Comparative “Research”: A Modest Proposal concerning the Object of Ethics Regulation

Complementing the broader project of treating human subjects research regulation (including “informed consent”) as an ethnographic object, this article scrutinizes the category research: that about which research subjects may (in some way) be informed, to which they may (or may not) consent, and in which they may (variously) participate. What is “research”? When does it begin and end? What is the relationship between its demarcations as a regulatory object and its demarcations in the everyday practices of knowledge production? Federal research ethics regulations take for granted that research can be distinguished from nonresearch and subjected to distinctive constraints. The regulations also presume an idealized scientific method with predetermined spaces, times, personnel, and procedures. Although such clarity is difficult for many kinds of human subjects research, it is impossible for ethnographic fieldwork. A modest proposal is offered concerning with whom ethnographers might make common cause.

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Research means a systematic investigation, including research development, testing and evaluation, designed to develop or contribute to generalizable knowledge. [DHHS 2005: 45 CFR 46.102(d)]

Anthropology, or anyway social or cultural anthropology, is in fact rather more something one picks up as one goes along year after year trying to figure out what it is and how to practice it than something one has instilled in one through “a systematic method to obtain obedience” or formalized “train[ing] by instruction and control.” [Geertz 1995:97, quoting Foucault]

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The fieldworker must choose, shape, prune, discard this and collect finer detail on that, much as a novelist works who finds some minor...
character is threatening to swallow the major theme, or that the hero is fast talking himself out of his depth. But unlike the novelist . . . the fieldworker is wholly and helplessly dependent on what happens . . . One must be continually prepared for anything, everything—and perhaps most devastating—for nothing. [Mead 1977:300]

So the point of my keeping a notebook has never been, nor is it now, to have an accurate factual record of what I have been doing or thinking . . . Instead I tell what some would call lies. “That’s simply not true,” the members of my family frequently tell me when they come up against my memory of a shared event. “The party was not for you, the spider was not a black widow, it wasn’t that way at all.” Very likely they are right, for not only have I always had trouble distinguishing between what happened and what merely might have happened, but I remain unconvinced that the distinction, for my purposes, matters. [Didion 1968:134]

Ethical practice—in research or any other domain—has an improvisational subtlety that exceeds what can be managed by bureaucratized processes. Over the past decade, research ethics in the United States and elsewhere have increasingly become subject to this kind of oversight; in the United States, the federal Office of Human Research Protections (OHRP) coordinates thousands of local institutional review boards (IRBs) that are mandated by federal regulations (45 CFR 46, cited in the first epigraph above) to evaluate research proposals at colleges, universities, medical centers, and other research institutions. As the regulatory ambit of IRBs has expanded in recent years, managerial rationales have threatened to overwhelm subtler (but more meaningful and effective) impulses in a number of fields. For ethnographic fieldwork in particular, ironically, regulatory imperatives have themselves engendered ethical quagmires. Sociocultural anthropologists are responding to this challenge by moving regulatory effects from background—as a begrudged context for their work—to foreground: as a provocative subject for field research and ethnographic interpretation (Lederman et al. 2006).

Adopting a classically anthropological strategy, this article uses relativizing comparison to map the terrain of the regulatory object: “research.” Beginning this inquiry “in the beginning,” it next explores the poor fit between demarcations of “research” as a regulatory object and its different, ambiguous, or absent demarcations in the knowledge-making practices in various academic disciplines. It considers in detail the extremely poor fit, suggested in the first pair of epigraphs, between the regulatory definition of research and anthropological evocations of fieldwork. The second pairing—mirrored valuations of “happenings”—hints at a final comparison at the outmost border of our map, whose practical implications are suggested by way of conclusion.
In the Beginning: What Research Is and Isn’t

When I teach ethnographic field methods, a core course in my department’s undergraduate program, the focus is participant-observation: sociocultural anthropology’s paradigmatic approach. Most years, when the term begins, I ask students what they figure “research” refers to and how they learned to do it. My aim is consciousness-raising. Over the years I have become convinced that students’ ability to learn how anthropologists use participant-observation is impeded by presuppositions about research that they have absorbed in school, from media sources, and from everyday life.

A key problem is that, while it is taken for granted in sociocultural anthropology as the basic medium of fieldwork, participant-observation is in fact anomalous among social and behavioral research methods. To novices and nonpractitioners, participant-observation appears as a “method of no method.” That wayward opacity is charmingly captured in Geertz’s representation of anthropology (this article’s second epigraph) as “something one picks up as one goes along.” In contrast, the apparent precision and familiarity of the regulatory definition of research (the first epigraph) is much closer to what my students expect when they walk into class in September with thoughts of questionnaires and interviews dancing in their heads. This stark opposition will be complicated below, but for now let it serve to fix the idea that an absence of method—in the specific sense of defined-in-advance, replicable, hypothesis-testing protocols—is a problem not just in ethically managing ethnographic research but also in teaching it.

When I ask students about their experience with research, they recall secondary school social studies and science classes in which they learned about the necessity of referring to “sources.” If one were assigned to find out about Elizabeth Cady Stanton’s lifework, or the habitat requirements of freshwater mollusks, one was expected to locate appropriate sources and to report what one learned from them in appropriate ways.

But what is a source? And how to report?

My students’ stories—heavy with episodes of misadventure (memory is cruel)—suggest that the answers were not so obvious. Moreover, learning to do research meant learning how not to do it; grasping what research is involved building up complex indexical understandings of its alternatives. Certainly, there were alternatives of an internal, more-or-less sort: maneuvers within the boundaries of research practice that separated excellence from mere adequacy. But there were also alternatives of a border-breaching, qualitative nature. Students remember fearful encounters upon straying from the zones where research (of any quality) is done into a phantom zone of actionable transgressions.

Interestingly, these same misadventures taught students that fields of study (like history and biology) privilege different sources and use the same sources in different
ways. In other words, the same activity may be “research” here but not there, and the propriety of a source is relative. For example, it is “research” to consult one’s older sister as a source when investigating one’s family tree for a biology unit on Mendelian inheritance. But, no matter how accurate her knowledge, it is not “research” to consult that same older sister when writing a history paper on women’s suffrage: instead that is called “naïveté” (if one cites her) and “laziness” or even “cheating” (if one solicits her help but does not mention it). Similarly, it may not be considered “cheating” to consult one’s imagination as a source for that biology paper on mollusk habitats, but it most definitely will not count as “research.” In provocative counterpoint, whereas both one’s imagination and one’s sister’s experiences are completely proper sources for a short story writing assignment, that work does not usually count as “research” either (a point to which we will eventually return).

Students’ stories suggest that schooling establishes, as academic second nature, that kinds of research practice—ways of producing worldly knowledge—are distinguished by their ethical signatures; by which their products also become identifiable as of particular kinds—history, biology, and so on—capable of being compared and evaluated relative to others of the same kind.

I had an unexpected intimation of just how early the moral ordering of distinctive ways of knowing is inculcated, several years ago, during a “Back to School” night at my daughter’s elementary school. Early October. A dozen parents were sitting at their fourth graders’ desks, being briefed by the teacher about what to expect that year. Public speaking would be on the agenda: each student would make two presentations every month—a news report and a poem. We were encouraged to ask questions.

One parent raised her hand with a problem. She had gotten into an argument with her son over his presentation that week. He had informed her that his teacher had said “plagiarism” is “illegal.” He had been praised for “giving the news report in his own words” earlier in the month, and now he was insisting that he had to do the same with that week’s poem! She wanted to know how she should explain to her nine-year-old why it is that, to stay on the right side of the law, newspaper articles are indeed paraphrased but poems must be recited word for word.4

To a jaded adult, poetry and journalism are so clearly incomparable that it is a shock to be confronted with a vantage from which the two are not obviously distinct. Although articulating such distinctions is rarely necessary, most adults would have little trouble explaining that poems and news articles are assembled from different sources and crafted with different intellectual tools. Most of us are also aware that communities of producers and consumers evaluate these cultural products by different criteria. We do not generally look to today’s lead story for winking allusions to other texts (although that is possible); nor do we expect that a poem inform us about current events (although that is also possible). However, we are apparently less likely to remark on the dissonant tonalities of
right and wrong through which we may have come to distinguish them in the first place. The moral inflections of our knowledges are learned early enough and with sufficient ambiguity and danger that they are most often tucked out of sight.

**Many Kinds of (Proper) “Research”**

To become research adepts, academics endure demanding initiations into disciplined knowledge production. During their careers, they learn, resist, and transform some number of these categorical distinctions: if not poetry and journalism, then anthropology, sociology, and the like. Their transformative agency often involves energetic, cross-border scavenging for facts, topics, tropes, and theories. Despite all this border traffic, however, practitioners are rarely conversant with one another’s traditions of research practice—particularly, with the historically elaborated, value-laden conventions for recognizing and using “sources.”

In this regard, cross-disciplinary naiveté and what might be called disciplinary cultural chauvinism are widespread. These leanings are important obstacles to meaningful interdisciplinarity; they are also obstacles to cross-disciplinary communication in practical contexts, like IRBs.

This problem is a motivation for research that I have been pursuing concerning disciplinary boundary-work within anthropology and between that discipline and some of its neighbors in the humanities and social sciences. Taking a relativizing comparative approach to the cultures of disciplinary practice has meant abandoning scholarly business-as-usual. Instead of following my intellectual nose across disciplinary borders in the usual manner, I have been doing something more like fieldwork “among the disciplines”: conventional ethnography with an unconventional object. In contrast with the opportunistic permeability of disciplinary borders that scholars experience when we follow our noses, this relativizing approach accesses the hidden boundary trip wires and camouflaged fences that crosscut academic fields of collaboration.

It is especially enlightening to pay attention to what counts as “real” or “proper” research within this or that field—positions that are articulated sharply in intradisciplinary ethical controversies. Although a historical sociology of science clarifies the “moral character” of truth claims generally (e.g., Shapin 1994), the specificity of epistemological assumptions implicated in intradisciplinary ethics arguments becomes evident when disciplinary cultures are juxtaposed. There is a lot to be learned, for example, by comparing controversies about acceptable and unacceptable uses of deception within social psychology (where it has become a conventionalized technique) and only apparently similar controversies within sociology (where deception is a controversial strategy with a long history); by comparing sociology and sociocultural anthropology with respect to the problematics of the fieldworker’s social positioning (e.g., the two fields’ relative emphases on distance and intimacy as warrants for their observations); or by considering the contrasting
stances concerning the value and ethics of naming names in anthropology or sociology (on one hand) and history or journalism (on the other).

Appreciating complex relativities like these in what counts as legitimate scholarly practice in different disciplinary communities is crucial for understanding the limitations of bureaucratized research ethics regulation and management, which require one consistent model of research and a unitary ethics.

Consider the regulatory idea of informed consent, for example. Ensuring that research participants give their informed consent is one of the central mandated concerns of IRBs. What does informed consent look like in light of the divergent conventions of information exchange between investigators and investigated in, say, experimental psychology and sociocultural anthropology?

Good research design in social psychology may involve deception, which is viewed as a sometimes necessary, although admittedly risk-bearing, means for controlling experimental variables (Morawski 1988). In psychology, persuasive research depends on access to a “naive subject pool,” that is, investigators need subjects whose lack of knowledge concerning research protocols ensures that their behavior can be treated as valid data. Ironically, deception as an experimental tool is procedurally enacted in and around the “informed consent” process itself. To be ethical, then, deceptive techniques necessitate postresearch debriefing: explicit procedures for revealing the ruse and mitigating whatever distress it may have caused while educating subjects about its rationale and enlisting their complicity.

In contrast, anthropologists construe information to be exchanged and consent negotiated in relationships they develop gradually with their interlocutors, as a substantive part of the research itself. With regard to deception particularly, anthropologists part company not only with psychologists but also with investigative journalists and some ethnographic sociologists, both of whom accept the need for dissembling in special circumstances. However, unlike psychologists, journalists and sociologists do not justify the practice as an experimental necessity but rather as an enabling condition for critical research on social problems.

Implicit in these partially convergent, partially divergent proprieties of information exchange and consent are discrepant assumptions about control in the research relationship. Federal regulations assume that good behavioral and social research demands investigator control of the research process. IRBs are set up to evaluate formal research protocols: documents that aim to demarcate research from nonresearch by identifying its locations, time frames, personnel, and procedures. Psychological experimentation neatly fits this model: demanding prior written consent is feasible because experimental research occurs in specifiable settings by means of precisely predetermined procedures (a research “design”).

Ethnographic fieldwork does not fit this model. 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, ethnographers find it more than just inconvenient to provide IRBs with detailed, accurate protocols: a research design is antithetical to participant-observation. Like oral historians, ethnographers expect substantive foci and specific questions to emerge in a relatively uncontrolled, unpredictable, and intentionally interactive discovery process. Anthropologists nowadays often view their interlocutors as consultants or collaborators; they persist in using metaphors of reception—like “apprenticing”—that have long represented fieldwork relationships.

Consequently, anthropologists have struggled with the disconnection between IRB informed consent protocols and ethnographic field ethics. As other articles in this issue illustrate, asking people to sign a form may imply collusion with unwelcome authorities, from whom researchers may need to distance themselves. Informative communication and consensual relationships in fieldwork take place not in investigator-controlled environments (like clinics or labs) but on the informants’ home turf. Fundamentally, fieldwork is itself the process through which the cultural understanding necessary for meaningful communication (concerning “consent,” among other things) is acquired and applied.

Among mere mortals, knowing the world means making choices. Disciplines (such as anthropology or psychology) respectively privilege particular worldly constraints on the imagination, defined against both earlier versions of themselves and one another. This is to say, there is no least-common-denominator Research: no suite of ideals or traits shared by the practices to which the term customarily refers. Instead, there are diverse, historically specific, socially organized ways of knowing the world, in shifting partial relations with one another as well as ambivalent relations with particular non- and quasi-research activities like medical therapy and literary journaling, as we will see below.

This may all seem obvious. The problem is that bureaucratic regulation cannot tolerate such incoherence. overseeing human subjects research means bracketing practical variation to create a stable object with clear boundaries. Criteria must be found for deciding what is and is not research and when instances of research begin and end. However, as the next parts of this article show, research involving human beings persistently resists reductive objectification in the interest of consistency. What is more, this is true not only among the ornery humanistic social studies but also in biomedicine and other unambivalently science-identified fields whose research models fit the regulatory definition perfectly.

Original Sin: Research Is Not-Therapy

How did research become viewed as an object dangerous enough to require an elaborate structure of regulatory controls? Answering this question means recognizing that the regulatory definition of research was designed originally to provide a means of eradicating a potentially lethal ambiguity in medical practice concerning the
boundary between research and therapy. This danger, publicized in the 1960s and early 1970s, is central to the history of the U.S. federal regulations (see Lederman et al. 2006). But the design was flawed; the original ambiguity is still with us.

U.S. human subjects research regulations (known since 1991 as the "Common Rule" but formally set in place in the early 1970s) derive from earlier National Institutes of Health guidelines based on specifically biomedical experience and ethical problematics. Their logic goes something like this: First, medical therapy is appropriately evaluated in terms of individual patient interests, because its central concern is the direct improvement of individual patient well-being. Second, medical research is appropriately evaluated in terms of society’s and science’s interests, because its central concern is the production of knowledge “generalizable” beyond individual cases. And third, although physical risks to persons are inherent in both medical research and therapy, the risks to individuals are qualitatively greater in research (where individual persons are not the central concern) than in therapy (where they are). Consequently, research needs special oversight.

These assumptions are clear in a close reading of the regulatory code: for example, in provisions that, first, limit risk/benefit calculi to research “as distinguished from risks and benefits of therapies” and that, second, exclude from consideration the “long range effects of applying knowledge” (45 CFR 46.111[a][2]). The subtext, if you will, of the Common Rule definition (our first epigraph) is that research is not therapy: not the application of medical knowledge to individual persons (normally enabled when research results are published and made available to practicing physicians).

Now, although the regulations were applied widely to federally financed non-biomedical research from the start, the idea of research-as-not-therapy does not translate readily outside of biomedicine. For example, 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 practical payoffs to its participants. Its benefits tend to be indirect and its harms are located less in the research process itself—IRBs’ mandated concern—than in its products (e.g., publications, which are subject to a very different form of oversight).

Not only is research-as-not-therapy untranslatable across fields, but the regulatory definition did little to resolve the very ambiguities within medical practice for which it was designed. Heroic efforts of clarification can be found in works that interpret the Common Rule for IRBs (e.g., Penslar, with Porter 1993). Nevertheless, to this day it continues to be a frequent topic of debate in IRB circles.

Research Is Not-Process-Improvement

IRB Forum Listserv discussion (http://irbforum.org) makes clear the regulations’ impotence in demarcating research clearly from therapeutic process. The Forum’s
regular participants include IRB administrators and chairs; its mostly biomedical discussions are prompted both by subtle dilemmas raised by IRB habitués and by basic questions from people who have recently shouldered IRB duties.

This past year alone saw several extended discussion threads wrestling with the distinction between research and therapy (e.g., IRB Forum 2006a, 2006b). In these discussions, sorting research from therapy and the improvement or assessment of patient care is acknowledged to be positively nightmarish.

A glimpse of the struggle: consider that physicians may innovate when faced with difficult cases, using drugs “off label” and trying other unconventional approaches in hopes of improving a particular patient’s condition. Viewed as a normal part of therapeutic practice, in hospitals and other institutional settings these innovations may be monitored by “Process Improvement” committees unrelated to IRBs.

If an innovation looks exciting, then a physician may suspect that it is worth writing up and publishing. This is where the categories slide into one another. Medical case reports, when submitted for publication, fall in an ambiguous zone. As one IRB Forum participant put it (2006a), the only reason to submit such a report is that there is a “lesson to be learned.” In other words, he explained, one publishes the case because it might contribute to “generalizable knowledge” and this appears to place it within the regulatory definition of research. Nevertheless, the idea to publish may have occurred only post hoc, in the wake of experience with a particular patient—the only risks posed being those involved in the clinical care of that person. This repositions the activity as “therapy” rather than “research.” Then again, in many such instances, a transition commences before therapy is completed, in which additional “clinically unnecessary” tests may be administered to make the case more publishable. Just when does therapeutic innovation become “research” and therefore require prior review?

In summary, he warned that when a medical practitioner tries out a novel surgical or drug-related approach in a unique patient situation, it is not clear when the approach ought to be “formally validated” by being submitted to an IRB before proceeding, rather than continuing on as “unsupervised research, medical malpractice, moral bankruptcy, aggravated battery or just orderly progress.” The writer’s absurdist tone suggests the hopelessness of pinning down these gelatinous distinctions.

Nevertheless, the struggle continues. In these discussions, as noted above, publication is often proposed as retrospective evidence of research intent, because a common meaning of “research” is work that produces knowledge worthy of or intended for dissemination. However, various discussants (IRB Forum 2006a, 2006b) cautioned that particularistic case histories (i.e., nonresearch) may also be published. They noted that if the point is curbing unethical practice rather than regulatory nitpicking, then although experimenting with no intent to publish may not be “research,” it may be unethical and dangerous to patients nonetheless.
Discussions like these demonstrate that regulatory contexts in no way suppress practitioners’ thinking about the complex realities of practice and the unruliness of everyday experience. At the same time, ironically, this grounded ethical thoughtfulness is one of the forces propelling local IRBs to regulate research beyond Common Rule guidelines and is therefore itself a source of the system’s overall arbitrariness.

Significantly (for the purposes of the present argument), among indices of “research” that might be observable before the fact, the regulatory definition of research as a “systematic investigation” is widely accepted as offering “clues that might affirm research intent.” As this writer suggested, “if an investigator designed a study with some novel, highly systematic approach (i.e., a cohesive theory or literature driven design; control or comparison groups; random sampling; etc.) and called it ‘working a hunch’ rather than calling it research, I would have to take issue . . .” (2006a).

This returns us to our starting point: if one is seeking a common element in the jumbled assortment of activities that happen presently to be labeled “research” for regulatory purposes—including participant-observation, oral history interviewing, and such—then systematicity is certainly as useless an index as any of the others. There does not appear to be a consistent rationale for connecting the regulatory category to cases.

Desperately Seeking Consistency

Such discussions suggest how local IRBs come to apply the existing regulations in expansive and mutually discrepant ways. Insiders at the national level have long acknowledged this as a problem; unfortunately, their solutions are limited by a bureaucratized “audit culture” (Strathern 2000). This was painfully evident during deliberations of a presidential body called the National Bioethics Advisory Commission (NBAC), which held hearings for six years during the mid/late 1990s to review and rethink human subjects research oversight, concluding its work in 2001 by issuing two volumes of analysis and recommendations (NBAC 2001). A certain conservatism was evident during the hearings, where honest arguments about the specificity of research ethics in different fields were mostly heard by regulators as self-serving evasions of responsibility.

The following analysis of the hearings suggests that so long as ethical accountability is only imaginable in the form of managerial auditing (using unitary compliance criteria external to the historically elaborated disciplinary standards of good practice), practitioners of divergent research styles will continue to simulate consilience with the regulatory ideal so as to appear compliant, cooperative, and transparent—therefore ethical. The consistency-seeking logic of bureaucratic oversight persistently refuses to recognize diverse professional ethical standards as ethical, suspecting them instead of self-interest. However, readers should not confuse the simulation of compliance with cynicism on the part of researchers.
concerning their own actual ethical practice: far from it. It is, however, this very
chasm between regulatory ethics (as too often enacted in IRB reviews) and the
divergent practical realities of principled, upstanding research in this or that field
that has led to widespread cynicism concerning IRBs.

I consider one segment of discussion from the transcript of an NBAC meeting
devoted to the “Definition of Research” (NBAC 2000). My example is not drawn
from the panel that heard testimony from representatives of disciplines like
anthropology and oral history, because we might expect them to resist the com-
mission’s search for consistency. It derives instead from a panel devoted to “occu-
pational studies,” “health service studies,” and “population-based surveys”: fields
that we might expect to be comfortable with the existing definition of research
and its ethical entailments.7

Representing survey research, Norman Bradburn—then Assistant Director for
Social, Behavioral, and Economic Sciences at NSF—began his testimony by
observing that, unlike clinical research, survey research makes statements “about
groups and not about individuals.” Bradburn emphasized that—in contrast with
the Common Rule’s biomedically derived emphasis on the dangers of the research
process itself, population researchers’ two key ethical concerns—privacy and con-
identiality—relate to how data are used and shared after the research is over.

In a comment reminiscent of ethnographic objections to consent forms, Bradburn
noted that it is not necessary to document consent in survey or population
research because you are approaching people either in their homes, over the tele-
phone or sending them letters” and therefore they can easily refuse (NBAC
2000:40). Like ethnographers, survey researchers understand themselves to be
encountering respondents in contexts where the latter have the power to control
how the interaction develops.

Bradburn’s testimony was the last that morning before a general discussion
including both the invited panel members and commissioners. The panelists,
abetted by several of the commissioners, qualified one another’s definitions of
“research” with concerns derived from their own research experiences. As the real
complexities, not to say incoherence, of research cultures—with their corre-
sopondingly intricate network of ethical boundaries—were unveiled in testimony
and then reproduced in discussion, the group chose to retreat. Scrambling back
from that precipice, one of the panelists reasserted that “the challenge . . . is to
have some general principles that would apply across the different kinds of
research” (NBAC 2000:54).

In the event, metaphor came to the rescue. Two images helped articulate the appar-
ent reasonableness of demanding a single, regulatable object and ethical standard.
The first offered a Driver’s Ed model of human subjects research regulation:

It strikes me that . . . we cannot ask every IRB member to review a full
explication in every proposal of every potential ethical implication.
We can, of course ask how they are going to keep data confidential and a few other specifics. It seems to me the question is . . . whether there [is] standard guidance—guidelines, rules of the road for researchers who want to know how they can conduct research in an ethically acceptable manner. [NBAC 2000:64]

Later on, the implications of the Driver’s Ed model were developed this way:

Let’s imagine you took every 16 year old who was applying for a drivers license and they went of the Bureau of Motor Vehicles and . . . you had them sign a form that said that . . . they were responsible for coming up with all the rules that they were going to follow when driving. . . . We do not do that. We give them a set of guidance. We tell them these are the rules but we all know that just giving them the rules is not good enough. [NBAC 2000:89–90]

The implication is that—absent a central system of rules and oversight—researchers are isolated monads, making up ethical practice as they go without any notable social context. Indeed, throughout the day’s discussion and most especially during the commissioners’ final colloquy, scholarship’s social organization—for example, professional communities, their training practices and ethical conventions—was either ignored or viewed with suspicion.

The second metaphor offered a Copernican model of human subjects research regulation:

I cannot help having the impression that what we have is a system built up over a quarter of a century or so that resembles in many respects the Ptolemaic universe. . . . if someone asked me . . . how is it that we have the system we had, I would have to explain, well, it began—it was born in scandal with a few kinds of particular wrongs, some of them quite heinous wrongs that should never have happened, and the whole system was made to deal with those. That was the original design. . . . It began as a—you know, relatively focused idea but it has now tried to form all these epicycles to bring [in] all these other kinds of human subjects research. I think we need a Copernican revolution in human subjects research protection and I am going to ask you what you think ought to be at the center. What ought to be the sun in that particular system? [NBAC 2000:80]

Responses to this plaintive query reflected the same individualism evident in the “rules of the road” metaphor, only this time applied not to researchers but to research subjects. Mixing metaphors a bit, one panelist asserted, “Protection of the individual subject is the kernel of the system—should be the kernel of the system” (NBAC 2000:81). Another elaborated, “I think I would put risk at the center, the sun and so forth” (NBAC 2000:82); he suggested that as a focus, risk has the advantage of being amenable to economic models. One commissioner cautioned
(in vain) that if one argues “by analogy,” then “the analogy has to be a good one.” Nevertheless, Copernican images closed the meeting when the chair—prefacing a final question about differences among kinds of research—observed,

I do not know how to answer Tom’s question of what is the center of this universe but I—the issue that always comes back in my mind—I do not know if it is the center or not—is we find ourselves dealing with vulnerable—people who are vulnerable for one reason or another. They are vulnerable because they are uninformed. They are vulnerable because they may be exposed to risks. They are vulnerable for various reasons. [NBAC 2000:91]

Opting Out?

These discussions seem to suggest that the complex landscape of research across the disciplines resists resolution into one homogeneous terrain. To use the National Commission testimony’s all-too-revealing images, a “Copernican revolution” is not in the offing because the expanding research universe does not revolve around a single sun; nor will appeals to U.S. Department of Motor Vehicle “rules of the road” save us because the existing research highways are not all located in the same country.

An improved mapping of this terrain would begin to differentiate among its various frontiers. A recent initiative by one of the suspect scholarly professional associations alluded to above is worth mentioning. Defending the ethical integrity of its distinctive knowledge practices, a few years ago the Oral History Association (OHA) attempted to locate itself mostly off the map of regulatory “research.” In response to energetic lobbying by OHA and the American Historical Association (AHA), in August 2003 the OHRP exempted oral history interviewing from IRB reviews. An announcement signed by representatives of AHA and OHA explained, “Oral history interviewing projects in general do not involve the type of research defined by [federal] regulations and are therefore excluded from Institutional Review Board oversight” (Ritchie and Shopes 2003). It elaborated,

The type of research encompassed by the regulations involves standard questionnaires with a large sample of individuals who often remain anonymous, not the open-ended interviews with identifiable individuals who give their interviews with “informed consent” that characterize oral history.

However, over the next few months, queries and clarifications were exchanged between OHRP and OHA/AHA (Shopes and Ritchie 2004; Shopes et al. 2007); in the end, only a very narrow part of what oral historians do was in fact exempted from regulatory oversight.

Was this ultimately failed effort to establish a defensible frontier relevant to ethnography? The anthropologists I spoke with at the time of the oral history
ruling were briefly thrilled, but there was no notable effort to follow suit. Viewed comparatively, although anthropological fieldwork is similar to oral history interviewing in some ways, it differs dramatically in others. Like oral historians, anthropologists avoid administering standardized questionnaires to large samples of anonymous individuals (i.e., people about whom the investigator neither seeks nor records identifying information): a method common in quantitative sociology and demography. But whereas anthropological ethics conventionally privileges confidentiality—protecting the privacy of persons whose identities are known to the researcher—the professional ethics of historians demands names and other identifying particulars.

Despite their long-standing involvement in life history projects with named individuals, anthropologists do not do “oral history” as that term is used by the OHA (whose website outlines guidelines that are more constraining than anything in the Common Rule). Exempted by the OHRP for its radical particularity, oral history interviewing aims to create a reconsultable archive of testimony by named historical actors whose unique points of view are not yet part of the historical record. Oral history narrators sign legal agreements giving them copyright control over their narratives; they determine whether, when, and how widely their narratives may be made available for scholars to use. In contrast, ethnographic interviews almost never stand alone in this way. Anthropologists rarely archive their field notes and other field materials, which may include interviews but also a larger corpus of personal journals and other noninterview documentation of their own observations and interactions. These research artifacts remain in their private possession; control over them is not usually relinquished to informants.8

**Taking Consistency Ultra Seriously (A Modest Proposal)**

In a paper prepared for the National Bioethics Advisory Commission’s final report, University of Illinois lawyer C. K. Gunsalus (2001) commented critically on the commission’s call for an expansion of federal regulation of human subjects research. After six years of work, in her view, the regulatory object was still inadequately defined:

> Comprehensive application of the present [broad] federal definition of research . . . could sweep myriad low-risk activities into a regulatory structure with unknown costs and implications. Activities that have never before been labeled as “research” could become subject to regulation, commanding resources for their review and oversight, ultimately to the detriment of human subjects in higher risk situations. [2000:D5]

Oral history’s effort to define itself out of the expanding ambit of IRB oversight did little to locate a principled definitional boundary; there is no reason to think that anthropologists or any others might be more successful.

Maybe we need a different approach.
One gambit as yet unexplored is to take the managerial demand for consistency super seriously and to ask what research-like activities have been arbitrarily, even narrow-mindedly, excluded simply because we are not (yet) in the habit of construing them as “research.” To this inclusive end, sociocultural anthropology is handy exactly on account of its apparent location at the outer border of what might pass for social, behavioral, and biomedical science, due to its reliance on participant-observation. Its lack of a methodical method, its processually indistinct beginnings and endings, and its preference for case-centered interpretations over explanatory generalizations are suggestive of a host of other so far woefully underregulated activities that may be considered systematic and productive of generalizations in distinctively humanistic (rather than scientific) ways.

It is, after all, simply a historical accident that participant-observation fieldwork is located as a species of human subjects research. That fact is a contingent function of the disciplinary organization of universities that typically groups sociocultural anthropology with social sciences like sociology or economics, rather than with humanities like cultural studies and creative writing.

And it must be admitted that participant-observation practices and their ilk are not entirely innocuous. What risks do they entail? Like survey researchers, ethnographers seek out informants on their home ground, but unlike survey researchers, ethnographers are easier to overlook and harder for their ever-hospitable informants to shake off. Although they do identify themselves at first, fieldworkers hang around for months—for years even—embedding themselves deeply in their interlocutors’ communities and always threatening to fade into the social background.

The behavioral/biomedical model of “good science” (Penslar, with Porter 1993, ch. 4:1)—shared by interview-based social science—aims to construct a purified relation between knower and known, carefully bracketing other potential identities and expectations by controlling the contexts and media of their engagement. In contrast, the research relation in fieldwork is socially contaminated, crosscut with multiple, potentially conflicting demands and possibilities (Lederman 2006b, 2006c). Not at all a contingent feature, this contamination is the very source of participant-observation’s power as a means of understanding social worlds. Russell Bernard (2000) has warned that because participant-observers embed themselves in their informants’ communities, even when they are scrupulous about avoiding active deception, a confusion of hopes and intentions is inevitable. There is a potential for anger over apparent acts of betrayal, or simply for disappointment over the inevitable inadequacies of interpersonal understanding (e.g., Brettell 1993; Fine 1993).

However, although anthropological fieldwork may be unusual when compared with the behavioral and social sciences, the character of its potential to disturb as well as to enlighten is far from unique. If there are indeed other ways of knowing the world that are similarly entangled in the everyday but not yet benefiting from
IRB oversight, doesn’t fairness dictate that all of these modes be surveilled in the same manner? What would happen if ethnographers made common cause—all in (or all out)—not just with ethnographically inclined sociologists, political scientists, religion scholars, and folklorists but also with urban planners, architects, engineers, literary and cultural studies scholars, and colleagues in college and university writing programs—all of whom are engaged in varieties of research-with-human-participants?10

Anthropological fieldworkers and literary fiction writers make a particularly interesting set. Is the nature of “risk” faced by a writer’s friends, family, and communities qualitatively different from the risks faced by an ethnographer’s informants? Is the assertion that novelists are not conventionally thought to do “research” in preparation for writing a sufficient justification for excluding their work from the expanding application of the regulatory definition? Their intent to publish and to edify clearly qualifies their work for review (as clearly, that is, as many of the other things being reviewed recently).

Just where do novelists get their ideas? Answering this question may prompt a review of lessons we have learned to tuck out of sight (if my students’ memories ring true) concerning the dangerous frontier between imagination and worldly reference.

The Novelist’s Notebook

This article’s second pair of epigraphs counterpose an excerpt from Joan Didion’s essay on keeping a notebook and a passage from Margaret Mead’s letters from the field. Didion compares imagining—making things up—to lying. But she argues that this otherwise scandalous transformation of experience works to preserve personal truths: sources of her edifying writings. Literary imagination is liberated by its worldly encounters. In contrast, Mead implies that although anthropologists also transform experience as they write, the ethnographic imagination is productively constrained by its worldly encounters. “[W]ho is and helplessly dependent on what happens,” their charge is to preserve intersubjective actualities.

“Where do you get your ideas?” is a favorite question asked of fiction writers, so they tell us. A rich source of such tales, the New York Times series “Writers on Writing” (New York Times 2001) offers a sampling of responses. Carol Shields’s contribution, “Opting for Invention over the Injury of Invasion” (211–216), may be used to assay the commensurability of ethnography’s and literary fiction’s enabling social conditions and ethical dilemmas. It may suggest just how far we could extend this article’s exploration of semi-overlapping similarities and differences among modes of knowing the world and, therefore, just how unmanageable are the boundaries defining “research” as a regulatory object.

Shields describes running into an acquaintance at the mall who admitted buying candles to match the colors of her nightgowns. Shields’s first impulse was to
incorporate this glimpse of “extraordinary otherness” into the novel she happened to be writing. Later in the year, as she was reading the novel’s proofs, she reconsidered. She removed the reference for fear that the woman—recognizing herself and suffering a “storm of self-consciousness”—might end their acquaintance. She is sure that “no one wants to make an appearance in someone else’s fiction,” and she confides that her central worries as a writer concern violations of other persons’ integrity. Like some ethnographers, perhaps she overestimates her own power or her friend’s sensitivity. Nevertheless, her concern over writerly privilege inadvertently recalls a familiar ethnographic guilt: just like fieldworkers, authors glean opportunistically from among the insights that come unbidden in their everyday encounters and they wonder whether and how to accept and repay these inadvertent gifts.

Shields is silent about the ethical rationale for reporting her encounter in this nonfiction essay. Other fiction writers are more forthcoming with their ideas about the different responsibilities entailed in fiction and nonfiction writing. In The Facts: A Novelist’s Autobiography, Philip Roth (1988) is chastised in an epistolary afterword by his fictional alter ego, Nathan Zuckerman, for ducking the hard questions about his central character’s “motivation.” Together they make a powerful—and thoroughly queered—case for fiction’s superiority over nonfiction as means for rendering a writer’s voice ethical and brutally honest simultaneously. Despite influential interventions on this matter over the years, anthropologists are still at least vestigially ambivalent about their dependence on interpretive ethnographic invention, like that to which Mead refers: they are all about “what happens.” Conversely, Shields and other contributors to Writers on Writing show us that many novelists are embarrassed about their dependence on experience: their trademark is “imagination.” Whereas ethnographers relish them, novelists resist answering questions about where they get their material. Instead, with Shields, they hope that readers will understand “what fiction really is” and cease reading biography into that work. Nevertheless, when Shields admits that anyone who knows a writer knows “they are in danger,” her admission has an ethnographic echo.

Rich variations on this theme of the worldly sources of a writer’s imagination appear regularly when writers discuss their work. When Toni Morrison was interviewed on National Public Radio in 2004 on the occasion of the publication of a new edition of her collected works, she commented that she is asked so often whether she draws on her actual life that she can no longer take those encounters seriously (she mugs a typical question-and-answer). At the same time, she admitted that, reading her work over in preparation for writing an introduction to the new edition, she was struck by how powerfully her writing did in fact draw on experience in intimate ways. In contrast with Shields’s story, the appropriations Morrison retrospectively recovered were unpremeditated and not always conscious.

Writers deemphasize the influence of experience on their work relative to imagination and language; ethnographers reverse this weighting. Literary and
anthropological self-representations and their respective interlocutors’ expectations are distinctive in other ways as well. Moreover, this article’s opening narratives of schooling suggest that the heavily policed boundary between imagination and literary form on one hand, and either direct or mediated experience—“what happens” in both Mead’s and Didion’s terms—on the other, may define our earliest, most general sense of what is and is not research.

Nevertheless, as already noted, participant-observation fieldwork is a limiting case among social and behavioral research methods. For all their evident differences, the resemblances between ethnographic and literary practices are uncanny. In particular, the ethical dilemmas distinctively engendered by working where one lives afflict ethnographic and literary writers in similar ways. If consistency is the measure of bureaucratic virtue, then it is hard to imagine how a bureaucracy of virtue might justify subjecting ethnographers and novelists to different systems of ethical accountability.

Conclusion

Can a comparison between literary fiction and ethnography be sustained as something other than parodic in a forum about the politics of “informed consent”? Is the contrast between Didion’s privileging of imagination and Mead’s privileging of “what happens” a good enough reason why college writing professors and their autobiographically inclined students so far mostly escape IRB scrutiny—imagination being excluded from our primary consciousness of what constitutes “research”—while ethnographers, journalists, and oral historians and their students do not? Are fiction writers free to write what they like because “making things up” categorically renders their work sufficiently harmless in the real world?

Well, no. Writing teachers playing at the border between fiction and nonfiction have been sanctioned (e.g., Wright 2004); and novelists have in fact been sued for libel by people who—recognizing themselves in a work—believe themselves defamed or otherwise harmed (see, e.g., Hilden 2003). A comparison between the possibilities for harm and redress in ethnography and literary fiction is not unreasonable. At the same time, it suggests contexts other than human subjects research regulations in which researchers might be held responsible for their actions.

These contexts certainly include disciplinary communities, which have—in part through their professional associations—long-standing although certainly imperfect histories of active concern for and engagement around ethical research practice.

These contexts also include the courts. With ethnography as a background concern—and bearing in mind Didion’s distinction between “lies” and “fictions”—legal arguments over libel charges in the case of journalistic or literary satire are especially interesting. Judicial decisions about what a contested piece of writing is are precedent-informed judgments concerning how readers might reasonably interpret the work and how such interpretations might affect an aggrieved party’s reputation. Despite their own imperfections, they recognize a wider array of
agents and aims than IRBs are empowered to recognize, including both the protection of vulnerable persons and the legitimacy of social criticism.

The social value of social research is all too readily marginalized when the principled demand of bureaucratic regulation for a single manageable standard faces the recalcitrant heterogeneity and improvisational subtlety of genuinely ethical research practice. But that social value is the heart of the matter for ethnographic sociologists, journalists, and others who write about the implications of IRB prospective review for First Amendment protections against censorship (e.g., Hamburger 2005), and who consider U.S. courts more accountable and capable of ethical nuance than IRBs, even the best of which are severely limited by their regulatory mandate.

Sociocultural anthropologists have recently joined this conversation, contributing their characteristically disorienting, critical–comparative perspectives. Doing its bit, this article has sought to defamiliarize the regulatory object itself by following “research” across contexts, regulatory and otherwise, where its resistance to heuristic objectification is obvious. Whether ethnography in particular is ever excluded from IRB oversight is a problem that remains. Meanwhile, anthropologists will not be able to keep themselves from exploring the entailments of their inclusion and from making ethnographic sense of the regulatory cultures that are increasingly consequential across social domains.

Notes

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1. As a disciplinary strategy, relativizing comparison uses cross-cultural juxtapositions to make evident the specific cultural content of apparently culture-neutral universals (Lederman 2005:50–54).

2. The terms participant-observation and especially ethnography have divergent connotations in different fields. Unlike the social science norm, anthropologists have historically relied more on the conversational and participatory development of social relations than on interviewing (e.g., Briggs 1986).

3. At the same time, participant-observation’s absence of method (in this sense) is the source of its value relative to other social science approaches.
4. Much more could be said than I have space for here concerning how morally weighted distinctions—like plagiarism and citation—bearing on attribution, ownership, influence, and inspiration shift their valences as we move across the sciences and humanities, and outside of academia altogether (see, e.g., Lethem 2007). This vast landscape is background for the present, narrow goal of teasing apart expectations knotted together around “research” in the context of the regulatory ethics management.

5. Although the practical slippage between therapy and research can be inadvertent and benign, the notorious cases that precipitated the establishment of IRBs involved deliberate deception of people who believed they were receiving personal medical care when they were in fact experimental subjects.

6. For a powerful argument about this dynamic, see Bledsoe et al. 2007. For more on the “simulation” of compliance and its alternatives, see my discussion, “On Fieldwork Boilerplate: An Educational Experiment” (http://savageminds.org).

7. All quotations are from verbatim conference transcripts and appear here without corrections.

8. Assuming that anthropological field notes are like unarchived documents, historians have difficulty understanding the justification for such privacy; but that analogy is misleading in ways I have insufficient space here to discuss. (As implied above, anthropologists have reciprocal misunderstandings of historiographical ethics.)


10. This question is not strictly hypothetical. IRBs are already involving themselves in the lives of writing teachers, journalists, and others who had not heard of “human subjects research” until their own work came under scrutiny (e.g., Wright 2004).

11. Russell Banks and others in Writers on Writing qualify this brilliant, complex provocation in ways, for want of space, I cannot discuss.

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