INTRODUCTION: DISCIPLINE MATTERS

In June, 2008, the US National Endowment for the Humanities (NEH) began a program to support the development of courses addressing “Enduring Questions” (e.g. “What is evil?”). According to the competition description, “enduring questions” are questions to which no discipline or field or profession can lay an exclusive claim. In many cases they predate the formation of the academic disciplines themselves. Enduring questions can be tackled by reflective individuals regardless of their chosen vocations, areas of expertise, or personal backgrounds. (NEH 2009)

The following year’s competition announcement was met by frustrated, not to say infuriated, commentary by philosophers on PEA Soup (a philosophy, ethics, and academia blog). Commentators construed questions like these to be “philosophical”, not in a contingent historical sense but essentially so, and therefore treated the competition as evidence of the NEH’s profound ignorance of their discipline.

Against the “predisciplinary” tag, Ben Bradley argued that the philosophical field of ethics has been devoted to answering these questions for centuries. I think this gives us, if not an “exclusive claim” (whatever they mean by this), at least some reason to think that we have special expertise in teaching courses on these subjects. (This and the following commentaries are in Bradley 2009.)

Agreeing that the program description reflected a popular misconception, another asked, “After all, how can ethics be a serious academic discipline if
no special skill is required to do it well?” A third commentator observed:

One might, for instance, study how ethical attitudes were expressed through fiction in a Literature course .... And so on. However, to ask these enduring questions directly and attempt to answer them oneself is to begin to do philosophy, and expertise in philosophy is required to effectively teach a course directly addressing an enduring question.

Boundary worries are common among professional disciplinary communities. These philosophers’ identification with “enduring questions” should remind anthropological readers of their own field’s anxiety over English professors playing fast and loose with “culture” or market researchers, say, playing faster and looser with “ethnography”. Worries of this sort can be overplayed: disciplinary borders, like cultural ones, are normal sites of cultural, social, and material exchanges; scholarly creativity often takes the form of inadvertent trespassing, playful exploration, or strategic cross-border raids, alliances, and migrations; and disciplinary regeneration relies on the transformative effects of these maneuvers. And of course intellectual work takes place all the time in (academically) undisciplined spaces (see Mills infra).

While the philosophy–anthropology border is not as trafficked as some others, it is hardly a wasteland. With respect specifically to ethical questions, philosophers have been provoked by anthropological work (e.g. Hacking 1998); philosophers and anthropologists have collaborated (e.g. Edel and Edel 1968); and several generations of anthropologists have adopted, adapted, and engaged with a variety philosophical traditions (e.g. Read 1955; Geertz 1968; Rappaport 1999; Faubion 2001, 2011; Laidlaw 2002; Lambek 2010b). Nevertheless, the acknowledgment of disciplinary distinctions can also be a critical move, a point to which this chapter will return in its final sections; and for sociocultural anthropology particularly, attentiveness to boundaries is itself part of the disciplinary project. In that way, this chapter’s approach is anthropological and without philosophical designs (and see especially Lambek 2010a: p. 7ff).
ETHICS IN PRACTICE

Among anthropologists, “ethics” tends to refer to sociable practices and culturally-legible frames for assessing and indexing the “goodness” or “rightness” of human conduct: that is, it refers to the quality of human selves vis-à-vis other persons (see Keane 2010; Rumsey 2010; compare, e.g., Silverstein 1976; Foucault 1984). Anthropological ethics-talk tends to situate ethics and politics in an internal relation (e.g. Ackeroyd 1984; Armbruster and Laerke 2008; Price 2009a): that is, an ethics, in the sense of moral evaluations worth arguing about, implies a politics, in the sense of alignments giving those arguments socio-material form and impact.1

Michael Lambek (2010a, 2010c: 61) construes ethics as “a property of speech and action” rather than “a discrete object” (and see Rumsey 2010, who demonstrates that point and maps its bio-psychological dimensions). From this perspective, the ethical is woven into the human insofar as human being is irreducibly sociable and ethical assessments index the relations in terms of which persons act.

Because social relationality is both the medium and object of their research, anthropologists have found morality/ethics a challenging topic to delimit. Durkheim’s influence has been blamed for this difficulty (Laidlaw 2002; Zigon 2008), although there are other ways of reading his work and Lambek (above) points to a more profound and interesting obstacle blocking projects of delimitation.

In any case, this chapter’s references to the social and the moral ordering of relations are not meant narrowly to connote obligation or collective forms: they are not meant to foreclose creativity, ambiguity, inconsistency, and the open indeterminacy of human experience and its ethical challenges (see Lambek 2010a: 9–13). Nevertheless, ethical judgment and choice – the spaces of personal “freedom” (e.g. Laidlaw 2001; and see Faubion 2001 re: Foucault) – become significant both for selves and others

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1 This chapter follows the common anthropological practice of using “ethical” and “moral” more or less interchangeably (e.g. Edel and Edel 1968; Lambek 2010a: 8–9). Faubion (e.g. 2011: 20–4) has proposed freshly sharpened distinctions in response to the inconsistencies built up in cross-disciplinary writing on ethics/morality (but compare, e.g., Howell 1997 or Zigon 2008, who reaffirm one or another familiar mode of distinguishing ethics from morality).
insofar as they come to be situated in, and have implications for, socio-historical contexts that oftentimes re-position intentional values (see Sahlins 1981; Fernandez and Huber 2001).

ETHICAL PROMISES AND PREDICAMENTS OF FIELDWORK

For over a century, cultural translation has been at the heart of social anthropology’s project, both intellectually and ethically (e.g. Beidelman 1971; Boon 1982, 1999: Chap. 4; Asad 1986; Fox 2000). Its foundational condition of possibility has been fieldwork—the immersive social engagement and disciplined, close listening to vernacular “native points of view” in unfamiliar social fields. Anthropological fieldwork has informed and been informed by a distinctive 
comparativist sensibility, attentive to contextualized practices and beliefs and, over its history, progressively skeptical of the universalizing, naturalizing, and normalizing of Western folk categories and practices (e.g. Sahlins 1972, 2000, 2004; Strathern 1990; Lederman 2005). Its intended effect “was to ‘displace’ or remove readers from their home cultural beliefs” enabling them to develop “a critical consciousness of [their] own culture from the account of another” (Fox 2000: 4–5).

“Cultural relativism”, commonly associated with Boasian cultural anthropology, is similarly characterized (e.g. M. Wax 1987; Kelly 2008; see Boas 1896; Boon 1982, 1998; Vann infra). Insofar as anthropologists have typically studied non-Western cultures and construed their students and readers to be Euro-Americans, cultural relativism offered a critical challenge specifically to Western values and beliefs (e.g. Engle 2002). Unfortunately, Boasian relativism has also been caricatured, by anthropologists and others, as a global injunction against researchers having their own values and even a prescription for embracing those of their hosts. Contradictorily, relativism is also caricatured as an assertion of the insularity, ineffability, and untranslatability of unfamiliar world views (for one ambivalent example, see Hatch 1981; compare Geertz 1984). But anthropologists do not “go native” to do credible work; and if they are “native”, their disciplinary sensibility makes them something else again.

Either way, immersive involvement in with some others’ ways of
being in the world has, over the past century, enabled social and cultural anthropologists to raise novel questions not only about Western folk understandings but also about the categorical foundations of academic philosophy and social theory. Concepts of personhood, community, and the sacred, intentionality, responsibility, and reciprocity, shame, guilt, and justice, and other elements of Euro-American ethical and moral discourse are unpacked and decomposed during long-term fieldwork, then recontextualized in ethnographic depictions, and examined comparatively for what is lost and gained in translation. This work is explicit both in classical ethnographic arguments and their renovations (e.g., concerning witchcraft, sorcery, and gossip, or concerning forms of exchange and implications for social personhood, see Mauss [1925] 1990; Evans-Pritchard 1976; Lindenbaum 1979; Parry and Bloch 1989; Stewart and Strathern 2004; Pietilä 2007) and in more recent literatures (on, e.g., the cultural politics of new medical technologies and imaginaries, see Sharpe 2006 and see below).

Ironically, the conditions enabling the ethical promise of critical comparativism also present an ethical predicament. G.N. Appell, whose 1978 ethics case-book is worth (re)reading both for evidence of historical shifts in the discipline’s assumptions and of continuities among the dilemmas that anthropologists have faced, put the point this way:

By its very nature, cross-cultural inquiry takes place at an interface of ethical systems. As a result, the anthropologist is frequently forced to make a choice or select a plan of action in an environment of conflict between different customs, principles, and values that normally shape action [within which] no obvious or immediate solution may exist. Furthermore, the investigator usually has to make a decision without adequate information or sufficient time ....

Complicating the problem ... is the fact that the anthropologist occupies multiple roles [all of which] include moral expectations which can conflict and may be unreconcilable. As a result, an anthropologist characteristically must be able to tolerate a certain degree of moral ambiguity. In
fact the best field workers may well be those who can acknowledge and live with these moral ambiguities. (Appell 1978: 3) Ethical practice in anthropological fieldwork, thus, balances deliberately at the point of conjuncture between inevitably-multiple and potentially-dissonant reference frames: including the anthropologist’s disciplined stance, his or her culturally-informed personal values and commitments, and a welter of partially-understood values, aims, and commitments informing the actions of everyone (hosts, sponsors, gatekeepers, ordinary people, etc.) with whom the anthropologist is socially entangled within his or her scenes of research. The case studies Appell assembled over thirty years ago, together with others published before (e.g. Rynkiewich and Spradley 1976) and since (e.g. Cassell and Jacobs 1987; Caplan 2003; Meskell and Pels 2005) illustrate and reflect on the moral predicaments of anthropological fieldwork as instances of the more general negotiation of cross-cultural encounter, translation, and understanding.

ETHICS EVERYWHERE

In recent years, paralleling a wider “ethical turn” across the humanities and social studies (e.g. Garber et al. 2000; Davis and Womack 2001), attempts have been made to delimit an “anthropology of” ethics or morality as a focus for ethnographic description and analysis (e.g. Howell 1997; Faubion 2001; Laidlaw 2002; Fassin 2008; Zigon 2008). These projects agree about the value of bringing ethnographic accounts of ethical judgment into the foreground in order to sharpen anthropological contributions (see particularly Laidlaw 2002). To this end, they have sought intellectual resources from outside anthropology, especially in philosophy and social theory (notably Aristotle, Kant, Foucault, Heidegger, Alisdair MacIntyre, and Bernard Williams).

Over the past couple of generations, anthropologists have renovated their discipline on several fronts by turning the contexts and means of disciplinary practice into its substantive contents. Recent advocacy for an “anthropology of” morality/ethics recalls earlier advocacy for an “anthropology of” colonialism or gender, for example, each of which
asserted that foregrounding new ethnographic objects was necessary to advance understanding, however much we had been studying these things all along as dimensions of kinship, politics, and other already-marked topical domains.

These arguments are compelling but are not universally joined by those who promote more focused, explicit, and philosophically-informed anthropological attention to the ethical/moral. Notably, several contributors to Lambek (2010b) argue persuasively on philosophical and broadly-disciplinary (that is, evolutionary/developmental, linguistic, and socio-cultural) grounds that the ethical is so thoroughly implicated in the sociable nature of human being as to resist delimitation as an ethnographic object in its own right. Oddly, the emergent “anthropology of” ethics/morality is limited in another way. While proponents have asserted the importance of representing anthropological observers as ethical actors in their scenes of research (e.g. Fassin 2008), this literature tends not to engage explicitly with anthropological writings on the “ethics of” anthropology.

If instead we treat research ethics not as an externality but as an ethnographically substantive dimension of anthropological reflexivity, there is no question that reflections on ethical practice have long constituted both a focused literature and a much wider field of ethnographic description and debate. A survey of this wider discourse is in order; after which, this chapter will consider how an “anthropology of” ethics might treat research practice as a properly ethnographic object, as a basis for forging linkages among these literatures.

ETHICS IN TRANSFORMATION

During the 1960s and 1970s, when articles, monographs, and case books explicitly devoted to anthropological ethics, like Appell’s, began to appear, ethical issues – like choices concerning self-representations (e.g. class or sexual identities) in light of their implications for the power dimensions of research relationships – were also evident throughout a new and expanding corpus of first-person narratives with diverse thematic and analytical emphases (e.g. Golde [1970] 1986; Spindler 1970; R. Wax 1971; Rabinow 1977; Kulick and Wilson 1995; Lewin and Leap 1996; Armbruster
They have also been present, over the past three decades, in politically-charged critical reflection on anthropology’s lineages and present configurations written in light of the multifarious effects of post-World War II decolonization, including the US Civil Rights movement, opposition in Europe and the US to the war in Southeast Asia, activist and academic feminisms, and widespread concern with global inequalities evident amidst resurgent globalization (e.g. Hymes 1972; Asad 1973; Rosaldo and Lamphere 1974; Reiter 1975; Fabian 1983; Marcus and Fischer 1986; Fox 1991; Harrison 1991, 2008; Sahlins 1992, 2000; Carrier 1995; Baker 1997; Cooper and Stoler 1997; Amit 2000; Moore 2000). Ethical motives have been prominent as well in critical commentary on the discipline’s forms of representation (e.g. Clifford and Marcus 1986; Behar and Gordon 1996) and other dimensions of professional practice, including pedagogy and mentoring (e.g. Cerwonka and Malkki 2007; Faubion and Marcus 2009).

Pat Caplan (2003: 5, following Appell 1978 and, after a fashion, anticipating Zigon 2008 re: “moral breakdown”) offered one way of making sense of the pervasiveness of ethics-talk:

it is precisely at the moment when the boundaries of a discipline are redefined that ethical discourse increases .... In other words, debates around the topic of ethics are part of the way in which anthropologists seek to constitute themselves as a moral community.

These varied writings cohere as a sustained moral argument over the form and content of sociocultural anthropology in a post-colonial era characterized by global interconnections, conflicts, and inequalities. Calls for making “practical ethical engagement” a priority and for “embedding ethics” in methodological reflection and practice (Meskell and Pels 2005: 1, 21–25) are being answered in multiple quarters; and worries that anthropologists do not actively read, discuss, or use ethical resources (Fluehr-Lobban 2009) can surely be laid to rest.

A number of themes recur throughout this teeming landscape. They directly reference (a) the changing circumstances for enacting ethical
fieldwork in a decolonized Third World and for revaluing fieldwork in metropolitan centers, (b) a pervasive interest in inequality, oppression, and contestation within and affecting the communities where anthropologists work.

Thus, Laura Nader’s (1969) influential argument about “studying up” inspired many to do fieldwork among elites. They matched studies of colonized subjects with studies of colonials in Third World countries and back in Europe; and they extended attention to a wider array of metropolitan actors including, for example, doctors, scientists, and bankers. Relatedly, they extended topics and venues for fieldwork in Europe and the US and argued for its theoretical importance. Simultaneously, they brought fieldwork in archetypal out-of-the-way places, like Papua New Guinea and Kalimantan, to bear on these trends (e.g. Lederman 1998).

Insofar as anthropology’s moral principles and most ethics writing still presume the discipline’s classic subjects – marginalized people with whom researchers hope to collaborate and make common cause (e.g. Lassiter 2005) or for whom they seek to advocate (e.g. Fluehr-Lobban 2008) – they are not a neat fit for anthropologists’ shifting orientations, including a sharper focus on metropolitan actors and technical and political-economic elites (see especially Meskell and Pels 2005), on social fields encompassing mutual antagonists among whom the fieldworker was never neutral (e.g. Martin 1995), and on social actors whom the anthropologist finds problematic or offensive (Handler 1988; Holmes 2000; Harding 2001). Nevertheless, the discipline’s methodological commitment to an ethics of intimacy is surprisingly durable, in the sense that even a “militant” anthropology entails disclosure of the researcher’s identity and interests in backstage spaces (Schepers-Hughes 2000, 2004).

These same changes have had implications for the concepts of “culture” (especially in the US) and “society” (especially in the UK) that long oriented anthropological work. In an earlier era, cultures and societies were commonly represented as if bounded, internally homogeneous, and timeless. In contrast, greater explicitness in contemporary writing about the socially embedded character of ethnographic understanding – as a project of
“understanding relations by means of relations” (Strathern 2004) – more readily suggests the open-endedness, reflexivity, and indeterminacy of field practice while construing cultures and societies as unbounded, internally heterogeneous (not “shared”), and historically contingent.

For all these reasons, it has also become increasingly unpersuasive to approach cultural translation, including its critical impact, as a movement between interlocutors “in the field” and readers and audiences “at home”, because the two are likely one and the same (Brettell 1996). Not only do anthropologists work in already-familiar settings among literate people, but accelerated global movement and new communication media have enabled our classic interlocutors, literate or not, to engage us when they choose, rather than at our pleasure. The distinctions between “research work” and “personal life”, always blurred during fieldwork, weakened further, forcing us to become more conscious of “everyday ethics” (Silverman 2003) in descriptions of field encounters (e.g. Ackeroyd 1984; Lederman 2006a; McLean and Leibing 2007; see also Geertz 1968).

ETHICS IN PRINCIPLE

In contrast to accounts of quotidian ethical dilemmas, when public controversies mobilize professional communities they are constrained to move from messy, multi-perspectival, and polarizing particulars to an outcome of some sort: a critical analysis, definitive (even if mutually inconsistent) summations, another revision of professional principles and codes, an official (however contested) report, a policy statement. Especially after World War II, professional bodies like the American Anthropological Association (AAA) drafted position papers and ethics codes articulating principles that members could use in explaining their work to their publics, including funders, government regulators, and non-academic employers.²

PUBLIC CONTROVERSY

Sociocultural anthropology’s best-known writing on professional ethics has focused on high-profile cases and long arcs of crisis-driven

² As this chapter was being completed, the most recent overhaul of the AAA ethics code was entering a final phase of membership vetting.
controversy that engaged national and cross-national anthropological communities and professional associations (e.g. Fluehr-Lobban 2003; Mills 2003; Nugent 2003). These burst into view publicly outside of professional circles with consequential effects on public perceptions of the discipline (e.g. Gledhill 2000) prompting formal policy statements (e.g. AAA 1947, 1999, 2007) and ethics codes (e.g. AAA 1971, 2009; Association of Social Anthropologists of the UK and Commonwealth 2011; May 2005; and generally Stoerger 2005).

These crises have notably concerned the involvement of anthropologists in government- and military-sponsored counter-insurgency research: Franz Boas’s 1919 public denunciation, in The Nation, of “anthropologists as spies” and his professional community’s critical response; revelations during the 1960s concerning the abortive Project Camelot in Latin America (Horowitz 1967) and counter-insurgency research in Southeast Asia (Wakin 1992) that originally prompted the AAA to form an ethics committee and adopt a principled stance in 1971. More recently, ongoing investigations, publications, and organizing by sociocultural anthropologists, within and outside of the AAA and the Association of Social Anthropologists of the UK and Commonwealth, responded to intensified interest in social scientific and, particularly, cultural knowledge by the US military and national security establishment after the attacks on New York and Washington in 2001 (e.g. CEAUSSIC 2009; Price 2009b; Gledhill 2006; Moos n.d.; see also Albro 2010). These present-oriented involvements have been complemented by retrospective investigations into the activities of anthropologists during World War II and the Cold War (e.g. Price 2008).

Major controversies with ethical resonance and public impact were also prompted by revelations about prominent disciplinary figures, particularly concerning their variously compromised field relationships. These include the posthumous publication of Bronislaw Malinowski’s Trobriand Islands field diary (Malinowski 1967; M. Wax 1972), controversy over the accuracy and social competence of Margaret Mead’s Samoan research and of Derek Freeman’s critique (Freeman 1983; Shankman 2009), and extended critical discussion of Napoleon Chagnon’s research with the
Yanomami (Nugent 2003; see Stoerger 2005 for links to a wider array of controversies, and below).  

**PROTECTING AND PROBLEMATIZING PROFESSION**

There is an emphasis on “profession” in American ethics writing (e.g. Fluehr-Lobban 2003; see Meskell and Pels 2005: 1–26); British and European perspectives on major disciplinary crises are more attentive to post-colonial anthropological responsibilities (e.g. Caplan 2003). All, however, consider ethical entailments of the movement of many anthropologists out of the academy to work for non-governmental organizations, corporations, and governments, where their research and products are governed by conventions different from those of academia. The regulatory formalization of professional ethics emphasized in Fluehr-Lobban’s (2003) collection has met with suspicion, not to say hostility (e.g. Caplan 2003), including denunciations of ethics codes as screens protecting anthropologists rather than the people they study (Nugent 2003; see also Pels 1999).

These incongruent stances may reflect the different climates of academic work in the US and UK. Suspicions concerning the rationalizing impacts of bureaucratic oversight make sense in the fraught financial circumstance of British universities suffering the administrative surveillance of “audit culture”. The imposition of only-apparently neutral accounting standards evaluating “excellence”, in the form of standards blind to disciplinary variation and which have inhibited critical scholarship (e.g. Van den Hoonaard 2011), bears down heavily on the notoriously if necessarily slow pace of fieldwork (Strathern 2000; see also Marcus and Okely 2008).

In any case, Caplan (2003), Meskell and Pels (2005), and other recent works reinforce the caution concerning “moral ambiguity” that Appell articulated decades ago. Insofar as they represent the profession’s core principles succinctly as rationalized abstractions, codes and formal policy statements can be helpful to anthropologists in some of their communications with non- anthropologists, including college students (our

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3 While Malinowski and Mead are disciplinary icons, Chagnon’s prominence derives from the reliance of introductory courses on publications and films associated with his research, which has come to represent anthropology to generations of students.
future publics), members of ethics review panels, journalists, and employers in the public and private sectors. However, they are inadequate within the discipline for preparing students for fieldwork. The AAA has explicitly considered its accumulated ethics cases and its code as “educational” devices, both before and especially after ending the adjudicatory functions of its Committee on Ethics in the middle of the 1990s. But unlike case materials, the necessarily abstract principles articulated in a code (e.g. “do no harm”) sidestep all the key questions of practice. Anthropological ethics must be taught and learned experientially in situations that simulate fieldwork’s varied and unpredictable, therefore unroutinizable, conditions.4

For all their abstraction, disciplinary codes of ethics have also proved inadequate as means for squaring the realities of research practice with abstractly-framed ethical strictures written into legal codes governing research in the US and elsewhere. In the US since the 1970s, academic, medical, and other institutions that accept federal government funding are expected to comply with rules governing “human subject research” (see Lederman 2006a and references therein). Compliance is overseen by local Institutional Review Boards (IRBs: also referred to as “human subject committees”, similar to research ethics boards or committees in Canada, the UK, and the EU). IRBs are composed of researchers with biomedical, behavioral, and social science expertise, and community members representing research-subject interests. While it had other motivations too, over the past generation ethics-talk in anthropology and neighboring fields responded to the expansion of regulatory oversight. Notably, since the 1990s regulatory language (e.g. “informed consent”) has been incorporated into professional ethics codes.5

The regulations administered by IRBs enact philosophically-abstracted ethical universals – “justice”, “beneficence”, and “respect for

4 Indeed, this is true of practice in the professions more generally. For example, Gardner and Shulman (2005) argue that a key measure of “professional” competence is the capacity for ethical judgment amid uncertainty and doubt.

5 Because terms like “informed consent” can have divergent meanings in neighboring disciplines, apparent similarities among ethics codes using this language can be misleading: see Lederman (2009) for a comparison of sociocultural anthropology and experimental psychology, focusing on contrasting stances regarding the use of deception.
persons” – articulated in the Belmont Report (National Commission 1979), which drew on international agreements like the Nuremberg Code (Nuremberg 1949: 181–2) and the Declaration of Helsinki (World Medical Association 1964). IRB work is also guided by an ethic of fairness operationalized around the bureaucratic value of “consistency”: one set of rules for research in biomedicine, behavioral science, and social science.

The problem with all this is that research practice is not generic, and neither are research ethics, however much their codified representations appear the same in the abstract across the disciplines (Lederman 2007, 2009; cf. Fluehr-Lobban 2009). The difficulties anthropologists and other scholars have had in translating accurate depictions of their research conduct into regulatory language are notorious. These difficulties testify to the consequentiality of differences among the research styles and ethical infrastructures of disciplinary cultures. The challenge is to find ways of locating this diversity comparatively in this ethico-epistemological landscape.

One way of addressing that challenge is to incorporate an understanding of anthropological ethics (field practices and professional principles) into a comparative anthropology of ethics expansive enough to include ethical discourses and practices across the disciplines.6

So far, recent calls for an anthropology of ethics/morality (referred to above) have advocated renewing anthropology’s dialogue with philosophy while paying more focused attention to the ethical orientations and moralities of field communities. But while they have problematized “ethics” in various ways, proponents have taken the disciplinary practices and values of “anthropology” – along with those of other epistemic cultures, philosophy included – largely for granted.

The following necessarily-brief discussion is meant to advance the project of a more inclusive comparative anthropology of ethics, capable of incorporating academic practices and values among its ethnographic objects

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6 A comparative anthropology of (disciplinary) ethics might juxtapose academic philosophy’s professional ethics codes and practices with that of neighboring fields, the better to understand their respective social-relational conditions of possibility.
(but see Meskell and Pels 2005).

A COMPARATIVE PERSPECTIVE ON THE ETHICS OF RESEARCH

What might an “anthropology of” ethics look like that included anthropological and other research practices and professional discourses as objects of ethnographic attention? At the least, it would need to develop comparative understandings of the ethical underpinnings of disciplinary knowledges: treating disciplinary cultures critically as historically-contingent moral orders (e.g. following Sahlins 1981). Without a comparative framing, close attention to ones own discipline’s research practice can only offer primary data concerning “native points of view” that risks either over- or under-estimating the uniqueness of disciplinary practices and principles, and reifying their distinctive features.

RELATIONALITY AND CONTROL IN BEHAVIORAL AND SOCIAL SCIENCE

Malinowski’s foundational move “off the verandah” pointed the discipline in a direction that other social or behavioral sciences have rarely traveled. To adapt the anthropological trope, when sociologists and psychologists moved off the verandah, they took their work indoors. Most dramatically, psychologists adopted an experimental method modeled on physical science; in the same spirit, if less dramatically, sociologists adopted the interview as a privileged tool for controlling interactions between the researcher and researched so that the resulting data would be amenable to quantitative analysis.

Anthropologists’ social proximity to their sources, their method of opening themselves to being socially defined by the folks they aim to understand, appears to them as self-evidently edifying despite its ethical risks. But open-ended intimacy as a scholarly knowledge practice appears as uncontrolled, sloppy, and even perverse from the perspective of mainstream sociology and social psychology (Lederman 2006b, 2009). Practitioners in these fields have worked to limit and control the relational character of research encounters in the interest of objectivity, even when they are investigating human sociality.
What is more, both scientific (positivist) and interpretive anthropologists understand the epistemological value of personal engagement with others in social spaces they do not themselves control. In contrast, objectivist social science is committed to an apparently endless battle to socially decontamination the space of research. Its key device to this end is the researcher’s control over the information available to research participants.

Consider social psychology. Since the 1920s, when experiments expanded beyond testing the workings of memory and perception to tackle social problems of public concern, deception has been a central research tool. Incomplete or actively misleading communications, “technical illusions” in Stanley Milgram’s terms, were developed into elaborately staged scripts in the 1950s and 1960s. Milgram himself was an exemplar, famous and notorious for a series of literally shocking experiments concerning obedience to authority. These experiments have a direct bearing on the socio-historical understanding of everyday ethics. Inspired by events of the Holocaust, they explored how psychologically “normal” people could be induced to perform reprehensible acts. Milgram construed his experiments to support Hannah Arendt’s analysis of Adolf Eichmann as an “uninspired bureaucrat”:

> After witnessing hundreds of ordinary people submit to the authority in our own experiments, I must conclude that Arendt’s conception of the banality of evil comes closer to the truth than one might dare imagine. The ordinary person who shocked the victim did so out of a sense of obligation – a conception of his duties as a subject – and not from any peculiarly aggressive tendencies. (Milgram 1974: 6, original emphasis; see Arendt 1963)

Disciplined uses of deception made social psychology scientific in the same way as clinical trials involving placebo control groups, also ethically controversial, are the “gold standard” in biomedicine (Morawski 1988; Korn 1997; McQuay and Moore 2005). Despite five decades of controversy over its uses, deception continues to be “an integral feature” of experimental design: the American Psychological Association’s ethics code paragraphs on “informed consent” allows deception when experimenters “have determined
that the use of deceptive techniques is justified by the study’s significant prospective scientific, educational, or applied value” and that “effective nondeceptive alternative procedures are not feasible” (American Psychological Association 2010: §8.07a; see Lederman 2009). 7

The explicit justification for experimental misdirection is the need to control research subjects’ “reactivity”, their tendency to act as actual subjects who cannot help but devise interpretations of the circumstances in which they find themselves as bases for their behavioral choices. Instead, misdirection enables subjects’ behavior to be construed as responsive only to variables deliberately introduced. If subjects are not “naïve” concerning the study hypotheses, then they might capriciously work either to support or to frustrate experimental expectations. Either way, psychologists construe their data to be invalidated by research participants’ foreknowledge of the experimental situation and effort to enter into a relationship with the experimenter, however one-way. 8

While it may be counter-intuitive to anthropologists, the last thing an experimenter wants is to be surprised by unplanned, uncontrolled stimuli conjured into the lab by imaginative research participants. Psychological research design aims to simulate natural behavior, not to provoke it. Constructing convincing experimental environments enables researchers to make secure claims about the stimuli to which subjects are responding and generate results that are reproducible, therefore authoritative and credible (Orne 1962: 776; Adair 1973).

Toward that end, researchers also work to excise the personal in their

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7 The American Sociological Association’s (1999) ethics code also recognizes deception as “an integral feature of the design and conduct of research” and retains a place for it in its “informed consent” paragraphs (see also Lederman 2009). Whether exposing shrinded corners of society ethnographically or revealing unseen social patterns statistically, sociology is methodologically and ethically committed to the demystification of everyday personal experience. When anthropologists do this (e.g. Scheper-Hughes 2004), their work runs counter to mainstream disciplinary sentiment.

8 In other words, prior fully-informed consent is not technically possible in human experimentation because study validity depends on research participants’ ignorance of the study hypotheses. As the following section of this chapter will imply, prior fully-informed consent is not possible for participants in anthropological fieldwork either, but for quite different reasons relating to the emergent and intersubjective (even if not fully “collaborative”) nature of the field research relation.
research relationships. The fundamental condition of objectivity in social research, applying equally to psychological lab experiments and formal survey interviewing, is that variable results should not be functions of the idiosyncrasies of investigators and their relationships with subjects. Any investigators following the same protocol with the same categories of subjects ought to be able to replicate their colleagues’ results. That is, for research purposes, neither experimenters and nor their subjects are “persons”. A voluminous literature produced by psychology, sociology, and related fields reports and analyzes the difficulties encountered in approximating the ideal of non-reactivity. “Whole subfields” (Rosenthal and Fode 1963: 491) are devoted to devising strategies to compensate for “experimenter bias” and related distortions. These Herculean efforts to control the research encounter testify to the irrepressibly-sociable character of human sense-making.

Refining our distinctions: like anthropology, mainstream social and behavioral sciences study relations by means of relations; however, they do so by constructing specialized research relations with heavily managed, conventionalized expectations concerning contact with research participants. They construe undesigned relationality in the research situation as noise distorting reliable, objective results. Reversing figure and ground, anthropologists make knowledge by tuning-in what those others work so hard to tune-out.

**Relationality and Control in Sociocultural Anthropology**

Relative to sociology and psychology, a key distinctive feature of anthropological research conventions has historically been a deliberate relinquishment of control over research conditions. Moving from verandah to village (social network, boardroom, clinic), anthropologists enter domains controlled socially and culturally by the folks whose lives they hope to understand. Correspondingly, their motivating themes have typically opened outward toward unasked questions and opportunities presented by the contingencies of fieldwork. Unlike social scientists situated as experts with theories to test, anthropologists find culturally-appropriate ways of becoming their informants’ students or “apprentices” while they work to
acquire linguistic and social, and therefore ethical, competence. In that sense they are socially “one-down”, no matter what their structural position or personal style might be (Agar [198x] 1996; see also Briggs 1986). This applies both to recent fieldwork (e.g. in science labs) and to the classic kind famously exemplified by E.E. Evans-Pritchard’s rationale for becoming “cattle-minded” in Nuerland (although, certainly, methodological humility as a professional stance needs to be distinguished from the personal or socio-political kind; see M. Wax 1972).

Relatedly, the discipline has long valued collaborative relationships with research participants (Lassiter 2005: 26 ff; e.g. Majnep et al. 1977). In the past, explicit collaborations between anthropologists and their interlocutors appeared as relatively unique relationships (e.g. Franz Boas and George Hunt or Marjorie Shostak and Nisa). In contrast, the past generation’s refashioning of the disciplinary project has come closer to realizing the ethical possibilities of true partnerships, particularly as the principle has extended from individual relationships to whole communities (e.g. Lassiter et al. 2004).

Nevertheless, long-standing conventions of ethnographic writing and authorship, in which claims concerning anthropological expertise assert themselves over methodological humility, have qualified its ethical implications. They invest the scholar with explanatory or interpretive agency while filtering analogous (not to say opposed) creative forms of those being written about. They obscured the necessarily intersubjective character of anthropological practice: its dependence, so obvious during fieldwork, on the quality of anthropologists’ relationships with their interlocutors (Clifford 1983; Fabian 1983).

All that was given a sharp kick in the pants by Vine Deloria (1969), whose scathing satirical critique of Americanist anthropologists’ neocolonial relations was proclaimed an epochal calling-to-account in the AAA’s *Handbook on Ethical Issues in Anthropology* (Cassell and Jacobs 1987). Propelled by the same world-historical forces that made Deloria’s manifesto possible, over the past forty years the disciplinary structure of feeling has shifted. It is now normal (if not universally applauded) for anthropologists...
to recognize the intersubjectivity of fieldwork as the enabling condition, both ethically and epistemologically, of ethnographic production and to write that condition into their ethnographies. Not only do contemporary works open themselves to collaborative co-production, but they also acknowledge the contributions of quite untraditional interlocutors (e.g. Scheper-Hughes 2004; Holmes and Marcus 2008).

In the end, the thread connecting older and newer styles of methodological humility and the past generation’s critical impact may be anthropology’s typically self-directed or “true” irony (e.g. Boon 2001; Fernandez and Huber 2001; Robbins 2004; Lambek 2010). Whether one is studying “up” or “down”, whether aiming for apprenticeship, collaboration, or even exposé, the ethical valence of field relationships is not under the anthropologist’s control. Ethnographic quality may rest on how well writers grasp those relationships both from their own socio-material, cultural, and ethical perspectives, and from those of their interlocutors. Acquiring ethical, linguistic, and ethnographic competence takes time: it accounts for the “unbearable slowness of fieldwork” (cf. Marcus and Okely 2008) and it inevitably involves making mistakes. However, like other disciplines anthropology distinguishes between ordinary fumbling and the scandalous sort.

**SCANDALOUS RELATIONS AND OVERBEARING CONTROL IN ANTHROPOLOGICAL FIELDWORK**

Sociocultural anthropology’s public controversies have involved instances of systematic departure from the informal principle of relinquishing control and of serious straying from the disciplinary ideals of competent sociable engagement, backstage intimacy, and working collaborations. Accusations concerning the uses of deception resonate differently in anthropology than they do in neighboring disciplines because the discipline accords deception no conventionalized methodological justification. We might briefly revisit two examples mentioned previously.

**DARKNESS IN EL DORADO**

Napoleon Chagnon’s research has been at the center of cross-national anthropological controversy for decades, well before the crisis went public
with the publication of *Darkness in El Dorado* (Tierney 2000).9

As an anthropologist committed to sociobiological theory, Chagnon has long practiced a hypothesis-testing style of fieldwork. To answer his research questions, he had to gain access to kinship information that Yanomami normally consider private. When Yanomami deflected his questions with misleading responses, Chagnon did not adapt his project to his hosts’ constraints and openings. Instead, he found ways of manipulating their ethical conventions to induce individuals to reveal one another’s secrets and he used his knowledge of inter-group and inter-personal rivalries to gain access to information central to his research.

In effect, despite living in Yanomami communities during years of field visits, his style of research had more in common with field experiments in psychology, political science, and economics than it had with normative ethnographic fieldwork. Initial questions about whether he and his biologist collaborator had conducted experiments that intentionally harmed Yanomami health provoked particularly heated debate. While those accusations were laid to rest, their momentum was likely driven by the goodness-of-fit between scandalous imputations of active experimentation and the more passive forms of hypothesis-testing in which Chagnon appears to have engaged. Competent hypothesis-testing requires the scientist to manipulate and control research conditions. Given the nature of his manipulations, it could not help being judged problematic by the standards of mainstream anthropological ethics.

**ANTHROPOLOGISTS AS SPIES**

Several of anthropology’s most notable scandals have concerned the uses of anthropologists, anthropological publications, and anthropological identities as fronts for counter-insurgency intelligence work (see Winslow and Küyük infra). In these contexts, defending the ethico-political value of what is and is not “anthropology” can have practical bearing on the well-

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9 The following discussion can be read in provocative tension with D’Andrade and Scheper-Hughes (1995): a particularly sharp rehearsal of recurrent anthropological arguments over, among other things, the ethical/moral values of “scientific” and variously “engaged” anthropologies. For a longer account of this argument and its wider disciplinary (and extra-disciplinary) resonance, see Lederman (2005).
being of both anthropologists and those they study.

Both during the 1960s and recently, the AAA acted to distinguish disciplinary practice from government intelligence work. In 1965, public exposure of Project Camelot, a soon-abandoned US government plan for counter-insurgency operations in Latin America, prompted the AAA to commission a report (Beals 1967), form an ethics committee, and draw up its first ethics code. Project Camelot did not single out anthropologists for recruitment, but counter-insurgency operations in Thailand a few years later implicated their work more directly. Adopted in 1971, the Principles of Professional Responsibility asserted that “anthropologists’ paramount responsibility is to those they study. When there is a conflict of interest, these individuals must come first”, a statement widely understood to condemn deceptive interaction with field communities. As the Beals report put it, “the international reputation of anthropology has been damaged by the activities of individuals ... who have pretended to be engaged in anthropological research while pursuing ... intelligence operations”. It declared these activities “not anthropology” regardless of the credentials of those responsible.

This position was echoed 40 years later in the final report of the AAA’s Commission on the Engagement of Anthropology with the US Security and Intelligence Community (CEAUSSIC 2009). The report reviewed the Department of Defense’s “Human Terrain System” (HTS), originally set up to prepare teams of social scientists, including anthropologists, to work within battalions active in Iraq and Afghanistan. The report sharply distinguished anthropological ethical precepts and concept of culture from those in play in HTS documents. While the report was at pains to recognize the legitimacy of anthropological work within government security and intelligence settings, it concluded that the activities of Team members within combat units “is not anthropology” and ought not to trade on its intellectual or moral credibility.

CONCLUSION

Anthropology has a long history of interest in the “ordinary” or
“everyday” ethics of field communities: ethics has been a focus both of ethnographic description (e.g. in studies of religion, law, and exchange, and of colonial, gender, and class hierarchies) and of entwined methodological-reflexive concerns (e.g. in autobiographical and prescriptive discussions of fieldwork). Some recent contributions have worked to improve our understanding of the social embeddedness of ethics by drawing more widely on resources from across the subfields, including sophisticated integrations of linguistic and sociocultural anthropology. Pulling both with and against that, other recent contributions have sought to demarcate the anthropology of ethics/morality to intensify topically-focused ethnographic attention and debate; toward improving anthropology’s analytical resources, they have also seriously reengaged moral philosophy.

This chapter has suggested that while this work has usefully problematized “ethics”, it has tended to bracket “anthropology” and the contemporary contexts of scholarly practice. In particular, it has distanced itself from anthropology’s well-developed critical discourse on its own ethics (practices, codes, and controversies) and, more recently, both practical and ethnographic discourse on the regulatory and other non-academic environments within which research across the disciplines is embedded. A more inclusive anthropology of ethics/morality might integrate these themes.

Among other rationales for such an integration, ethnographic and comparative studies of the ethical structuring of disciplinary knowledges can illuminate how researchers with different professional training evaluate one another’s work on grant committees and Institutional Review Boards, in promotion cases, and in other contexts (e.g. Brenneis 2004, 2005); they have implications for collaborative research where outcomes depend on shared expectations about research design, data sharing, authorship, and the like. As well, a comparative understanding of the values orienting anthropological practice, making clear both its conjunctures and disjunctures with mainstream social science, can contribute to anthropologists’ public engagements in classrooms, mass media, and workplaces.
Social-science conventions are learned in school long before students arrive at university, and they are assimilated implicitly through a variety of media. Teaching field-research ethics is powerful evidence in particular of anthropology’s anomalousness; but this cannot be appreciated if one simply discusses codified principles or even cases with students. Every year without fail, when my students read about the historical rationales for IRBs and for the AAA’s ethics code, they denounce the use of deception in social research and applaud the principle of informed consent. Nothing appears amiss when they begin class field-projects, unless one keeps close track of what they are actually doing (or restricts them to conventional research styles, like interviewing). Every year without fail, half the class spends weeks “lurking” in their everyday identities – engaging in passive observation, eavesdropping, and interacting with people in familiar ways – rather than marking themselves in words and deeds as “anthropologists”.

Outing the anthropologist means articulating not just their curiosity but also their approach. They fear that the people with whom they hope to interact, misunderstanding themselves as research “subjects” and feeling like “lab rats”, will reject being “put under the microscope”. And quite reasonably: expectations concerning the objectifications of normative social science are challenging for novice fieldworkers to overcome both in their interlocutors and in themselves. With encouragement they find a route, from experience and one another, past the Scylla and Charybdis of subjectivity and objectivity, twinned conceits of researcher-controlled work. That route is to an intersubjective recognition made possible by the predictably unpredictable development of real-world relationships of varying quality and depth that are the hallmarks of anthropological participant observation.\(^\text{10}\)

The disciplinary histories of both Boasian and Malinowskian

\(^{10}\) Teaching students about IRB applications tends to undermine this fragile understanding: filling out applications and getting consent forms signed feel like accomplishments, whereas ethical fieldwork is always a work-in-progress. Novice fieldworkers are burdened with understanding both the distinctive values of anthropological principles in practice and the otherwise principled practices on which regulatory ethics are based, while also appreciating their mutual contradictions.
anthropology are replete with conflicts and crises. However, if we understand them as social projects rather than as congeries of individual careers, then we are in a better position to appreciate how, over the past century, they have created a distinctive approach. Anthropologists today are beneficiaries and custodians of this history’s specific value, by turns complementary and subversive relative to its epistemological neighbors. Whether serving particular communities, employers, and sponsors with activist or applied research, or contributing to a common human self-understanding with curiosity-driven research, anthropologists will continue to contend over the shape of disciplined intersubjectivity as a foundation for ethical fieldwork and persuasive ethnography.

NOTES

This chapter was drafted and edited in mid-2009. While its bibliographic citations reflect that history, I am very grateful to the editors for indulging my desire to update its argument and (to a much lesser extent) its citations at the end of 2011, as this collective project finally went to press.

REFERENCES

AAA: see American Anthropological Association


Brettell, Caroline (ed.). 1996. When They Read What We Write. Westport, CT: Greenwood Press.


