémoires. But, even when anthropologists do refer to their fieldnotes explicitly or quote them directly in their ethnographies, they don't conventionally cite the notes in their ethnographies, nor do they generally archive the notes for public use (Mead was unusual in this respect).

Why don't more anthropologists make their notes public? I'll briefly note just one reason, setting aside the complex issues of memory and the systematic incompleteness of notes alluded to above. The anthropological ethic of informant confidentiality is the most usual reason cited for not archiving notes.
Anthropologists have argued over this principle, and it would appear that we are in something of a double bind here. Privately, anthropologists do share their notes with their students and with other scholars, who seek them out either (most commonly) as a resource in planning further fieldwork in an area, or else (more rarely) as a source in projects of intellectual or disciplinary history. Fieldnotes have also been made available institutionally (e.g. at the Smithsonian). But these efforts exist in tension with well-established ethical principles of the discipline concerning the privacy of informants.

For this reason, at least, it is not strictly accurate to equate historical archives with anthropological fieldnotes. As in the case of the sociology/anthropology relationship, more sensitive (that is, more thoroughly mutual) translations of our respective practices are necessary. Not recognizing that a translation is required here -- that historians and anthropologists aren't simply speaking the same language -- is a set-up for misunderstanding, especially in cross-disciplinary evaluations of research. With respect to history and anthropology it might be more meaningful to compare historical archives and "the field" itself: comprising all manner of ephemera, together with documentation of various sorts (including, perhaps, the fieldnotes). Resituated this way, fieldnotes begin to look rather more like the notes, photocopies, and marginal comments historians make when they are "in" the archives: things historians generally don’t "cite" either.

Memory is one of the things that make ethnography something more than a collation of fragments. Like ethnographies, histories are also very much more than collations of fragments, but historians conventionally produce this effect in another way. Historical footnotes -- a tracing of the researcher's engagement with archival sources -- are central to the authority of historical texts (to a degree that surprises many anthropologists). Key historical scandals, not to say the everyday worries when historians get manuscripts ready for publication, make this point very clear. Questioning the accuracy of an historian's footnotes is the equivalent of questioning an anthropologist about making up data, or actually being in the field at all.

Consequently, from an historian's perspective, while the apparent access of anthropologists to "lived experience" of their subjects is enviable, our lack of public documentation may understandably be viewed as unprofessional, even scandalous. Our conventional valuation and reliance on memory and eye-witnessing is equally incomprehensible. This is the context in which McDowell's treatment of archived fieldnotes as "fieldnotes" (and not "archives") might be looked at askance, if the looker were an historian. A more complete comparison between anthropology and history would develop a reciprocal point about the view back from anthropology to history.

The premise of my current research is that understanding disciplinary practices is important for those of us -- whether junior or senior scholars -- who cross the borders between disciplines. It is as important for those who actively seek to move or remove such borders as it is for anyone else since -- given the explosion of path-crossing these days -- every one of us is ever at risk of being (mis)construed as crossing some line even when we do not intend to (as my McDowell case suggests). Disciplinary distinctions are socially real and experienced, in part, as ethical affronts. Attention to the ethical ordering of disciplines may help trace out the complex interweaving of intellectual and moral justifications for our ways of meeting responsibilities to research subjects, and for our expectations about trust and truth-telling within specialist communities. Such attention may help also in the remaking and broadening of those communities.

Endnotes

(1) This is a revision of a talk originally presented at the 1998 American Anthropological Association annual meeting in a symposium entitled "Disciplinary Practice: The Work of Cultural Anthropology" organized by myself and Don Brenneis (UC-Santa Cruz). A long version, entitled "Ethical Ordering of Disciplinary Boundaries" was presented in 1999 at the University of Pennsylvania History and Sociology of Science Department seminar and another long version was presented in 2002 at the City University of New York Graduate Center Anthropology Friday colloquium. This version responds to questions posed to me by Critical Matrix: thanks here to David Ball and CM colleagues for their
editorial acumen and flexibility.

(2) Anthropology is peculiar in several ways that I don't have space to detail here. I'll just mention that we ourselves (and certainly also folks in other fields) often call anthropology "interdisciplinary." Anthropologists get borrowed from a lot by practitioners in other disciplines; we also borrow a lot. The field is itself an uneasy alliance among "subfields" that span the humanities-social science-natural science divisions: interpretive cultural anthropology on one hand and evolutionary paleoanthropology on the other. So one of the key challenges of the book on which I am working is to articulate what's "disciplinary" about anthropology. At the same time, it is absolutely true that anthropology is a friendly place -- both at the undergraduate and graduate levels -- for people who want a certain intellectual flexibility and playfulness while, at the same time, wanting an unambivalently grounded, out-in-the-world subject matter. Anthropologists have always studied people "everywhere." (That is, it is a myth that, in the past, anthropologists used to only work in places like highland Papua New Guinea. In fact, we were always in New York and Ibadan too.) While our topics have shifted and even expanded in recent years, there's a basic continuity in our discipline-wide, transnational commitment to face-to-face ethnographic research. Unlike what I take to be the current situation in literary studies, contemporary anthropology is not in inner turmoil about the loss of that basic substantive core.

(3) The phrase "moral order" invokes the work of Mary Douglas (e.g., Purity and Danger). Provisionally, "discipline" refers to an academically institutionalized knowledge practice involving credentialing within a more or less identifiable discursive "community." Disciplines aren't necessarily isomorphic with "departments." Proposing a definition is problematic, in the framework of my concerns, because my ethnographic object is, in part, the contesting of these communities' identities, boundaries, etc.

(4) The phrase, "refuguration of social thought" is drawn from Clifford Geertz's essay, "Blurred Genres," and also from Renato Rosaldo's book, Culture and Truth. Note here the relevance of Mary Douglas's observation that old forms of knowledge are dismissed not just as useless but as morally bad (see her essay, "Forgotten Knowledge" in Marilyn Strathern, ed. Shifting Contexts). I am arguing that the observation doesn't apply only to historical progressions of knowledges but to contemporary disciplinary distinctions as well.

(5) Sociologists (along with other social scientists) appear also to be less ambivalent than anthropologists about their status as "experts," and more ambitious than anthropologists are about having a policy impact. There are historically interesting reasons for this, given the canonical association of anthropology with European colonial empires and the discipline's ambivalence vis-à-vis colonial and missionary agendas. The effect of this was to center anthropological ethical allegiances with informants. The other side of this is anthropologists' classic "apprenticing" metaphor for relations with their interlocutors. The point is that there are many factors that situate anthropologists ambiguously vis-à-vis "expertise."

(6) There's a lot that ought to be said here. For example, accounts of anthropologists' efforts to explain their research may have a parodic character, in acknowledgment of how regularly or predictably our explanations are misunderstood! There is another point as well: anthropological research is notoriously unhyphoesis-driven. While anthropological research proposals surely have research questions, they have nothing like the "research design" normal in sociology. There is an informal value in coming back from fieldwork with something quite different from what ones research proposal outlined. The point is that anthropologists are more likely, for reasons having to do with disciplinary values and training, to follow the interests of their interlocutors than to have anything of much substance for their informants to second-guess.

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