Misunderstood, misrepresented, contested?
Anthropological knowledge production in question

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Abstract: This article draws out some of the implications of the fact that what anthropologists claim to know, or want to say, is unavoidably and in complicated ways bound by the ethics of involvement, detachment, and institutional location. I will first consider the increasingly common practice of circulating the output of anthropological research within the social context of its fieldwork, among the various research participants and interlocutors. Second, I will try to account for the sometimes negative reception of ethnographic accounts, especially where the research has focused on organizations (e.g., NGOs), activists, or others professionally concerned with public representations of their work. Third, I will reconsider the notion of "speaking truth to power" by pointing to the unacknowledged power of ethnographic description. Finally, I will suggest that ethical concerns are generated as much by the theoretical framing of research as by fieldwork practice, and that these are matters of choice rather than inherent in the ethnographic method.

Keywords: activists, ethics, ethnographic writing, fieldwork, knowledge, NGOs

Anthropological knowledge production of all kinds is embedded in sets of social relationships: among them relationships between fieldworkers and those they research, between professional colleagues and collaborators, between supervisors and research students. Anthropology's epistemology is itself relational—in the sense both that knowledge is collaborative, dialogical, and gained by way of relations, and that (in consequence) the relationships between researchers and their collaborators become a property of the object of inquiry itself; that is, "[t]he relation between the 'knower' and the 'object' of necessity bends back into the perception of the object itself and is cemented in writing" (Hastrup 2004: 456). Then the wider set of relationships involved means that such (anthropological) knowledge "is no simple 'object', because it bears all the marks of its institution, including a particular 'style of thought' that by itself becomes a standard of objectivity" (ibid.: 457). In this article I draw out some of the implications of the fact that what we claim to know, or want to say, is unavoidably and in complicated ways bound by the ethics of involvement, detachment, and institutional location.

My particular awareness of these issues came from the experience of negotiating the publication of an anthropological account of an international development project—a project in

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Adivasi ("tribal") western India funded by the British Department for International Development (DFID)—with which I had worked from its inception over 13 years as an anthropologist (1990–2003), alongside other professionals including plant geneticists, soil scientists, foresters, economists, and project managers and staff. Some of these colleagues and program managers raised objections and sought to disrupt the publication of an account of this project, while others (especially social science and gender specialists, community organizers, and fieldworkers) endorsed my analysis.

I have described this process and its implications elsewhere (Mosse 2005, 2006), so I will not offer details of the case here. Suffice it to say that I was in this instance an organizational insider subjecting the long-term shared experiences of aid project work to anthropological analysis, generating evidence from inferences out of these experiences and observations, inferences that my colleagues did not necessarily share. Included as objects of this analysis were the conceptual devices and institutional processes by which we developed certain policy frames and representations of events and our actions, often against the different logic that actually generated these events, so as to align theory to practice. I described how an extended "interpretive community" was constituted that requisitioned expertise to the task of stabilizing official policy models, and how project "failure" had more to do with the disarticulation between practices and their rationalizing models than the failure to turn designs into reality.

Ethnographic writing of necessity involves a kind of "exit" from particular social and professional relations in order to bring another perspective. One could say that in my case the exit was from an "interpretive community" that had developed around, and built consensus knowledge about, a "successful" development project, and that the disagreements within the community that followed weakened the "hardness" of project facts (see Rorty 1991). In any event, the aim of my objectors was to prevent this exit, to deny my ethnographic text (and its evidence) the reality it claimed. Significantly, this was not so much by refuting its facts as by attempting to re-enroll me into a self-constraining set of obligations of the project as a system of relationships and representations (see Mosse 2006 for a full explanation).

The process was fraught and involved the claim that the ethical guidelines of the Association of Social Anthropology (ASA) had been contravened because harm had been done through damage to the professional reputations of my colleagues as research subjects. Interpretive disagreements thereby gained ethical force. There were appeals to the authority of the publisher, the university, and the ASA, and an anticipation that pressure would be brought to bear on me as an academic researcher to rewrite my book. The ultimate failure of these efforts (the book was published largely unchanged and is now widely cited) was the outcome not of a battle over facts but of a metacontest over the terms of negotiation of my text: that is, over whether academic or professional rules would apply. In the event, academic rules applied, against which my objectors had little capacity to act. They wanted a kind of "court" ruling against a book and its author that would require certain changes; they got instead an academic seminar and an open exchange of views that did not bind anyone to particular actions.

In common with other such cases, it was never entirely clear what the objections were about. Certainly, they were not about factual details (being under this kind of scrutiny made me treat the evidence I had very carefully). Disagreement was rather over how knowledge and meaning was constituted; about what was, or was not, a valid interpretation; and how representations of a development project were to be authorized. The ethical issues arose from anthropological theorizing as much as ethnographic practice, which implies a broadening of the field of ethical responsibility from the conventional focus on the conditions of field research to the constitution of an intellectual project.

In the remainder of the article I want to address four issues. First, I will consider the increasingly common practice of circulating the outputs of anthropological research within the social context of its fieldwork, among the various research participants and interlocutors; and the point that not infrequently the response of those who read what is written about them is mixed or challenging. Reactions to ethnographic descriptions may also of course be methodologically significant, and reveal further aspects of the social or institutional worlds we want to understand, as well as clarifying the nature of our anthropological knowledge claims.

Second, I will try to account for the sometimes negative reception of ethnographic accounts, especially where the research has focused on organizations (e.g., nongovernmental organizations [NGOs]), activists, or others professionally concerned with public representations of their work. Third, I will briefly reconsider the idea that anthropologists can "speak truth to power", pointing out that ethnographic description can have unacknowledged and disturbing power, even though anthropological knowledge is contested and cannot but come into relation with that from other positions. Finally, I will suggest that the ethical concerns in research are generated as much by the theoretical framing of research as by fieldwork practice, and that these are matters of choice rather than inherent in the ethnographic method.

From field to desk to field

The now perhaps commonplace point that I made in an article a few years ago is that something important has changed in the anthropological method inaugurated by Bronislaw Malinowski such that the "field" and the "desk" are no longer apart from each other (Mosse 2006). Relationships that shape fieldwork continue to have a bearing on the way writing takes place and/or how it is received by collaborators. This is more and more evident in my work as a supervisor of PhDs, in particular those that focus research critically on aid, development, or expert communities, or by researchers drawn to the study of those they admire greatly (chosen as a focus of research for that reason), especially social activists struggling for justice, equality, democratic processes, or accountability of those in power. The talents and commitments of these researchers have drawn them deep into the worlds of their collaborating subjects (and at their invitation); but the depth of their knowledge of these worlds has later presented problems in *exiting* to ethnographic writing and publishing, despite in many cases having taken great pains to explain their purposes and gain consent to their researcher roles, and having periodically renegotiated this consent as events unfolded.

If returning the ethnographic writing to hosts, informants, and collaborators is increasingly unavoidable, it is not because this is a formal requirement of ethics committees, but rather because it has become part of the way in which ethnographic research is negotiated. The sponsors of research increasingly ask about collaboration, engagement, and impact. Fieldworkers may also be expected by their informants to promise feedback, local accountability, and sight of (if not review of) what they have written; or if this is not expected, such promises are made in the course of negotiating relationships with those who will be subject to description.

At the outset of field research, these can seem simple enough demands. Among the PhDs I have supervised in the past 10 years are those focused on revolutionary Maoists, Dalit and Adivasi activists, religious nationalists, NGO workers, environmentalists, and agriculture or forest bureaucrats, to name just a few. Of course, not all have evoked researcher sympathy or the desire for continued relationships, and some researchers have actively avoided postwriting contact with informants (some have even feared reprisals, for example, from militant religious nationalists). Others have simply not pursued postresearch encounters. Some have successfully negotiated the return of outputs to participants; and a few have remained "insiders" to the studied activism all along.

In cases that range from NGO activists and their networks, media, protests, and dramaturgical forms to Dalit and Adivasi activists and their cultural politics, researchers have chosen to return their ethnographic accounts to their collaborators (the thesis or chapters of it, pre- or postexamination). The reception of the work has ranged from the awkward to the outright hostile. In an unusual turn of events in one case, as PhD supervisor I even received emails from informants and research participants expressing concern over the thesis's "inaccurate and disrespectful analysis," asking about ethical codes on research subjects' consent to representations and the university's policy on the "respondent validation" of research, and insisting that informant responses be made available to examiners who were asked to appoint an activist to the examining committee ("to ensure objective assessment"). One respondent insisted that the student do the fieldwork again, another that my student not be awarded the PhD on account of the incorrect and disparaging (or, in terms familiar enough to me, "defamatory") reporting. The appeal to academic authority and emotion-laden reassertion of the social relations of the fieldwork find parallel in my own case of refusal of ethnographic exit.

The straining and breaking of relationships of fieldwork can equally be expressed in silence when activists distance themselves from the ethnography, or (politely) refuse discussion or dialogue ("that is your view … go!"), avoiding taking a view rather than mounting a challenge. Sometimes this is even harder to deal with.

However careful the negotiation of research, the returned ethnographic account reveals divergence in objectives and epistemologies, unstated expectations, and differing interpretations of the research project. What had been tacitly taken as shared objectives turn out to be far from this. The ambiguities of fieldwork roles—independent researcher versus advocate—return in difficult ways. In negotiating research access, anthropologist researchers may implicitly steer away from conflicting perspectives that later reappear; they may imply usefulness and solidarity that return as accusations of betrayal (I return to this below).

The process of sharing the thesis, or publication, is itself fraught, and the anthropologist can cause upset both by sharing too widely and not widely enough. Then, research participants may begin to comment on the reactions of other participants as well as on the work itself in often complex scenarios unleashed by the feedback. The thesis text is now itself a medium of positioning, relating, and articulating divisions or alliances, not only vis-à-vis the author. In certain contexts, an ethnographic description is contentious, initiating a process that changes relationships and maybe ruptures them.

Sources of contention?

There is much that could be said about what underlies the sometimes highly emotive reactions to anthropological writing. Some of these reactions concern in particular the relationship of professionals or activists to descriptions of their organizational work.

One problem is that social workers or activists find it hard to recognize their social goals in the ethnographic analysis—its framework of interpretation is off-beam—and they feel that (as one reacting to a thesis chapter put it) the researchers have "not understood our struggles, conflicts and dilemmas" in the way that was expected. The ethnographic account is not empathetic in the ways it should be. In a response sent to one of my students as well as to me as PhD supervisor, an NGO informant, complaining that a PhD draft had not been framed around the organization's view of its own purpose, goals, strategies, or outcomes, wrote:

> Imagine then a description of a football tournament—without any reference to the purpose—scoring goals, winning matches—or without any reference to the other teams—either the competing team in that match, or the other teams on the tournament—and without any reference to the audience—within the stadium or outside. The sole focus of description is

> how players jostle for their position in the team—whether they are played as centre-forwards, or half-backs, or full backs or the captain, or have to sit on the bench. Reference to goals, and the interaction with other teams occur—but only a backdrop to its implication on the positioning of different players.… In your version of football, there is no team at all, only jostling, competing individuals.

Activists (or those working within organizations) implicitly expect knowledge about them to be framed in terms of their prospective goals, the articulations of their campaigns and official documents. "Surely you must have read numerous documents, attended their numerous meetings, observed the ways in which the organisation of [the] campaign was being addressed," exclaims one exasperated activist to his ethnographer. They are likely disappointed, angered, or enraged by ethnographic descriptions of informal relationships, the jostling for position in agencies, the divisions and disputes that animate an organization (its hidden transcripts or informal goals and systems). Such things are not the point; they do not represent activists as agents in the way that they mostly imagine themselves to be. Offended subjects demand "proportion, perspective," and a recentering of the work in recognizable terms, rather than in terms of anthropological theory that is only "wise men from colonizing nations" displacing commitment to the project or campaign. And the challenge to the researcher is often expressed and experienced in highly personal terms: as betrayal, insult, disrespect.

There is a gap in expectation; or rather, the ethnography constitutes a "counternarrative" perceived as irrelevant, even if not subversive or dangerous to an agency's reputation. Ethnography also contravenes implicit rules of description, which might, for example, insist that actions are explained in terms of a hierarchy of goals and plans—outcomes in terms of intentions. Description should respect the distinction of domains of action and not, for example, muddy the account of official roles with kinship relations. The ethnographic description of a multiplicity of agents and intentions unravels the singularity of policy, and this then provokes demands from organizational actors that their agency be redescribed as the property of (collective) ideas, expertise, and policy (see Mosse 2006).

The problem here is that ethnography gives a solidity to things that ought to be ephemeral and transitory in a proper history, focused on the unfolding goals of the movement or project or whatever, and in which conflicts are resolved, problems solved (ibid.). At worst, unchannelled description opens up, or may reignite, problems that ought not to be dwelt upon: they might say "there were no differences", "no question of any conflict". Forgetting is a necessary part of organizational life that the ethnographic eye compromises. Besides, a history of cooperation, solidarity, and shared commitment provides the basis for future action. In my recent experience of development NGOs, splintered organizations or networks redescribe their environment so as to work together, avoiding examination of the reasons for division or failure.

Description can be dangerous, and because not framed in familiar terms, it has no use, it is a waste of time and resources—research interlocutors are disappointed, cynical, and dismissive of academic research. They do not find insight from an unfamiliar view of their world, but complain that there is little that could be learned from such writing. Moreover, ethnographic "evidence" (short interview excerpts, contextual descriptions of people and places, expressions or comportment) is seen as arbitrary, unweighed, untriangulated, "not factually substantiated", unscientific, or disrespectfully irrelevant (Mosse 2006, 2011).

A defamiliarizing description in which you do not recognize yourself quite as you expect might be intriguing, but can also be disorienting. Being objectified in unfamiliar terms is uncomfortable especially for those "ordinarily objectifying others" (as one PhD researcher noted of the response to research).

But perhaps there is another problem here, at least regarding certain kinds of professionals. It sometimes seems that the things that are of interest to anthropologists—the everyday, the contingent, the exceptional and the unintended, informal relational processes underlying official actions—may threaten the work of expertise, or of activism, or professional altruism. Elsewhere (Mosse 2011), I have argued that such narrations of events and specifics are difficult because they are awkwardly connected to discourses of failure—explained by contingent factors—and to the dividing dynamics of blame (Latour 1996). It almost seems as if the very analysis through which an anthropologist generates fresh knowledge and fulfills his or her professional identity is the same that might unravel the expertise and professionalism of others.

Finally, there is the matter of socially sanctioned knowledge. The anthropological thesis (or book) returned as an *individual* interpretive product, autonomous of its context, may be regarded as making universal claims and judgments in a way that contravenes the expectation that knowledge is socially produced and collectively sanctioned. As I put it in relation to my own objectors, their view was that "'fairness' in research is a question of respect (and unfairness disrespect) rather than verifiability. Research data and analysis are 'correct' (and mine was incorrect) in the normative sense of socially appropriate (as in 'correct behaviour') as well as factual" (Mosse 2006: 944). There is in some sense a rejection here of the notion of evidence as external to the situation.

But this, then, brings about a rather odd situation in terms of truth claims in disputes of the sort I was involved in. To put it simply, my analysis of the development project involved a constructivist insistence that "success" was manufactured through socially significant relationships, as was "failure"; and success and failure should be treated symmetrically (Mosse 2005). From the perspective of my objectors' positivist ontology, this was plain nonsense. But then their positivist ontology—the insistence on a singular truth of project action—was defended through a relational epistemology (that is, the necessity of shared accounts, interpretations authorized within a structure of relations), and my own relationalist ontology was itself defended by recourse to what was ultimately (at second order) a positivist theory of truth. After all, I insisted upon evidence independent of relationships and resisted assertions that truth was a matter of agreement and consensus (Mosse 2006: 954).

The power in ethnographic description

Those with whom I engaged over the publication of *Cultivating Development* may have been fairly powerful aid bureaucrats or international consultants, or large organization managers, but they had a rather fragile hold over their legitimizing representations and were vulnerable to description. Moreover, their capacity to exert social control over anthropological texts that affronted them was very limited. Their efforts to do so—to sustain an aid project as a system of representations—in fact only demonstrated the argument to which they objected (about the social protection of project representations). We might ponder at playing such a powerful hand (Richard Fardon, personal communication), especially where we are writing about agents with progressive agendas that we share, whether aid projects, NGO networks, or social movements struggling for justice on various fronts, whose strategic truths we might be a lot less comfortable disturbing because we believe in the cause, and like to construe ourselves as supporters and in solidarity.

If anthropologists hold ideas of "speaking truth to power", it is often without recognition of the institutionally backed power that academic writing carries and the powerful effects it can have. Anyone whose ethnographic description has been subject to contestation by participants in the research who feel injured is likely to be aware of the power wielded (perhaps inadvertently), and in consequence to have acquired a new ethical awareness in relation to the production of knowledge about others. But as on most things, anthropologists vary in their approaches. When she returned to Ballybran, the community in rural Ireland at the center of her award-winning and controversial *Saints, Scholars, and Schizophrenics* twenty years after its publication (1979) on a mission of reconciliation, Nancy Scheper Hughes was drummed out of the village; there would be no going back. "*Unrepentant* meets *unforgiving*" is how she summarized the re-encounter (2000: 137, emphasis in original).

Unrepentant, because Scheper Hughes's view was that as an anthropologist she could do nothing other than know about others through her own subjective categories of thought and feeling. For her, "[b]oth the danger *and the value* of anthropology lie in the clash and collision of cultures and interpretations as the anthropologist meets her subjects in a spirit of open engagement, frankness and receptivity" (ibid.: 127, emphasis in original). She nonetheless acknowledges that as an act of "translation" "ethnography has a predatory and writerly motive to it" (ibid.: 133). The question is, at what point does commitment to the integrity of an anthropological intellectual and communicative project become a questionable refusal to engage with the communities we research? With hindsight, Scheper Hughes admits that the easily decoded conventions of anonymity "makes rogues of us all—too free with our pens, with the government of our tongues and with our loose translations and interpretations of village life" (ibid.: 128).

The ethical quandaries of ethnography can appear as inherent to our core method and epistemology—unavoidable. But ethical dilemmas are also the product of choices about how to frame research, to interpret and to write; or they are the effect of the relatively unexamined changing demands of the discipline and the way in which researchers develop a project that will be judged by peers as intellectually significant or politically worthwhile. They are, in other terms, a product of the personal but also institutionally- produced predilections, judgements, and aesthetics that we can know of ourselves through what Bourdieu (2003) termed 'participant objectivation'.

The ethical issues faced by PhD researchers are then produced by the departments and supervisors with whom they work, the intellectual training and ethnographic exemplars that form their scholarly expectations, and all that goes into shaping current notions and standards of excellence and what count as anthropological knowledge.

Rendering ethnographic

Amanda Lashaw (2013) explains some of the influences, choices, and challenges as she reflects on her anthropological training and her own difficult transition from education reform worker (addressing class and race inequalities in Oakland, California) to ethnographer.

The first challenge is to find an intellectually worthwhile project. How, Lashaw asks, when researching activists or reformers with whom she shares values, political ideals, and whose networks she has been part of, does she fulfill the anthropological injunction to make the familiar ethnographically visible so that we do not simply reproduce "indigenous" terms of self-representation (cf. Miyazaki and Riles 2005)? What does it mean to cultivate a critical stance (or critical distance) from those familiar activists or NGO subjects who already hold as part of their own repertoire the tools we might use in finding our "terms of analysis", whether derived from ideas of governmentality, antipolitics, neocolonialism, or neoliberalism, and that now appear too simple and ill-fitting to the experience that is to be rendered ethnographic (Lashaw 2013: 516)?

Such tools of analysis are, moreover, honed from an intellectual "will-to-denunciation" (ibid.: 509) in relation to powerful elites and institutions such as the World Bank. They are not so useful in the study of those we have elected to study through affinity and who we come to through personal connections. As Latour (2004) points out, ethnographers are not well prepared intellectually to celebrate the difficult, perhaps fragile accomplishments of the institutions we study, or the unexpected combinations of radicalism, governmentalism, and philanthro-capitalism. When translating from activist to academic frames, we can easily get it wrong. The question, conversely, is how can we take optimism, zeal, and ethical commitment seriously without being bound to specific normative frameworks (Lashaw 2013)?

The second issue is that the struggle to define an intellectual project involves a set of purposes for researchers such as Lashaw that is quite distinct from those of former colleagues enrolled on her ethnographic project. This complicates the relationships with activist or other communities. There is a distance between her analytic position and the way her fieldwork companions view themselves *and* the way in which she herself is taken (by colleagues) to understand what is going on (because of the way in which she participates as coworker, co-opted producer of program outputs, and so forth). Where institutional, cultural, or other boundaries do not make the difference in purposes obvious, participant observation is to some degree an act of concealment. Commitment to an intellectual project moored elsewhere involves a being-there-and-not-being-there that is not apparent to those whose lives ethnographers share; it involves an issue of "doubleness" (if not feelings of duplicity) (Lashaw 2013).

Like Lashaw, I, too, had to make a transition from project participant to researcher. My ethnographic project was not conceived as such from the start, but was an unintended by-product of involvement as a development consultant over several years. But having negotiated a change in position from insider to analyst, I did not just return to the project with interview plans and a tape recorder; I also began to construe shared experiences over 10 years in a new light. The nature of this reframing was not necessarily apparent in my changed role.

The question, then, is what do ethnographers do with the fact of their different purposes, however they come to be framed? How explicit are fieldworkers able to be about their struggle for an intellectual project, or the transformation that has occurred between, for example, Lashaw's work as an education reformer and her return as ethnographer, or my own changed frame of reference? Is the insider-become-outsider masquerading as an insider? What is the relationship of an ethnographer to his/her own earlier insider experience? To what extent should ethnographers be explicit about their (developing) analytic perspectives, or the fact that what is recorded from intersubjective experience is destined to be recontextualized within a broad analytic schema for a different audience (cf. Descola 2005; Mosse 2006)? Would explanation of this be necessary to make consent to participate in the research properly *informed*?

Perhaps more commonly (as noted earlier), fieldworkers allow an ambiguity of perceived purpose so as to maintain relationships and forego critical engagement that might bring conflict or disruption. But every small effort to maintain relationships in the field in this way might store up potential rupture when it comes to writing.

Are these ethical dilemmas or analytic choices? We can perhaps think of two ends of a range of options. At one end there is a merging of the analytic perspective of ethnographer and his/her subjects of research through the adoption of collaborative or participatory modes of research and analysis, or a policy-oriented framework that foregoes the demands of critical distance; and at the other end is a maximal withdrawal into highly abstract analytic frameworks, for example, oriented in broad terms toward leftist political critical commitments and speaking to a largely academic audience. These are among the array of options that Lashaw describes in her effort to find her "footing as a scholar with political obligations" (2013: 506), and they are aligned to the distinction Michael Burawoy (2005) makes between a "policy sociology" aimed at addressing problems, and a "public sociology" having a critical contribution to expanding democratic dialogue. If the first forgoes the challenge of "rendering ethnographic", the latter may forego maintaining relationships of common purpose with at least some interlocutors (depending upon their location within particular configurations of power and allowing for the fact that, as in my case, an analysis may be rejected by some and lauded by others). The question is, are there ways of staying close to our research participants (when we want to) without sacrificing our intellectual projects?

Lashaw herself finds an answer, first by placing the ideals of her activist subjects within a "multiplicity of progressive opinions" rather than as the object of critical analysis; and second by giving centrality to morality and "morality-making in progressive practice" (2013: 518). This involves paying attention to the "energies and emotions," the felt optimism involved (the "affective practices") that offer a means to understand reformers and activists in terms other than those in which they present themselves but that do not appear to deplete or substitute their agency, but in fact regard "reformers as subjects of their own promises" (ibid.: 519). As she puts it, "[r]eformers give themselves—not merely time and labour, but neurons and muscles—to dramatize and defend the very possibility of 'interrupting social reproduction'" (ibid.).

Conclusions

At its narrowest, anthropological research ethics and "ethical guidelines" have been about issues such as informed consent in the practice of data gathering. Given the way in which ethnographic writing circulates, the discussion of consent has to be extended from research participation to the more difficult area of interpretation and representation. There is much still to learn about the effects of new social scientific circuits of knowledge: are they positive (learning) or negative (disabling)? How can anthropologists respond to demands for consent to research findings or after-the-event withdrawal of consent, claimed on the grounds of "harm" to research subjects (maybe through reputational harm)? How might this demand for *interpretive consensus* play out in different contexts? Anthropologists might at the same time reasonably worry about codes of research ethics (institutional review board [IRB] "briefings", defamation law) being used by powerful bodies to gain control over the outputs of research or to constrain academic freedom, or as in my own case, to resist academic boundary making that is the pretext for the production of ethnographic data. There are also questions of who owns the "data"—the protection of personal data (maybe its destruction) or its obligatory archiving.

These are, of course, important issues; but they tend to treat the ethnographic project as a constant, and not sufficiently to consider disciplinary theory and research expectations as ethically charged. I have suggested that an exploration of the ethical dilemmas of ethnography requires that we look at the whole set of relations involved in this kind of knowledge production, including those academic/institutional relationships that frame intellectual projects as well as those of fieldwork.

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