To all who contributed, many thanks for commentaries that complement our articles in important ways. They expand and deepen this *AE Forum* by providing global, institutional, and cross-disciplinary vantage points on research ethics regulation and fieldwork practice and by offering criticisms and ideas toward improving the conditions for ethical ethnographic work. Insofar as the commentaries engage not only the articles but also (inadvertently) one another, I aim in this rejoinder to tease out a couple of the connecting threads.\(^1\)

As a set, the forum articles do two things. First, they go some distance toward articulating the distinctive features of participant-observation fieldwork as it is practiced: By considering field practice in what may usefully be construed as a limiting case—informal (unfunded) research at home, where it has no predetermined temporal and practical demarcations—certain mostly tacit features that are also present in conventional fieldwork become explicit and available for scrutiny. Commentators variously pick this up to offer fresh examples (e.g., Didier Fassin), to sharpen and nuance our representation of field practice (e.g., Don Brenneis and Marilyn Strathern), and to worry it (e.g., Gustavo Lins Ribeiro and Nandini Sundar).

Second, the articles analyze the conjuncture of this style of research and the existing U.S. system for regulating research ethics and begin discussion of the system’s limitations and of actions taken to address them. Commentators variously provide non-U.S. comparisons and contrasts (Fassin, Ribeiro, Strathern, and Sundar) and reflect on the requirements and conditions for ethical ethnographic practice (George J. Annas, Brenneis, Stuart Plattner, Ribeiro, and Deborah Winslow).

Many of the comments (e.g., by Annas, Fassin, Plattner, Ribeiro, Strathern, and Sundar) complement the articles’ emphasis on studying peers and elites in the United States with a wider range of conventional and activist projects in which the power relations between researcher and researched place researchers in exploitative positions. The articles’ emphasis on the tendency of local institutional review boards (IRBs) to overregulate is
balanced by comments by Plattner and Sundar, for example, who remind readers that researchers face pressures that may encourage them to minimize the possible risks to informants posed by their work.

Certainly, our point in developing a critique of the U.S. federal system for overseeing human-subjects research was not to assert that ethnography is necessarily harmless and ethically unproblematic and, therefore, that it should be entirely unconstrained. Arguments about ethical responsibility in fieldwork are well developed elsewhere: ground well plowed and freshly on ethnographers’ minds in the wake of the multinational controversy over Yanomami research. Although Plattner usefully notes that, in his many years as a National Science Foundation (NSF) insider, not one case of harm caused by anthropological research came to his attention, at the same time, a hefty literature of case studies, hypothetical and real, describes dilemmas that ethnographers face in fieldwork: evidence that anthropologists have a history of taking their responsibilities seriously. Brenneis’s point that ethnographic projects have “life histories,” with ethical entailments that far transcend fieldwork itself, exemplifies this stance (see also Brenneis 2005).

Rather, the point of this critique is to make clear that the existing U.S. system is ineffective when it comes to meaningful support of specifically ethnographic “best practices.” Not only do IRBs, as presently constituted, not help to cultivate an “ethical climate of research” for ethnography but they also actively undermine such a climate by fostering cynicism, dishonesty, and confusion. Frequently frustrating, occasionally outrageous bureaucratic tangles associated with IRBs focus attention on models of research design and consent documentation that are construed by many nonethnographers as epistemologically neutral but that can be shown to reflect particular, mutually inconsistent ways of understanding the world (each with its own “best practice”): a politics of knowledge production. These tangles drain energies better spent designing ethics education relevant to field research or alternative structures for reviewing field projects.

There is no shortage of ideas about what ethnographers might more productively be thinking about. Sundar and several other commentators suggest the need for critical discussion of what informed and consent might mean in fieldwork conducted in particular political contexts; Ribeiro questions why professional ethics codes are not put to better practical use as appropriate frameworks for evaluating research plans; and Annas boldly proposes that the American Anthropological Association should formally request the central Office of Human Research Protections to exempt ethnographic projects altogether, a move that would force the invention of an alternative to IRBs more appropriate to this style of research.

Meanwhile, Winslow, Plattner, and Sundar are all correct that both novice and experienced fieldworkers could benefit from ethics education; they believe that IRB reviews can play a role toward that end. Plattner and Winslow also argue that ethnographers need to get involved in their local IRBs to help design procedures appropriate for reviewing ethnographic projects. I agree completely that, as long as anthropological and sociological fieldwork falls under IRB control, ethnographers need to become actively involved (Lederman 2006). But for this involvement to be effective, researchers must become much more specific and honest about what ethnographic fieldwork is really like. As cultural anthropology program director at NSF, Winslow is in a position to help articulate these realities and the rest of us need to find ways to support that effort.

Comments by Annas, Brenneis, and several others reinforce the central argument of the articles that, as it is practiced, participant-observation fieldwork poses a distinctive challenge for bureaucratized, consistency-seeking regimes of regulation and oversight based on a biomedical model of research. Our focus on informality in fieldwork zeros in on the systematic characteristic of participant-observation practice that is most anomalous in this context: the foregrounding of formal protocols, most obvious in unfunded fieldwork but true in funded ethnographic work as well.

Ribeiro, Sundar, and Plattner, however, object to the representations of fieldwork offered in our articles. Ribeiro and Sundar are concerned that to emphasize informality is to deny that ethnography has a proper method. Sundar is perhaps concerned about how to advocate effectively for anthropology in meetings with social-science colleagues in economics or political science (a concern all of us who sit on IRBs likely share).2 Contesting our claims about informality, Ribeiro asserts that ethnography is a “special method” involving a “different kind” of social immersion with its own frame of mind, attention, self-presentation, and so on. But different how and compared with what? I could not agree more with Ribeiro that ethnographers need a fresh round of discussions of methodology—of field practice. But I believe that that will have to look something like what we have attempted here and will mean putting the “difference” of ethnography into words as well as developing comparisons with specific alternative knowledge practices (one needs to know one’s disciplinary Others).

Social scientists have adequate language for conveying the value of interviewing, surveying, and similar investigator-controlled approaches to understanding the world—approaches that make sense against the background assumptions of the hypothesis-testing research model on which IRB deliberations are based. Plattner expresses these assumptions quite clearly in his commentary.3 As typical IRBs tend to do, Plattner subsumes ethnographic fieldwork within the general category “social science.” Echoing the Common Rule definition of “research,” he asserts that social scientists’ goal is to “document reality to explain it” and to “explain general processes.” These assertions do not capture the projects of those many anthropologists (and some
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sociologists) who understand what they are doing in terms of cases and interpretations, although it suggests why those folks have trouble making themselves understood by their IRBs.

In contrast, the language for conveying the logic and value of participant-observation to IRBs is still inadequate. Ethnographers need to make clear that the unprescribable procedures of participant observers—their disciplined openness, specifically, to informant-generated constraints on the conditions of interaction—are not by the by but necessary for access to the tacit social meanings and practices of everyday life. These are the constraints within which ethical best practice in fieldwork needs to be thought and enacted, and that fieldworkers need to communicate in IRB and grant proposals. These articles, and several of the commentaries, work to develop a language for talking about this specificity and its ethical demands.

For example, Strathern moves the discussion closer to a necessary specificity with her deceptively obvious argument that ethnographic fieldwork is the exploration of relationships by means of relationships and that this entails “negotiation” with interlocutors: a social contamination of sorts. This move—with its clear ethical charge (to shake hands, yes!)—is an important corrective to the socially abstracted language of “research” (or “investigator”) ethics that ethnographers are encouraged to use in the present regulatory system. The present framework construes investigators as the key social agents and construes the context of their agency in their terms alone: as socially purified, demarcated “research.” This framework accurately captures the goal of most social science, in which considerable effort is devoted to limiting or eliminating the relational quality of interactions among investigators and research subjects. It is, however, a complete and total misfit for participant-observation, which has the opposite goal of embedding investigators in everyday relationships, in all their unruliness. Quotidian social relations involve multiple agents with divergent interests, intentions, and assumptions about the relevant contexts of their relationship (which, they may agree, are partly “research” but, simultaneously, many other things that are not, as Brenneis reminds readers, neatly containable within the regulatory frame). Those multiple agents have correspondingly divergent ideas about the right, the good, and the proper: about social harms and benefits, about fairness and justice, about the proper enactment of respect for persons, and so on. This last point has serious implications for Plattner’s misguided attempt to manage or adjudicate a distinction between “journalism” and “ethnography.” Presuming a model of purified research, Plattner takes for granted that, whereas journalists’ audience is society at large, ethnographers’ audience is strictly “professional.” Presuming a notion of socially embedded (in that sense, contaminated) field research, other commentators disagree. For example, as an activist fieldworker, Fassin positions himself as an “observant participant” (one suspects that Plattner would relabel him a journalist), and Sundar argues that ethics review boards ought to mandate the setting aside of funds for translating ethnographic work into the national and local languages of the folks being studied. In fact, ethnographic sociology and investigative journalism have more than a century of common history in muckraking investigative research that aims to influence social policy. And, as several contributors illustrate, whether they work at home or away, anthropologists have also engaged in applied and activist fieldwork on behalf of their informants. Anthropology has a long-standing (if uneven) interest in public presence and impact, often enacted in the process of research itself, not simply in the dissemination of its findings.

I appreciate Plattner’s and Winslow’s point that the Common Rule is not itself the key problem. Winslow emphasizes that the problem is how local boards implement the laws. She cites Shweder’s account of local action as exemplary and directs readers to the NSF website’s very helpful advice to investigators that they can use in negotiations with their local boards, and for which they may thank Plattner. She also cites the Illinois White Paper: together with the conference papers on which it is based (several of which are still available online if one follows the White Paper links), this document might be used as a resource for strengthening ethics review boards in the process of their findings.

Although they may not be the whole problem, the regulations are a headache because their biomedical assumptions continually undermine efforts at the local level to educate boards about ethnography. In effect, if one does not have what Plattner refers to as a “reasonable” IRB and a department member on the case, year after year, to ensure that the understanding keeps getting reasserted, it all too easily slips away. So long as participant-observation remains an anomaly, it needs actively to be defended.

Which is to say, the ethical is political. IRB ethics trouble has spawned an explicitly politicized discourse on research “best practices” across the disciplines within universities as well as nationally, between universities and the federal Office of Human Research Protections. Comparing the lack of research oversight in France with the severe constraints on research in South Africa, Fassin cautions that national cultures of research ethics are different and that, in some places, hypervigilance is less the result of biomedical tragedies and more a function of nationalist assertion of control over research. As he puts it, “ethics is also a matter of politics”—in this case on an international scale. Describing an activist project in the suburbs of Paris, he illustrates how the ethical is political on a personal scale as well. Considering Annas’s
“benefit sharing,” Strathern’s “negotiation,” Brenneis’s “partial measures” and other formulations that capture the unavoidably multisided relationality of ethnographic practice, this would seem a fruitful proposition on which to base a contrapuntal reading of this forum.

Notes

1. And thanks, again, to AE editor Virginia Dominguez for selecting such a thoughtfully varied set of commentators and for the charming and conjuring skills that helped convince all of them to respond. More generally, she has reworked American Ethnologist as a medium for advancing conversations about matters ethnographic. Amid the decentralized proliferation of practitioners and media—which, for all its benefits of access, also means that discussion and debate are increasingly scattered—the opening of disciplinary “central places” (like American Ethnologist) to productive argument provides an important counterpoint.

2. Sundar’s otherwise interesting comments are marred by an important misunderstanding of the tone of our arguments. She reads our discussion of ethics as “angst-ridden” and of IRBs as “resentful and threatened,” whereas the point was to take productive analytical and practical action. For example, she understands me as expressing “indignation about being forced” to engage in cross-disciplinary translations. On the contrary, the cross-disciplinary miscommunication to which my early IRB experience alerted me has been a source of insight in my exploration of disciplinarity. Voluntary immersion in cross-disciplinary language learning for more than a decade—too long, no doubt—has sharpened my understanding of features of anthropological research most often taken for granted and informs my representation of participant-observation in my article and in this response.

3. His representation of the constitutional and social mandates of journalism and scholarship reveals an imprecise, misleadingly simple understanding of rights in a free society, but that is another story (in addition to Katz’s and Shweder’s articles in this issue, see Hamburger 2005).

References cited

Brenneis, Donald

Hamburger, Philip

Lederman, Rena