The perils of working at home:
IRB “mission creep” as context and content for an ethnography of disciplinary knowledges

ABSTRACT
Among kinds of fieldwork “at home,” ethnographies of higher education inevitably draw on informal gleanings of everyday insider experience. Such informality is implicitly outlawed by federal human-subjects research regulations, which presume a clinical biomedical model that formally demarcates research from other activities. Intricately implicated in these circumstances, this article describes a comparative investigation into the methodologically embedded ethical conventions of anthropology and related disciplines for which institutional review board (IRB) participation itself became inadvertently informative, work that also reveals a conflict between the ethics of human-subjects protections (confidentiality) and of collegial exchange (citation).

Over the past five or six years, a long-brewing controversy has once again come to a boil concerning the adequacy of the present framework for overseeing human-subjects research in the United States. Hypervigilance at the federal level has percolated through the network of local institutional review boards (IRBs, commonly referred to as “human-subjects committees”) that do the everyday work of reviewing research proposals and that have final say over questions about compliance with the federal regulatory code 45 CFR 46 (DHHS 2005, known since 1991 as the “Common Rule”). Anxious uncertainty impels IRBs across the country to apply the profoundly ambiguous federal regulations (which many board members have neither the time nor the inclination to study) to a widening array of heterogeneous research practices, whatever their funding sources. Critics have labeled this trend bureaucratic “mission creep” (e.g., Gunsalus et al. 2005). Insofar as its better-safe-than-sorry logic has caught oral historians and even journalists in its web, some observers wonder whether college novelists and poets in residence are next.

The Common Rule’s otherwise perplexing emphases and wording make sense only if viewed as residues of a generation-long history of response to biomedical research scandals (see introduction to this AE Forum). Its language presupposes biomedicine’s distinctive harms and benefits, typical research protocols, and background assumptions concerning the agents and objects of study. Nevertheless—and despite controversy from the outset—the regulations have been applied to a disparate array of behavioral and social-science research practices and, in recent years, to an assortment of humanistic ones as well.

Doing anthropology among the disciplines
The implications for ethnography are the focus of this article. Its illustrations derive from comparative research on disciplinary knowledge practices: Over the past decade, I have been studying disciplinary boundary-work within anthropology and among anthropology and some of its academic (and not...
so academic) neighbors, including social psychology, sociology, journalism, and historiography. This exploration has involved reading publicly available written sources, speaking with practitioners of various sorts at my own and other institutions, and being ethnographically attentive to the media of everyday academic life and to the typically transactional sociability of scholarly practice.

Treating normal circumstances as field experience works to relativize and remap one’s sense of similarities, differences, and relationships by shifting one’s point of view from the explicit to the implicit. When disciplines are understood in the normal way, in terms of their overt subject matters and characteristic products (e.g., following the logic of many colleges’ first- and second-year general education requirements), a certain complementarity is expected: History concerns other times, anthropology concerns other cultures, and so on. That same focus on subject matters and rhetorical styles has underwritten the widespread contemporary conviction (particularly in the humanities, and often citing Geertz 1980) that conventional disciplinary distinctions are “blurring.”

A contrastive perspective comes into view, however, when attention shifts from the substance and form of scholars’ products to how those products are made: to the relatively backgrounded, taken-for-granted practices of knowledge production in this or that field of study. Paying special attention to how research practices are evaluated—including how ethical judgments are deployed—is especially enlightening. That is, whereas research ethics are a legitimate and increasingly prominent focus of interest in their own right (e.g., Meskell and Pels 2005), they are also a productive means for understanding other things. Disputes over the criteria for “proper” and “improper” research practice within particular fields often turn, in part, on tacit assumptions about what makes a piece of work “anthropology” or “history” (or whatever) in the first place, such that measuring it by one standard rather than some other one makes sense. Epistemological assumptions implicit in intradisciplinary arguments over adequacy and ethics become more evident when they are situated in a cross-disciplinary landscape: juxtaposing controversies about acceptable and unacceptable uses of deception within social psychology and only apparently similar controversies within sociology; comparing sociology and anthropology with respect to the problematics of the researchers’ positionality (e.g., their relative emphasis on intimacy or distance); or considering contrastive stances concerning the value and ethics of naming names in anthropology, history, and journalism.

Although an ethics angle does not reveal a vista of untraversed borders between disciplinary cultures (on the contrary, intimate liaisons abound), it does reveal evidence of property lines in an only apparently unfenced intellectual neighborhood.

Ethnography as anomaly: An example and a point of departure

My work on disciplinarity began inadvertently as a byproduct of my collegial role as a good university citizen. An initial suspicion that ethical and methodological presuppositions structure disciplinary distinctions arose in the late 1980s when, as a more or less conventional Melanesianist working with notes and other materials derived from two periods of fieldwork in highland Papua New Guinea, I was also beginning a first four-year tour of duty on my institution’s human-subjects committee.

As IRBs go, Princeton’s may be characteristic of small universities without medical or public-health schools. Comprising mostly social and behavioral scientists (along with university counsel and health-services representatives and a contingent of community members), it was headed by a social psychologist when I was a member in the 1980s, as it has been consistently ever since. This makes a certain sense: IRBs are meant to have members competent to review the variety of research proposals they are likely to receive; most of the proposals Princeton’s IRB reviews are from the psychology department.3

My moment of enlightenment was a serendipitous function of everyday social engagements; that is, it arrived in classic participant-observation fashion even though (or one might just as well say, insofar as) at the time I was not engaged in “research,” as such, at all (see, e.g., Bradburd 1998).

As the novelty of monthly IRB meetings wore off and I became familiar with the panel’s routines, I came to feel increasingly like an outsider. The occasional anthropology proposal was always a problem. From the beginning, it was clear to me that something quite different from long-term, open-ended, participant-observation-based “fieldwork” was the unmarked term. After a while (and to his credit), our chair agreed that it might be a good idea for me to write the panel a memo summarizing my departmental colleagues’ evidently exotic research conventions. Although my advisory memo was a first, it was not to be the last: Despite my departmental colleagues’ subsequent restatements of the rationales for participant-observation, significant meanings were lost in translation, and the fragility of cross-disciplinary understanding was evident. In any case, in the 1980s, it was hard for me to shake the feeling that, in the eyes of my fellow IRB members involved in survey research or experimental social psychology, there was something shady, or disreputable, about the “ethnographer’s magic”: The radical inefficiency of anthropologists’ time frames? Our obstinate vagueness about research protocols and subject pools? Our inexplicable qualms concerning consent forms?

That is to say, as a minority IRB member, epistemologically speaking, I became uncomfortably aware of
the implicit entanglements of “is” and “ought” in the panel’s deliberations. Every meeting engendered a kind of double consciousness, presenting me with implicit comparisons between the ethically loaded suppositions of psychological or survey research and those of anthropology, whether or not there were ethnographic proposals on the table. It appeared to me that the core research methods of ethnography and of the other social sciences differed not only in matters of practical detail but also in the ethical structuring of their conventions of best practice.

But the subject was difficult to raise directly: The potential for cross-disciplinary conflict latent in the panel’s work was, instead, systematically defused by an explicit etiquette—characteristic of the university, generally—of disciplinary autonomy. Those of us on the panel would, from time to time, remind ourselves that “we’re not here to evaluate the research”; that is, technical evaluations of research design and significance were—within rather broad limits—understood to be the proper concern of disciplinary peers (e.g., departmental thesis advisors and external grant reviewers), not of the IRB. In this way, overt expressions of our respective disciplinary worldviews were muted. Refusing to micromanage disciplinary practices, the panel construed its mandate narrowly as overseeing compliance with the federal regulations, which members generally understood to concern ethical principles universally relevant to social research. These principles were itemized—quite succinctly (I have since learned) compared with the situation at other institutions—on the Princeton IRB’s “full-review” questionnaire (the form that researchers of all fields are directed to complete).

In fact, a potential for cross-disciplinary conflict existed in all directions. Not only were many of the other IRB members persistently flummoxed by my colleagues’ research proposals but I also found myself having a complementary reaction with respect to theirs. Moreover, it seemed to me that the full-review questionnaire took entirely for granted a set of assumptions that, although apparently transparent (commonsensical) to my fellow panellists, were rather opaque to me. I was particularly struck by one cluster of full-review questions—a frequent focus of discussion—that asked researchers to detail the contexts and procedures for debriefing subjects after the completion of research.

“Debriefing”? I was surprised and then a bit outraged (naively, I now realize) to learn that debriefing is a necessary corollary of the expectation, also built into the questionnaire, that the intentional “deception” of research subjects is a design feature of many research protocols in social psychology and sociology (e.g., Bosk 2001; Cassell 1980; Fine 1993; Harris 1988; Korn 1997), as it is in biomedical research using double-blind procedures and placebos.

Questions of control

A few years after I rotated off the IRB, my teaching and reviewing responsibilities drew me into worries about the difficulty that graduate students and younger colleagues seemed to be having writing grant proposals and explaining their work in postfield interdisciplinary contexts, particularly, with regard to justifying ethnographic research vis-à-vis perceived alternatives. Those concerns were an initial motive for the project concerning disciplinary boundary-work described above. Doing anthropology among the disciplines, and paying close attention to disputes concerning proper and improper work, I was initially curious about the unevenly porous boundary between historiography and ethnography. The importance of tacking between history and anthropology was a given by the 1990s among my fellow Pacific-area scholars and a key interest of our graduate students no matter where they worked.

In this fresh context, my inadvertent revelation on the IRB in the 1980s—that disciplines were, so to speak, “moral orders” (or epistemic “cultures”)—became relevant to the study. Consequently, my disciplinary ambit expanded to include sociology and social psychology, as I recalled my shock over deception and debriefing. This time, I scrutinized the federal regulations and vast commentary on them, something most active IRB members probably do not do. Intersecting IRB matters, I also surveyed the history of experimental social psychology, comparing some of its notable ethical controversies with those of ethnographic sociology and sociocultural anthropology.

Federal regulations take for granted that the condition for adequate behavioral and social research is investigator control of the research process by means of specialized, specifiable settings (like clinics or labs) or by means of specialized, specifiable procedures (a “research design”). Consequently, IRBs are set up to evaluate formal research protocols—documents that clearly demarcate research from non-research by identifying its locations, time frames, personnel, and procedures. Finally—bearing in mind that human-subjects regulations derive from guidelines originally designed for biomedicine, in which physical risk is often inherent in the research process—IRB decisions about particular proposals involve relative risk–benefit judgments.

Within this framework, it becomes clear how deception is not, by itself, a violation of human-subject interests. The social psychologists and survey researchers on my 1980s IRB panel were comfortable treating deception as a sometimes necessary, although risk-bearing, means to the benefit-bearing end of persuasive research because they understood deception as a sometimes unavoidable means for controlling key experimental variables. In social psychology, proper research depends on access to a “naive subject pool.”
that is, subjects whose lack of knowledge concerning particular research protocols ensures that their behavior can be treated as valid data. Since the 1960s, the term debriefing has been used to refer to explicit procedures for mitigating whatever distress deceptive research processes might cause while educating subjects about the rationale for the deception and enlisting their complicity (Harris 1988).

Ethnographic fieldwork does not fit this biomedical- and behavioral-science model of investigator-controlled research: Participant-observation—the research practice at ethnography’s core—is downright deviant on practically every count. As many sociocultural anthropologists and ethnographic sociologists practice it, the specific complementary value of participant-observation in relation to other research practices is its *systematic openness to contingency*, particularly, its interest in exploring unexpected entailments of informant-generated constraints.

Insofar as they do not organize their research primarily around formal interviewing or surveying, ethnographers find it more than just inconvenient to provide IRBs with a detailed, accurate research design: Developed protocols are antithetical to participant-observation and therefore deeply misleading. Instead, like many oral historians and journalists, ethnographers often expect specific questions to emerge in a relatively uncontrolled, unpredictable, and intentionally interactive discovery process. Structurally inverting the conventional human-subjects relationship, many anthropologists (especially these days) view their interlocutors as consultants or collaborators; they persist in using metaphors of incorporation or reception—like “apprenticing,” “learning,” or “listening”—to characterize their fieldworking relationships. These metaphors authorize ethnographic accounts by suggesting that—however conditionally and strategically—fieldwork involves a *disciplined relinquishment of control* over the sociotemporal contexts and conceptual frameworks of research by the researcher to his or her expert interlocutors (Agar 1980; Briggs 1986), be they Mexicali carvers, drug addicts and counselors at a Kentucky hospital, Papua New Guinea farmers, or academics at colleges and universities in the United States.

Psychologists and biomedical researchers control their spaces of research by demarcating them quite clearly. They narrowly delineate their roles vis-à-vis their subjects, sometimes introducing specifically defined, hypothesis-testing ambiguities by means of strategic deceptions. In contrast, cultural anthropologists and ethnographic sociologists tend to live, for a time, where they work; in these contexts, they are in a position neither to demarcate their research spaces nor to predetermine their own social identities. Embedding themselves in their informants’ environments, ethnographers cannot have settled identities as “researchers,” fixed and isolated once and for all from other social identities. Embeddedness and a certain lack of control over positionality may be more palpable for those ethnographers who work “at home” than for those who work in unfamiliar settings, but both situations challenge the fieldworker to maintain a tension between fluency and translation.

Formal research proposals obscure this fact. In honest efforts to comply with the demands of funders and regulators, ethnographers are constrained to make their open-ended process seem more managed and controlled than it is. The fine line between honesty and dishonesty on this point causes difficulties particularly for junior scholars, engendering a cynicism about research proposal writing and even misunderstandings about the realities of fieldwork.

Consider, for example, the argument Joanne Passaro makes in her chapter “You Can’t Take the Subway to the Field!” (1997). Contrasting research at home with the anthropological ideal of going elsewhere, she argues that research is more clearly grounded when (as in home-based work) the investigator is able to begin without first drawing up a formal research proposal. Proposals must specify all the whats, wheres, whens, and hows, but, she insists, one cannot know these things in advance of familiarizing oneself with real-world conditions.

For all its strengths, Passaro’s argument overstates the significance of research proposals—instruments designed for funders and regulators—as plans for actual fieldwork. Those of us who have done foreign fieldwork—and for that reason have had first to write funding proposals—know that, just like Passaro, we have also shifted our topics, our senses of relevant literatures and questions, even our research sites, populations, and time frames, in response to the contingencies and vagaries of field-based circumstances.

### Connections and disconnections among disciplinary cultures

So, scrutiny of the rationale informing IRB concern about deception and debriefing reveals an ethical-epistemological specificity beneath the veneer of universality: experimentalist assumptions about control that are conventional in some fields but not in others. Further scrutiny makes other disciplinary specificities evident. For example, IRBs typically demand that data be kept confidential: Research participants’ names are to be separated from personal information and removed from written reports. In the interest of confidentiality, researchers may also be asked to destroy their raw data after the analysis is completed. Anthropologists are generally comfortable with expectations about confidentiality; and although they are likely to balk at the idea of destroying their field notes, they do tend to keep the notes in their personal files rather than archiving them to give others access. In contrast, oral historians have been horrified by these expectations when, in the wake of the recent bureaucratic “mission creep,” they have come under IRB scrutiny. Primary research in oral history is all about creating a permanent, reconsultable archive of taped and transcribed...
words by named individuals whose stories are not yet part of the historical record (Shopes 2000). Similarly, with notable exceptions that underscore the rule, ethical practice in journalism—the very mechanism of a story’s credibility—also entails precisely identifying one’s sources.6

In fact, with respect to confidentiality, anthropological practice appears hybrid, in between that of journalists (or historians) and that of sociologists (or social psychologists). “Proper” research in anthropology is conventionally—or perhaps ideally—based on a distinctive construct of intersubjective encounter (rapport, or complicity) implied by the metaphors of incorporation and reception referred to above. The importance of demonstrating this sort of sociable engagement as a means of validating ethnographic knowledge is evident in numerous descriptive and prescriptive accounts of fieldwork. It is also evident in key disciplinary controversies, several of which have turned on instances of alleged failures to achieve, or betrayals of, that intersubjective, collaborative ideal (e.g., the Mead–Freeman or Chagnon–Tierney disputes). Consequently, although anthropologists are inclined to preserve confidentiality, their social positioning both during fieldwork and afterward has not infrequently encouraged them to respect their interlocutors’ requests that their names be used. In contrast, planted on firmer ground, the disciplinary identities of sociologists and social psychologists turn them more systematically into outsiders (and critics) vis-à-vis their informants’ understanding of the world. Rigorously preserving informant confidentiality allows these scholars a critical but still ethical voice.

With respect to IRB requirement of informed consent, it is illuminating to compare field practice in anthropology with experimental practice in social psychology. As noted in the previous section, good psychological research design may involve a degree of deception. Because deception as a research tool is procedurally enacted in and around the consent process (e.g., consent forms and other initial information provided to participants), it necessitates post-research debriefing, when research subjects are accurately informed about the study.7 In contrast, anthropologists generally construe consent to be negotiated throughout a long-term relationship, as a substantive part of the research itself. In this way, anthropologists part company not only with psychologists but also with investigative journalists and some ethnographic sociologists, who share a strategic, qualified acceptance of deception, particularly in critical research on social problems.

Anthropologists have long struggled with the disconnection between IRB protocols of informed consent and anthropological field ethics. Clearly, demanding prior written consent makes sense in a Western biomedical lab, when the research procedure itself may cause physical harm. In contrast, those of us who have worked in the rural Third World (or in marginalized communities close to home) know that asking folks to sign a form may imply collusion with unwelcome authorities, from whom researchers may need to distance themselves (Lederman 2004, 2006). Informed consent in fieldwork—which, unlike research in an investigator-controlled environment (like a biomedical or psychology lab), takes place in one’s informants’ own environs—“is an ongoing interaction . . . subject to the cultural rules and understanding of that community” (AAUP 2002).8

One size fits all?

In summary, scrutiny of ethical and other evaluative discourses reveals a complex configuration of similarities and differences among related disciplines. Nevertheless, the federal human-subjects protection regulations treat all research practices alike, as if distinctive epistemic cultures did not exist. Despite ample evidence that disciplinary ways of knowing have distinctive ethical constitutions, structural factors still incline the federal system, through local IRBs, toward applying one homogeneous ethical standard, based on one concept of “best practice”: a highly idealized model of the “scientific method” abstracted from clinical biomedicine and experimental behavioral research.

The official Institutional Review Board Guidebook (Penslar with Porter 1993) makes this bias clear but in an uncritical fashion that obscures important facts.9 For example, the guidebook is written without a clear warning about the accidental character of the federal code. That code was created in 1974 when the then Department of Health, Education, and Welfare (now DHHS) upgraded a set of preexisting NIH guidelines to regulatory status but without making good on promises to adapt those biomedical guidelines for social research systematically (Tropp 1982:391–392; see also Pattullo 1982, 1984). Instead, the regulations were revised piecemeal over a 30-year period in response to diverse pressures.

The guidebook is also written as if it makes unproblematic sense to implicate judgments about research design in judgments about ethics in the work of multidisciplinary panels. This stance is a misfit for IRBs in academic communities (like Princeton’s) that are inclined to allow departments considerable autonomy (trusting and deferring to local disciplinary expertise). But the same stance neatly fits IRBs in communities with medical schools or otherwise involved in biomedical research (Stark n.d.). Those IRBs may follow the guidebook’s advice religiously: scrutinizing research designs in detail on the basis of lengthy, elaborate full-review questionnaires; interviewing principal investigators; and even editing protocols as part of their ethics reviews. When they do, the review process may take months without commensurate benefits.

Whether followed to the letter or more loosely, the guidebook’s interpretation of the regulations is troubling not because it recognizes that methods and ethics are
inextricable but because it encourages IRBs to adopt a single, narrow model of adequate research design. In a nutshell: “Clearly, if [the proposed research] is not good science, it is not ethical” (Penslar with Porter 1993:ch. 4, p. 1).

The guidebook’s concept of “good science” is reductively biomedical: “The broad objective of the behavioral and social sciences is similar to that of the biomedical sciences: to establish a body of demonstrable, replicable facts and theory that contributes to knowledge and to the amelioration of human problems” (Penslar with Porter 1993:ch. 5, p. 1). This interpretation mirrors the regulatory definition of research (which is, however, more vaguely worded): “Research means a systematic investigation, including research development, testing and evaluation, designed to develop or contribute to generalizable knowledge” (DHHS 2005:102[2]). If one is not aware of the biomedical context implied when these words were written (a context that the guidebook’s wording clarifies), then one has scant basis for excluding the activities of literary fiction writers on college payrolls, together with their autobiographically inclined creative-writing students, from IRB oversight.

The regulatory definition of human subject is consistent with this sense of its origins: “Human subject means a living individual about whom an investigator (whether professional or student) conducting research obtains (1) data through intervention or interaction with the individual, or (2) identifiable private information” (DHHS 2005:102[f]). As the regulations clarify, “intervention” denotes “physical procedures”—the example given is “venipuncture”—and “manipulations of the subject’s environment.” This definition is clearly designed for experimental biomedicine. It makes sense, then, that the only example offered of “private information” is “a medical record.” In contrast, when the regulations go on to elaborate that “interaction” denotes “communication or interpersonal contact between investigator and subject,” readers are left to grope about in the dark. If the biomedical context of this otherwise utterly global definition is not kept in view, then it is hard to think of anything (asking your mom for her basic muffin recipe?) that might fall outside IRB oversight.

This implicitly clinical notion of “human subject” is therefore distinct from both the survey researcher’s “respondent” and the ethnographer’s “informant” (Cassell 1980). But, although noting that differences exist among biomedical, behavioral, and social research practices, the guidebook brushes them aside because “there is considerable overlap among the three areas” (Penslar with Porter 1993:ch. 5, p. 1).

The guidebook’s representation of qualitative social research was woefully inadequate in the pre—“mission-creep” 1980s when it was first drafted. Despite revisions, it is even more so nowadays, when IRB oversight is expanding deeply into the humanities. Its experimentalist reductionism is poisonous for ethnography. Intentional and unintentional evasion and active cynicism about IRBs are predictable responses when regulations appear to make no sense because they persistently refuse to take heterogeneity seriously in the ethical structuring of disciplinary practices.

Reprise: Colleagues as informants and vice versa

Given the recursive loopiness of my project’s engagement with these issues—an ethnography of disciplinary knowledge practices that had gradually come to recognize ethical discourses and controversies (including those relating to IRB practice and policy) as both field contexts and subject matter—I should have anticipated the Alice-in-Wonderland turn of events that transpired in August 2004 when my department chair passed along to me a request from our IRB that an anthropologist join the panel. One of us had to serve: “Who better than you?” she asked brightly.

I could not refuse. But the request forced me to face a dilemma that I had allowed myself to avoid and that this article and the others published in this AE Forum approach from various angles.

Working at home, informally (without funding-proposal rubrics), is a commonplace among ethnographic sociologists. For their part, sociocultural anthropologists have scrutinized the benefits and challenges of fieldwork in familiar places methodologically, politically, and in other ways, always in counterpoint with the discipline’s paradigm of leaving home (e.g., Jackson 1987; Messerschmidt 1981). Anthropologists, however, have not considered in a focused manner the specific dynamics of informality in field research. In formal fieldwork, the purposefully uncontrolled conditions of research associated with participant-observation, in particular—conditions that enable its distinctive access to lived realities—are qualified by demarcations, however nominal, necessitated by funding proposals. Insofar as it is not initiated in this way, informal fieldwork dispenses with even those distinctions. But the absence of such investigator-defined initial conditions with respect to places, time frames, and other conditions of research does not mean that informal research is unconstrained. On the contrary, it is structured by the researcher’s charge to remain systematically and critically attentive to the social conventions of the field communities in which she or he is progressively embedded.

Those social conventions may conflict with background assumptions guiding IRB deliberations. In the case of my informal fieldwork on academic cultures, there is a conflict between IRB assumptions about the ethics of research on “human subjects” and the social conventions of a field community in which the researcher and her interlocutors are “colleagues” (see also Bradburd this issue). Collegial relationships are conditioned by a problematical entwining of gift-inflected expectations about the mutual benefits of an open exchange of ideas and market-implicated rationales concerning intellectual capital, credit, and citation. Colleagues may be unequal, and their relationships fraught, but
their differences are commensurable and ideally worked out without third-party mediation (e.g., McSherry 2003, which makes these points through the analysis of a worst-case scenario, in which intellectual property laws were invoked).

Can a colleague be a human subject (and vice versa)? Can citation be squared with confidentiality? Human-subjects regulations construe the researcher and the researched as having radically different kinds of agency. They are construed not simply as unequal but as incommensurable: always potentially opposed and most certainly in need of third-party mediation. Can this logic of protection be reconciled with that of collegial exchange (whether in a gift or market or hybrid transactional mode)?

So long as my research remained informal and, in particular, undeclared—"subterranean," as Jack Katz (this issue) puts it—I had not had to face these questions. In my accustomed contexts—talking with historians or sociologists or journalists—working out how to be simultaneously a normal colleague and a proper fieldworker had been relatively easy, the distinctive challenge in developing fieldworking relationships amidst ordinary collegial exchanges being that I had learned to "profess" less than I might have otherwise, in favor of listening much more patiently to my interlocutors.

But if I rejoined the IRB, I would face a double bind: a social-relational conflict at the limit of what I considered ethically acceptable. Up to that point, I had not considered studying actual IRB meetings (as others were; see Stark n.d.). Instead, memories of my 1980s IRB experience were a suggestive rationale for developing other sources: publications, online materials, and fieldwork-style attentiveness to those many moments when IRB matters came up in encounters with friends, colleagues, and students. If, however, I were to again be an IRB member, there was no way I could imagine participating in IRB meetings without both injecting some of what I was learning about the national regulatory situation and being attentive—in the usual ethnographically global fashion—to the panel's practice. That is, whatever else it was, whether I liked it or not, my impending IRB service would necessarily also have to be "fieldwork."

In this new context, working out how to be simultaneously a normal colleague—doing my share of the panel's work—and a proper fieldworker—communicating my interests and then developing productive relationships with my interlocutors through mutual accommodations—promised to be a major challenge. Fieldwork risked being misconstrued in terms of the research models more familiar in IRB deliberations: The apparentordinariness of my presence would make it seem that I was not doing "research" at all (when I was).

There was also a more basic (although not necessarily more obvious) problem. Because I had understood my work on disciplinarity to consist in reading and consulting colleagues about their experiences (a style of work that Kristina Gunsalus [2004] has recently labeled "two people talking"), I had never applied for special research funding nor had I ever submitted a description of my research for IRB evaluation. It simply had not occurred to me to think of my colleagues as human subjects in need of special, federally mandated protections because, as suggested above, collegiality has its own distinctive ethics of transactional sociality and means of redress. In any case, I had begun these activities before the recent anxious expansion of regulatory controls made submitting a project like mine to an IRB—even for an "expedited" review or to certify its "exempt" status—something other than parodic. Although this project may be an extreme case, it nevertheless shares with more typical research in sociocultural anthropology and sociology the quality of being a disciplined species of everyday behavior, risking a wide range of possible everyday "harm(s)" (from boredom and annoyance to accusations of libel or intellectual property theft) and promising an array of familiar "benefits" (from friendships to liberatory insights and social criticism). The rationale for special control, including prior review, of this behavior is murky.

Precipitously faced with the prospect of monthly IRB meetings, however—in which I expected that the regulatory human subject would be the only category of person acknowledged in discussions of social research—I suspected that members of our IRB would not see things my way. So I met with the acting chair—a member filling in while the chair was on leave—to explain my pickle, half hoping he would tell me that I should stay off the panel.

He did not. Instead, he told me that the IRB needed an anthropologist to assist in reviewing ethnographic research protocols; and he was intrigued by my project, saying, "We could use your expertise!" What is more, he told me that, coincidentally, he had recently had a very interesting conversation with a sociologist from the University of California, Los Angeles, who was researching IRBs, apparently just as I was. That is, he treated me as a colleague—as someone with whom one might exchange information. Which is to say, he did not think to assume the subject position—as someone in need of special protection.

But this is not the only way things could have gone. IRB researchers have told stories of collegial interlocutors refusing communication on grounds that proper IRB approvals and consent forms were not to hand. For example, in 2005, Gunsalus (a lawyer at the University of Illinois Office of the University Counsel and professor at the College of Law) organized a multidisciplinary conference on the troubling trends in IRB regulation that I have noted in this article. Calling various national disciplinary societies to ascertain whether they were, for example, "tracking problems . . . members are encountering with IRBs," she was taken aback when one official replied (without irony), "I'm happy to answer your questions, but before I do so, as chair of a social humanist and behavior IRB, I must ask: Does Professor Gunsalus have IRB approval for her study????" (Gunsalus et al. 2005:23).
A quarter-century earlier, during another period of regulatory intensification, E. L. Pattullo had had an inverted version of this encounter that “set me wondering about how differently we perceive identical activities depending on whether or not they are categorized as research” (1982:377). He described a phone call he received from a Department of Health, Education, and Welfare staff member charged with advising that department about the 1979 Belmont Report proposals for revising human-subjects regulations. After responding to a long list of questions and being asked to answer a further series of questions in letter form, Pattullo “sat down, quill poised” when “it suddenly struck me: ‘This guy is doing research, and I’m his subject!’” (1982:377). The letter he drafted was a play of ironical juxtapositions that sharply underscored the regulations’ inconsistencies.

In happy contrast with this persistent and pervasive craziness, my colleague-informants on the IRB have been open to exploring the boundary between normal collegiality and ethnographic relationships, in search of a cache of partially overlapping interests.10

Conclusion

Everyone knows from experience that multiple social identities—parent, employer, friend, and coworker—with different and conflicting ethics and politics, intersect one another in everyday life. Situating themselves in the everyday both as persons and as researchers, ethnographers expect to learn how their interlocutors negotiate the tangles of social life by observing, listening, and getting partially caught up with them. Expectations of these sorts—the very framework that gives good work in ethnography its complementary value relative to other kinds of social research—simply cannot be translated in terms designed for experimental biomedical and behavioral science, in which good work depends on creating a demarcated, controlled space of research.

The focus in this article on informal (unfunded) fieldwork at home exaggerates the qualities that make ethnographic research in anthropology and sociology anomalous in an IRB context, underscoring a point that is also true of mainstream anthropological fieldwork conducted far from home: Genuine IRB compliance is impossible for ethnography (as for certain kinds of critical social research) so long as the regulations recategorize all research participants as human subjects necessarily in need of special protection. Ethnographic research is radically misrepresented if the only allowable ethical discourse implies objectification and predictability, one-way control over the research setting, and a research process that aims to design and thereby to predetermine the character of the researcher–researched encounter so as to tease single strands out of the tangled social fabric. Effective ethical scrutiny of social research is necessary. But agreements about by whom and how are not possible without a framework that clearly acknowledges the actual variety of distinctive, ethically ordered methodologies among the disciplines and that strengthens protections for varieties of critical social research as it zeros in on the significant contexts and sources of risk for research participants.11

Notes

Acknowledgments. This article was originally presented as a paper at the 2005 American Ethnological Society annual meeting in San Diego, in a panel that I organized entitled “Anxious Borders between Work and Life in a Time of Bureaucratic Ethics Oversight.” Thanks to all participants, to Virginia Dominguez for encouraging submission of the panel papers to American Ethnologist, and to Dan Jorgensen for alerting me to Canadian research ethics issues and sources.


3. I should emphasize that—like everyone else currently seeking to situate his or her own local IRB experience relative to IRBs elsewhere—I have scant comparative data of an ethnographically adequate nature to rely on (although see Brenneis 2005; Stark n.d.). What we have is a growing accumulation of provocative anecdotes. Amid this general uncertainty, one thing is clear: IRB experience varies widely. Despite appearances, the system is decentralized, a condition that—together with inconsistencies and vagueness in the regulations—guarantees diverging improvisations. Insofar as institutions with different histories, structures, and resources have distinctive improvisational styles, IRB practices may reflect the institutional culture of which they are a part. This is how I understand Princeton’s distinctive IRB process.

4. The specifically biomedical character of regulatory risk–benefit assessments is reflected in provisions that limit these calculi to research, “as distinguished from risks and benefits of therapies” subjects might receive, and that exclude from consideration the “long range effects of applying knowledge” (DHHS 2005:111[a][2]). That is, “research” in this case is that which is not “therapy” (the application of medical research results). Similarly, risk–benefit assessments are enjoined for the research, specifically not for the long-term therapies. Medical research volunteers (like cancer patients participating in clinical trials of new cancer therapies) can compare the likelihood of a direct personal benefit from new knowledge against the risk of harm in the research process itself. In contrast, social research rarely promises such direct payoffs to its participants. Its benefits tend to be indirect for participants, and both its benefits and harms are located less in the research process than in research products, such as the consequences of publication (Hamburger 2005; Tropp 1982).

5. I recognize the irony of these conventional metaphors. However much they may relinquish control during fieldwork, ethnographers mostly accept authorial responsibility for their public analyses and interpretations (talks and publications). Nevertheless, anthropology has a long, multivalent history of collaborative projects (including films and books) with informants; its still-popular metaphors of incorporation and reception are objects of productive, critical reflection that pays special attention to political and ethical dilemmas (Brettell 1993).
6. High-profile cases make the point dramatically (Tavris 2002), but everyday ones also do. Consider an editor’s note that appeared in the *New York Times* (2004), reporting corrections to a *New York Times* magazine exposé of sexual slavery trafficking. The note’s final paragraph admitted—and apologized for—altering the magazine’s cover photo, which had shown a 19-year-old who had escaped from her trafficker four years earlier: “An insignia on her school uniform had been retouched out of the picture to shield her whereabouts” (*New York Times* 2004: 3). This is an admission for which few ethnographers would dream of apologizing (confidentiality needing no special justification in their work).

7. Because so many research subjects are undergraduate students in introductory psychology courses—for whom a few hours as a research participant may be a course requirement—debriefing is also construed as a pedagogic tool and is a means of enlisting their complicity in preserving the deception for the next round of subjects.

8. For example, one of my undergraduate advisees planned to do summer fieldwork back home in Montana. The motivation for her research on high school rodeo competitors and their families was to demonstrate how rodeo is a central cultural institution in the western United States, despite being largely ignored by anthropologists. She needed IRB approval but was skeptical about how to introduce consent forms into her conversations with rodeo families. She was concerned that doing so would undermine her complexly balanced credibility and trustworthiness as a member of a rodeo family herself, who—having played basketball in high school and then gone off to a fancy East Coast school—was, with this work, returning home (Overstreet 2006).

9. The 1993 version of the *Institutional Review Board Guidebook* is accessible on the website of the Office of Human Research Protections (DHHS; http://www.hhs.gov/ohrp)—the office that anchors the national IRB system. A first edition of the guidebook was produced in the early 1980s at the prompting of a presidential commission, in consultation with officers and advisors at the Office for the Protection from Research Risks. For an analysis of the (very similar) 2001 version of this source, see Brenneis 2005.

10. With a qualification or two. After the IRB chair returned from her leave, and after I introduced myself and my special circumstances again—once again not wanting my inadvertent but unavoidable fieldwork to be overlooked, as it was wont to be in this context—I requested an exemption or an expedited (“minimal risk”) review for the IRB component of my disciplines project. Because research involving consultations with past and, especially, present Princeton IRB members was just unusual (and sensitive) enough that the chair felt she needed a proper, full-scale review, I was asked to make a formal application for full board approval, which was granted. The process was informative in ways I do not have room to discuss here.

11. After submitting this article to *AE*, I became aware of the 2004 Canadian PRE report, “Giving Voice to the Spectrum” (see this *AE* Forum’s introduction). Its guidelines and the emphases of this article (together with those of this forum, generally) neatly dovetail. It deserves a close reading and stands as a very useful resource for our ongoing negotiations, locally and nationally, over appropriate frameworks for ethically attentive research practice.

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