Jane Zavisca

Ethics in Ethnographic Fieldwork

Jane Zavisca
University of Arizona

Scholarship on the ethics of ethnographic field-work has flourished in the United States and the United Kingdom in recent years. In anthropology, a scandal emerged when a book entitled *Darkness in El Dorado* alleged that anthropologists working among the Yanomami people of the Amazon had violated human rights and caused a measles epidemic [Tierney 2000]. Debate continues over the fairness of these charges. The American Anthropological Association (AAA) ultimately rescinded its own report on the matter after the reporting commission itself was accused of unethical conduct in its investigation. The scandal inspired several edited volumes on ethics, including *The Ethics of Anthropology* [Caplan 2003], *Ethics and the Profession of Anthropology* [Fluehr-Lobban 2003], and *Lost Paradises* [Salzano, Hurtado 2004].

In sociology, Michael Burawoy's call for a 'public sociology' has invigorated discussions of ethics. Following Max Weber, Burawoy asserts that there can be no sociology without value com-

mitments, which cannot be determined by scientific methods. As Weber puts it: *'An empirical science cannot tell anyone what he should do — but rather what he can do — and under certain circumstances — what he wishes to do.'* Values set the scientific agenda: *'The very recognition of the existence of a scientific problem coincides...with the possession of specifically oriented motives and values'* [Weber 1949 [1904]: 54, 61]. Value commitments can be clarified by *'engaging publics beyond the academy in dialogue about matters of political and moral concern'* [Burawoy 2004: 5]. Public sociology thus articulates the ethical ends to which the instrumental means of professional sociology should be put. Debates about the possibility and desirability of this ethically-oriented research programme have been published in leading journals such as the *British Journal of Sociology* and *Social Forces.*

This latest wave of interest in ethics confirms Carolyn Fluehr-Lobban's claim that *'ethics in anthropology is like race in America — dialogue takes place during times of crisis'* [2000]. Indeed, the AAA adopted its first formal resolution on ethics, 'The Principles of Professional Responsibility', in 1971 in response to collaboration between anthropologists and the US military during the Vietnam War. The code declared that anthropologists had to put the interest of those they study first [Mills 2003]. Sociologists, by contrast, voted two to one against a proposal that the American Sociological Association (ASA) should condemn the Vietnam War in 1968. By 2003 sentiment had changed: two-thirds of the ASA membership supported a resolution against the Iraq war. Burawoy cites this turnaround as evidence for an increasing public ethos among sociologists in a nation *'governed by a regime that is deeply anti-sociological in its ethos, hostile to the very idea of "society"'* [Burawoy 2005: 7].

Perceptions of both disciplinary and societal crisis are also inspiring interest in ethics among Russian social scientists. Ethical issues repeatedly arise in the *Forum for Anthropology and Culture*'s 2006 issue on the 'Subjectivity of the Researcher'. In her contribution to the volume, Nancy Scheper-Hughes argues that the anthropologist has distinctive ethical responsibilities beyond impassive observation: *'The anthropologist as witness is accountable for what she sees and for what she fails to see, how he acts and how he fails to act, in critical situations.'* Pretensions to total objectivity impede reflection on the ethics of both the means and ends of social research by leaving the author completely out of the text, according to Sergei Abashin, who is dismayed that there is only paltry debate over *'the relationship between academics and the political establishment, how to come to terms with the Soviet experience, the fight with xenophobia, the relationship between academic work and ideology, and so on.'* Likewise,

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Elza-Bair Guchinova questions how Soviet ethnography may have served Soviet power, and bemoans the contemporary 'flourishing' of colonial intellectuals who express contempt for the people they study while pandering to Western scholars.

Although discourse on ethics escalates during crises, ethical problems are always present in fieldwork. Hopefully a dialogue on ethics in Russian ethnography will endure after the current sense of crisis passes. In this essay I describe institutional, scholarly, and personal responses to several common ethical quandaries. I begin by reviewing the history of the formal regulation of ethics in American universities. These processes of 'institutional review' are important both because they are an inevitable stage of research in many countries, and because critiques of these institutions have stimulated discussions of ethics in practice. Next, I explore ethical dilemmas that have inspired sustained scholarly reflection, and which I encountered in my own ethnographic fieldwork in Russia. These include: informed consent, justification of research agendas, duties to research participants, and duties to oneself. For each issue, I review published case studies of ethical quandaries, the most frequently cited of which involve life and death consequences. To show how analogous ethical problems arise in less dramatic circumstances, I supplement the discussion with examples from my own fieldwork, during which I believe no lives were risked, but feelings and reputations were.

I am an American sociologist and a recent Ph.D, so my perspective is limited by my corresponding dispositions and experiences. My research concerns the legitimation of consumer inequalities in Russia, where I conducted fieldwork in the city of Kaluga during 2001 and 2002. Data collection consisted of ethnographic observation of several families whom I befriended (with a focus on shopping, entertaining, and dacha cultivation as sites for participant observation), as well as several dozen semi-structured interviews and a mass survey of 1000 consumers in Kaluga. My capacity to conduct this research as a graduate student had as much to do with my position at a prestigious American university (the University of California at Berkeley) and the dollar-to-rouble exchange rate as with the potential scientific merit of my research.

Institutional Regulation of Ethics in Academic Research

The U.S. government requires all universities that receive federal funding to establish 'Institutional Review Boards' (IRBs) to ensure that researchers are following the rules of ethical conduct that are specified in laws concerning the 'protection of human subjects.' Although the system was set up primarily with biomedical research

in mind, laws on the protection of human subjects also apply to social and behavioural research [Singer & Levine 2003]. Institutions that fail to comply with the law risk suspension of all federal grants to a university, a sanction that has occasionally been levied (usually in cases of harm or death during medical research), and which universities are loath to risk due to their heavy dependence on governmental funding.

The template for contemporary IRBs and associated legislation can be found in the 1979 'Belmont Report' to Congress, which was commissioned under the National Research Act of 1974. The Belmont Report references two notorious cases of abuse of human subjects in calling for the formal adoption and enforcement of these principles in research. First, the Nuremburg War Crime Trials had revealed horrific medical experiments conducted on concentration camp prisoners by Nazi scientists. Second, the Tuskegee Syphilis Study observed rural African American men with the disease from 1932–1972. Scientists interested in the natural progression of the disease neither informed study participants that they had the disease nor offered them treatment. To this day African Americans cite the Tuskegee study as a reason they are reluctant to volunteer for medical research [Freimuth et al 2001].

The report proposes three ethical principles that should guide research: (1) respect for persons, (2) beneficence, and (3) justice. *Respect for persons* means that people have the right to choose whether to participate in research. 'Informed consent' is possible only if potential participants understand that participation is voluntary and are told about the research in language they can comprehend. Vulnerable persons with diminished capacity to give informed consent such as children, prisoners, and poor people must be protected from exploitation. *Beneficence* is defined in terms of the principle 'do not harm,' by maximising possible benefits and minimising risk to both participants and to society at large. *Justice* requires answering the question: 'Who ought to receive the benefits of research and bear its burdens?' Subjects of a study should be selected equitably, such that no one group disproportionately bears the risks or benefits of participation.

The Belmont Report strongly influenced both the formulation of federal law and the adoption of ethical guidelines by disciplinary associations. The ethics codes of both the AAA and the ASA explicitly refer to the three guiding principles. While few would disagree with the Belmont Report's basic principles, there has been considerable controversy over how to apply the principles to social sciences research. Social scientists often complain that they have been wrongly incorporated into a 'medical model' that is inappropriate for their research. There is a pervasive sense of increasing

intrusion and inappropriate levels of surveillance over social science practices. Even graduate students are socialised early into a culture of mistrust: first year graduate students in my research design class are eager to talk about ethics, but roll their eyes and sigh at any mention of IRB.

There are two different types of criticisms of IRBs' relationship with social science. First, IRBs are said to obstruct and delay research by overzealously applying protocols that are more relevant to medical experiments than to interviews and unobtrusive observation. One commentator in Canada, which has a similar oversight system, reports an absurd case in which an ethics committee *'informed a graduate student that she should look away when her participant observation research brought her into contact with individuals who had not explicitly consented to being studied'* [Haggerty 2004].

The second argument against IRB protocols is that, while well-intentioned, they do note ensure that research will actually be carried out in an ethical way [Bosk & de Vries 2004]. Many of the real ethical issues that arise during fieldwork are never raised in formal protocols that are approved, filed away, and largely forgotten. At their worst, IRBs protect institutions and scientists from litigation, but do not protect participants from harm. Indeed, formal codes can actually inoculate researchers from the ethical issues inherent in a human science: *'professionalization and decontextualization of ethics reinforces the ideals of science as a politics-free zone, ideals to which its members are expected publicly to aspire'* [Mills 2003: 44]. Both of these types of criticisms can best be illustrated through examples of ethical dilemmas faced in the course of fieldwork.

**Informed Consent
in Ethnographic Research**

Formal Consent

No issue has led to more accusations of IRBs being either obstructionist or useless than formal procedures for establishing informed consent. The medical model of human subjects protection requires research participants to sign a formal statement documenting their comprehension of the methods, goals, and potential risks and benefits of the research, and confirming the voluntary nature of their participation. Such formal procedures are obtrusive in ethnography, as is illustrated by the directive cited above to 'look away' when the paperwork is not in place. Classic ethnographic works in anthropology and sociology would never have been approved by today's IRBs. The methods appendix to William Whyte's book *Street Corner Society* remains canonical in sociological courses on field methods, not least for its sustained discussion of ethical matters

[1981 (1943)]. He would have had a difficult time establishing rapport with the working class youth of his study if he had had to whip out a consent form every time he met someone new or began discussing an unanticipated topic during three years of hanging out with them.

Today, many ethnographers do not strictly follow the IRB protocols to which they agree, since their research would be impossible if they did. Mitch Dunier, a sociologist who wrote the award-winning book *Sidewalk*, on homeless book vendors in New York City, did not walk around with consent forms on hand, IRB protocols notwithstanding. Rather, he took steps that he thought would better meet the spirit of informed consent: he told participants what was doing and protected their confidentiality by storing his notes out of state (and away from police). He did not ask them to sign consent forms until after he had written the book manuscript, read the relevant passages to participants, and asked if they were willing to appear in the book [Shea 2000].

The discrepancy between formal requirements and field realities 'turns everyone into a low-level cheater,' according to sociologist Ann Swidler [Shea 2000]. In many field sites asking participants to sign consent forms would not only impede data collection, it could actually impose harm by violating local norms of what it means to give consent or to feel protected from harm. The AAA's 2004 'Statement on Ethnography and Institutional Review Boards' charges that IRBs often assume that all research resembles clinical research in an American cultural context. *'It is often not appropriate to obtain consent through a signed form — for example, where people are illiterate or where there is a legacy of human rights abuses creating an atmosphere of fear, or where the act of signing one's name converts a friendly discussion into a hostile circumstance.'*

I encountered this problem in my own research. It seemed ludicrous to expect that asking Russians to sign a consent form would offer them protection from harm. After all, the signature would constitute evidence of their participation when I had just promised them confidentiality, and what credible legal recourse would this form really provide them if they felt I had violated their rights? I asked other graduate students and faculty who had recently worked in Russia what they did about this. All told me that the solution is to 'say one thing and do another.' Apparently, everyone filled out the IRB protocols as if they were going to use consent forms, but no one actually used them.

This arrangement seemed absurd. After combing the text of the regulations for a loophole, I found that exceptions are permitted when three conditions are met: 1) the consent document itself could

pose a *'potential harm resulting from a breach of confidentiality'*; 2) written consent would not normally be required in the setting outside the research context; 3) the risk of harm to subjects of participation is minimal. In my application to the Berkeley IRB, I used these three principles to argue that oral consent was more culturally appropriate because a signature could be construed as a breach of confidentiality, there is no comparable human subjects protocol in Russia and respondents would (correctly) perceive their signature as offering protection to me but not to them, and the focus of my research was on matters of everyday life that Russians openly discuss. The Berkeley IRB did grant me this exemption, but they required me to write up a script that I would read to respondents in order to obtain oral consent. I did so, but in truth I never read that document verbatim to participants in my research — rather, I made sure to communicate conversationally what my research was about, how I intended to use it, and to encourage participants to stop me at any time if they no longer wished to participate.

Sustaining Consent

Formal consent, whether written or oral, does not really ensure that consent in long-term ethnographic research is either informed or voluntary. Gaining sustained consent is both more difficult and more important.

Formalising consent to a piece of paper or a formulaic recitation destroys what it is intended to protect. It does this, first, by breaching interpersonal etiquette because it questions the trust that must underlie the interpersonal relations on which the research is based. In other words, it removes the right of local people to consent to the research in the way in which they believe their consent ought to be given: by their intimacy and their participation with the anthropologist's work. Second, given that anthropological fieldwork is long-term and on-going, over months and even years, formal consent — whether written or verbal — at one point in time removes people's right to withdraw consent at a later time, and to deny their past involvement, if they later wish to do so... Thus, it is unwritten and non-formalised verbal consent which best typifies participant observation in anthropology. It means that people's consent must be renewed each day — through their continuing interaction with the researcher and the project, through their help, co-operation and assent...Phrased another way, anthropological research can only take place in the light of informed consent — given continuously, openly and graciously because we are behaving, and have behaved, properly [Silverman 2003].

How can ethnographers verify ongoing consent? I will illustrate the ethical ambiguities through two examples: economic asymmetries, which call into question whether consent is voluntary, and the eth-

nographer's role as friend, which can blur what kinds of revelations are informed.

In *Street Corner Society*, Whyte writes that Doc, the key informant who helped him gain access to the youth groups he was studying, warned him not to try to 'buy his way in', for he might be perceived as a snob and fail to establish the rapport that his research required. On the other hand, to establish friendly relationships as a participant observer, he had to participate in a moral economy based on continual exchange of favours. His solution was to try to follow the norms of the group itself, giving small gifts and exchanging favours that were typical in the setting, but not lending large sums of money or providing forms of help that would create a relationship of dependence.

This seems to me a reasonable strategy ethically as well as practically. Human subjects protocols instruct that payment (in cash or in kind) for participation must not be so extravagant as to serve as a form of coercion. When people are desperate for income or medical care, they may participate in research that they would refuse if not for the incentive. The principle of beneficence suggests that participants do deserve to be compensated somehow when they volunteer their time, their stories, or their bodies for research. I once worked as a research assistant on a survey on sexual networks and HIV in Uganda. The investigators determined that no amount of money could be construed as non-coercive because people were so poor. They settled on offering gifts of a bar of soap and a Polaroid snapshot of oneself to thank participants.

When status asymmetries are extreme, there is probably no avoiding inequality in the process of establishing consent. Residents may perceive the fieldworker as a potential source of income or influence, and they may choose to participate in hopes of assistance, regardless of the intentions of the researcher. Xin Liu experienced this problem in his ethnography of rural China: in one potential field site, he could not dispel the impression that he *was 'someone who had the potential to bring future business to the community... I was hoping to find a place where I could participate in the everyday life of ordinary families. I left Li Zuang the following morning. This trying experience made me begin to realise that even for native scholar studying his own society, choosing a field site was highly problematic'* [Liu 2000]. He resolved this dilemma by moving to a site where he had more informal connections — the problem dissipated with time as his presence became unremarkable, but never fully disappeared.

I had similar problems in my own fieldwork in Russia, which were further complemented by status asymmetries with the professional

scholars upon whom I relied in my survey research. In short, without a Ph.D, I was lacking in cultural capital, but had a lot of economic capital due to the exchange rate. I had won a dissertation research grant that was barely enough to live on by American standards ($20,000), but enough to pay for substantial research assistance in Russia. I had a provisional agreement with the director of the Kaluga Institute of Sociology to pay him and his staff to help me field a large survey of 1000 consumers in Kaluga. Six months elapsed between our agreement and my arrival in Kaluga to start the fieldwork. During that period, the director had become interested in cosmology and most of his professional staff had left to found a market research firm. When I learned from the secretaries and students that he had alternative plans for my grant — he was going to 'download my data from the cosmos' rather than conduct the fieldwork that I had planned — I reneged on our agreement.

Although I felt manipulated and cheated by him, our final confrontation left me feeling that I was also in the wrong. He reminded me that I was a graduate student with no doctorate and no publications. As he put it, I was arrogant in thinking that anyone of his stature would work with me if I did not have money to throw around. It was inconceivable that a Russian graduate student could receive such a grant to go and study Americans and expect a full professor to serve as a glorified research assistant. I took my grant elsewhere to work with people whose dispositions toward research were closer to mine, at the Institute of Sociology at the Russian Academy of Sciences. There I assumed the role of apprentice rather than contractor of services. My collaborators at RAS profited little and gave me a deep discount based on shared scientific interests and the prospect of longer term collaboration as my career develops. Still, I was constantly aware that my ability to play the role of apprentice depended in part on my ability to buy my way in. This rather extreme, early field experience attuned me to the fact that my economic power and status as an American gained me certain forms of access I would otherwise not have, even in the course of daily interactions with people I was not paying. I felt that the best I could do was to be honest with myself and others about that inequality, rather than to try to mask it.

This leads me to another dilemma in fieldwork — the extent to which consent is truly informed. Like many ethnographers, I developed close friendships while in the field. The resultant 'role ambiguity' made taking field notes and writing up results ethically problematic. Did an informant who is also a friend reveal a confidence with the full awareness that it could wind up in print? Promising confidentiality does not fully solve this problem, since in extended ethnographies, it is often impossible to disguise identities in

communities where researchers and participants are well-known, without hiding attributes that provide important contextual information for analysis.

Jayne Howell presents a compelling case study of the ethics of friendship in fieldwork in her article on rape in Oaxaca, Mexico. Several of her friend-participants confided intimate and disturbing stories of rape during the course of ethnographic fieldwork that was not specifically focused on sexual violence. Howell felt compelled to write about this because the data offer insight into an important problem that is rarely discussed. Friendship was requisite to gain access to such intimate information, since rape is highly stigmatised in the community in which she worked. Thus her informants would have risked major psychological and even physical harm if they could be identified. As she sought consent to publish these stories, Howell decided not to disclose contextual details that would have been analytically revealing (in particular the professional status and family structures of the women involved) because the protection of confidentiality had to be paramount. Such circumstances surely arise in nearly any extended ethnography. Howell presents a model for ethical practices by both confirming consent before publishing sensitive material, and putting participants' interests ahead of her research agenda [Howell 2004].

Circumventing Consent

So far, my discussion has been restricted to cases where the researcher agrees to the principle of informed consent — the ethics are in the details of how to offer and maintain it. Some ethnographers engage in covert research, arguing that consent is neither ethically necessary (because of the public nature of a setting or the high status of the participants) nor methodologically sound (because the data could never be collected without secrecy). Sociologists tend to be more open to covert research than anthropologists. Perhaps this is because in the contexts in which anthropologists have traditionally worked, covert research is impossible because an obvious outsider cannot go undercover. However, it is possible to disguise one's identity when studying one's own culture. For example, sociologists have conducted covert ethnographies in workplaces, hospitals, religious cults, and public parks.

Several high profile cases have contributed to the ethical turn against covert research. The two most widely cited studies in social science are sociologist Laud Humpreys' *Tearoom Trade* [1970] and psychologist Stanley Migram's *Obedience to Authority* [1974]. Both were conducted before IRBs were widespread. In Tearoom Trade, Humpreys studied the phenomenon of anonymous gay male sex in public places. He played the role of the 'watchqueen,' who looks

out for other people, especially police, approaching the public restrooms where such sex takes place. He was open with some but not all of the participants whom he observed. He recorded the license plates of those who were reluctant to speak with him, followed them home to find out where they lived, and then months later visited them at home disguised as a health-service interviewer to find out more about their domestic lives. The men in his study were not the sexual predators of the public's imagination — they did not make advances towards teenage boys or uninterested adults. The majority of his subjects were otherwise conformist married men who did not identify as homosexuals. Many social scientists and journalists condemned the research as unethical for its deception and invasion of privacy. The controversial book also won many awards, and the author became a hero to many gay rights advocates for legitimising gay and lesbian studies.

In another famous study employing deception, Stanley Milgram recruited students to play the role of teachers who were to administer electric shocks of increasing intensity to learners. They were told that the purpose of the study was to find out how punishment affects learning. In reality, the 'learner' and the 'scientist' in the laboratory were actors in a ruse to find out how far participants would go in inflicting pain on an apparently suffering victim in the presence of an authority figure who insisted that the experiment must go on. Milgram's motivation for the experiment was to understand how the Holocaust was possible — during the Nuremberg trials, defendants repeatedly asserted that they were merely following orders. These experiments demonstrated that obedience to authority varied with the proximity of the authority figure (for example, students would inflict more pain if a scientist in a lab coat stood in a room than if he simply gave orders over an intercom). Although his work had evident scientific value — many psychologists were surprised by the findings, believing that only a few sadists would administer the 'maximum voltage' — some condemned it for the harm it inflicted on participants, many of whom experienced psychological distress during the experiment. Upon being surveyed later, however, most participants later reported that they were either glad or neutral to have participated in this landmark study.

Today's IRB's would likely not have approved either of these studies. However, covert research still goes on, particularly by sociologists. As an obvious foreigner, Michael Burawoy could not go undercover in his ethnographic studies of factories in Russia and Hungary. But during earlier research in Chicago Burawoy got a job at a factory and took fieldnotes without informing management or workers of what he was doing. His work has become a model for

a generation of workplace ethnographies [for recent covert work-place ethnographies by Burawoy's students see Chun 2001 and Sallaz 2002].

The typical justification of covert research given to IRBs is that these are 'public settings' in which there is not a reasonable expectation of confidentiality. Sociologists tend also to be less averse to covert work when they can justify it as 'studying up' and revealing exploitation. That is, if the major risk of harm is borne by corporate owners or managers, the research is seen to be justified. In Burawoy's estimation, the critical issue is not merely whether research is covert or overt, but the relationships of domination that ethnography has the potential to disclose or exacerbate [2000].

In other fields, however, covert ethnography is nearly universally frowned upon. For example, I recently discussed covert studies with a professor of education who conducts ethnographies in schools. She was shocked to learn that anyone conducts covert ethnography, saying that she couldn't believe any IRB would approve such an unethical practice. Human subjects protocols classify children as a vulnerable population incapable of giving truly informed consent, which means that parents, teachers, and administrators must give consent for them. However, upon reconsidering the ethics of the matter, she suggested that children may actually be more subject to harm if covert research is not conducted. School administrators, when informed of the presence and purposes of a researcher, have the opportunity to transform their schools into 'Potemkin villages' and present them to researchers in an idealised light.

Of course, there are many shades of gray between full disclosure and covert research. Does any ethnographer ever truly reveal all of the questions and purposes motivating their research? Whyte provides an account of the problem of trying to explain too much. At first, when asked what he was doing in Cornerville, he would launch into a long-winded explanation of his theory of social history. *'When I had finished, there was an awkward silence. No one, myself included, knew what to say. While this explanation had at least the virtue of covering everything that I might eventually want to do in the district, it was apparently too involved to mean anything to Cornerville people. I soon found that people were developing their own explanation about me: I was writing a book about Cornerville....Whether it was a good thing to write a book about Cornerville depended entirely on people's opinions of me personally'* [1981 (1943)].

Whyte decided to stop trying to reveal his full agenda because it was not appropriate in his field setting. Others may choose not to reveal their full agendas in the interest of scholarship and activism. For example, in an ethnography of campaigns for and against con-

structing a border wall in El Paso to prevent illegal immigration from Mexico, Tim Dunn reports that he adopted '"*adversarial methods" for researching bureaucratic power structures'*. For Dunn this meant actively participating in organisations opposed to the wall (which was eventually built). However, he tried to stay behind the scenes in his involvement so that he would be able to interview Border Patrol officials, who might not have consented to being interviewed had he been open about his own politics [Dunn 2003]. Most American ethnographers would, I believe, sympathise with this ethical stance because they sympathise with Dunn's politics. However, I suspect that if he had taken the opposite approach — collaborating with border wall supporters, and hiding those politics while interviewing the opposition, he would have been taken to task for violating the principle of informed consent. In short, the ethics of informed consent are intertwined with evaluations of the ethical ends that motivate research.

Ethical Intervention in the Field

Whether and how to gain informed consent is but one of many ethical decisions ethnographers must make in the field. According to the principle of beneficence, ethnographers should conduct themselves so as to minimise harm and offer some benefit to participants, all the while striving to collect data that will produce the knowledge that justifies the ethnographer's presence. A case study by Steven Vanderstaay dramatically illustrates the issues at hand [2005]. He sought to understand how involvement with the criminal justice system influenced the schooling of teenage criminals. His primary informant was Clay, a teenager who had been convicted of stealing a car. Vanderstaay formally interviewed Clay and also hung out with him and his mother. The researcher, not wanting to reinforce the teen's criminality, steered conversations away from criminal activity to focus on education and other matters of daily life. He felt ethically obliged to offer his informant something in exchange for his participation. He offered Clay $10 an hour for the time they spent together, but with the provision that the money would go directly to the court to cover payments Clay owed for his property crimes. At one point Clay's mother asked Vanderstaay to give $100 of Clay's 'salary' to his grandmother to pay the utilities bill, since they were behind on payments and their electricity had been turned off. Vanderstaay agreed. Clay then took the money from his grandmother and used it to set up a drug deal which ended with him shooting and killing two people. Vanderstaay was so distraught over the unintended consequences of his actions that he published nothing on the research for a decade, after which time he decided that it was selfish not to publish, since he was not helping anyone (other than his guilty conscience) by keeping the incident a secret.

Clearly, researchers' attempts to help may wind up harming the intended beneficiaries. At the same time, not intervening may also constitute harm. That was my feeling during my fieldwork in Kaluga when I repeatedly heard anti-Semitic statements, which often followed questions about my own ethnic heritage. My mother is a Polish immigrant who was born in a German refugee camp just after World War II and came to the United States in 1950. If I left out my religious background, the conversation would usually stop there. However, if I said that my family is Catholic (which is why they were not exterminated), some people would interpret our pan-Slavic heritage as a green light to share xenophobic sentiments, for example by saying that Slavs need to stick together against the Jews, who are running and ruining the world. Although these conversations made me uncomfortable, at first I tried to remain neutral, reasoning that it was my role to observe and not to try to change people. However, I feared that my silence could imply agreement and make matters worse, since my friends and acquaintances often reference me as an authoritative source on what Americans think in general (being the only American other than a Mormon or Peace Core activist many have ever met).

I decided on two courses of action. First I would flatly state that I disagreed with such sentiments when I heard them. Second, I started keeping notes on anti-Semitic statements, which, while not directly relevant to my work, I felt were important to record. I wrote a memo summarising my notes and gave it to the organisation Jewish Family and Children's Services (JFCS) in the United States — something I did not plan ahead of time, did not write into my IRB proposals, and for which I did not obtain informed consent. JFCS published the memo in their newsletter and used it to supplement their evidence on the persistence of anti-Semitism in Russia as a basis for Russian-Jewish refugee status in the United States.

'The field worker cannot afford to think only of learning to live with others in the field. He has to continue living with himself' [Whyte 1981 (1943): 317]. Of course, even the most well-intentioned, principled actions in the field may have unintended, harmful consequences. But is this not true of all social action, in or out of the field? The dangers that ethnographers pose to participants in their research often already exist in the participants' environment precisely because these are studies of daily life. As the AAA statement on the ethics of ethnography argues: 'Just as in daily life, in these natural settings of research there may be a high probability of risk, but the magnitude of such harm, like uncertainty, mild embarrassment, or boredom, is usually low. There are, however, instances where the possible magnitude of harm could be high, often in conflictive environments.' The

benefits to participants are usually also relatively low-grade: the satisfaction of having someone take an interest in one's life, and the sense that one is leaving a small mark on the historical record. Who does and should benefit from social research? I next turn to the issue of justice.

'Who Needs This?' Justice and the Ends of Ethnographic Research Human subjects protocols suggest that justice lies in a probabilistically 'equal opportunity' to participate in research, with all its risks and benefits. But justice is better conceived of in terms of the value orientations of researchers, and how they answer, for themselves and others, the question: 'Who needs this research?' In her contribution to *Forum*, Nancy Scheper-Hughes argues that the proper role of anthropologists who are *'privileged to observe human events close up and over time'* is *'to name and to identify the sources, structures, and institutions of mass violence'*. This demands the often painful work of revealing how *'the oppressed turn into their own oppressors or, worse still into the oppressors of others'*. Anthropology must thus reject cultural relativism and recognise *'the primacy of the ethical'*. In a similar vein, Carolyn Fluehr-Lobban argues that *'when there is a choice between defending human rights and defending cultural relativism, anthropologists should choose to protect and promote human rights'* [1995]. Both accuse anthropology as a profession as having too long resorted to a morally bankrupt neutrality in refusing to speak out against widespread cultural practices of violence, particularly on the basis of gender, age, or ethnicity.

What does this mean in terms of research practice? First, it suggests that calls to treat participants as full collaborators in research are misguided. 'Good' research (in the sense of both ethical responsibility and scientific validity) requires that scientists understand that scientific and the commonsensical point of views cannot and should not be collapsed. Pierre Bourdieu calls for two epistemological breaks in social research, both of which have ethical implications. The first is a break with the objectivist position that treats social realities as Durkheimian 'social facts' that are opposed to commonsense understandings of the world. Representations of social relations are themselves a part of social reality that must be analysed. The second break is with subjectivism, which erroneously equates agents' representations with reality and fails to analyze the objective conditions that structure subjective representations. Agents collude in their own oppression by misrecognising it, rendering their standpoint inferior to that of the refflexive researcher if the goal is to reveal social relations in hopes of changing them. Thus treating participants as full collaborators will not necessarily lead to better understanding [Bourdieu 1990].

Second, witnessing is not inherently a form of activism. Scheper-Hughes and others may overestimate their own power to change a situation simply by serving as active witness to it. Whether 'naming and identifying' the sources of mass violence changes it depends on the position of the scholar in the academic field, and of the relationship of the academic field to the field of power — that is, the ability of social science to command an audience that matters for the issue at hand — a relation over which the individual anthropologist often has little control. To behave ethically in the field, the researcher must strive to be honest with herself and the participants in her research about what she can realistically expect to achieve.

In preparing to write this piece, I reviewed much of the recent work published by anthropologists and sociologists about post-socialist transitions, since that is my area of expertise. I was surprised about how little most of this work had to say about ethics in the practice of research. Writers devote far more attention to justifying the value-orientations of their research questions and theoretical frameworks than to discussing the ethics of their means, that is, their personal conduct during the process of data collection.

The central ethical agenda of post-socialist ethnography by researchers from the U.S. and the U.K. appears to be a struggle against the political agenda of 'transitology.' Transitologists presume that capitalism is both a desirable and inevitable consequence of the collapse of socialism, and attribute social ills and difficulties to deviations from the correct path to capitalism. Qualitative researchers (including myself) seek to show the effects of post-socialist transformations on the ground by documenting the struggles of daily life. As Burawoy and Verdery put it:

A focus on the day-to-day realities of post-socialism reveals a much more ambiguous account of the transformation announced with such fanfare by theories of modernization and of market and democratic transition... In the ethnographies that follow, we try to show that the conventional metaphors — extinction, genesis, incubation, and legacy — are limited because they give insufficient integrity to the creative and resistive processes of everyday practices. Indeed, we find time an time again that every step forward in the direction of the market produces forces opposed. In reaction to the iron law of market expansion, we discover the iron law of market resistance [Burawoy, Verdery 1999].

I fully support the ethical and intellectual goals of studying capitalism on the ground, without reifying it as an ideal system. Although I am not terribly confident in the power of ethnography in today's world to transform it, I believe it is important to produce alternative accounts of capitalism to those offered by the architects of

shock therapy. However, there is a danger in being overly optimistic about ethnography's potential to effect change, particularly in how we present potential benefits to the subjects of our research.. Telling the stories of the vulnerable does not make one ethically invulnerable. I am reminded of a young graduate student and former peace core volunteer who wants to do ethnography in Uzbekhistan. When I asked him why, his response was, *'These people are suffering, teachers are living on $50 a month, and I want to do something about it!'* I gently told him that while it is not inherently wrong to study suffering, studying something is not necessarily 'doing something about it'. Attempts at witness notwithstanding, the inequality and degradation that accompany the institutions of capitalism tend to march on with or without ethnographic accounts.

One of the most difficult questions I faced in the field was: 'Who needs this?' (Komu eto nuzhno?), which I was often asked when I described my research or asked people to participate. To me, this question cuts to the heart of all of the ethical challenges of achieving consent, beneficence, and justice. I would have liked to have been able to tell potential participants that they themselves need this research, that social justice demands that the world know their stories. But I knew that documenting people's suffering was unlikely to change it, at least not directly, and not any time soon. To the question 'Who needs this?' I gave the most direct and honest answer I could: 'I do.' In the short term, I needed to write my dissertation, and they could help me do it. They might get something small out of it too — most of the people I interacted with seemed to enjoy the opportunity to talk about themselves, and got some satisfaction out of showing kindness to a stranger. In the longer term, I told them, this research might possibly be useful for scientific and historical understanding. As a 'history of the present' during a time of rapid change, perhaps future generations might find what I had to say interesting. But, I offered no hope that this research would help them directly.

Conclusion

My goal in this article has been to review ethical problems in ethnography from the standpoint of an American sociologist. I do not have specific recommendations for what Russian ethnographers should do or on whether and how Russian institutions should institute formal ethical oversight procedures. I began my discussion of IRB by stating that there are two types of critiques: those who say formal ethics reviews obstruct and impede ethnographic research, and those who maintain that they do not prepare researchers for the ongoing ethical dilemmas that occur in and out of the field, long after the planning stages. I fall into the latter camp. I have been able to do most of what I want within the confines of

IRB procedures, and so have most other American sociologists I know. Yes, my research was delayed by the IRB process, but I think it is reasonable and desirable that institutions that sponsor research should conduct ethical oversight. However, the formal pause that researchers take to write up human subjects protocols is a mere hiccup in the ethical practice of ethnography.

In Vanderstaay's account of his devastating field experience, he regrets that he did not have more exposure to ethical debates and case studies prior to entering the field. Human subjects protocols are not the solution: 'Given the importance of small details within ethnographic contexts, no exact recipe for ethical fieldwork can be written.' Vanderstaay attributes his ability to negotiate the minefield of ethical dilemmas he did successfully address, before everything went horribly wrong, to having read other researchers' accounts of ethical dilemmas in fieldwork on crime. Certain ethical decisions can and should be made before entering the field, but many must be made on the fly, and this will be easier to do to the extent that the researcher has thought about ethical puzzles encountered by others. Vanderstaay recommends that scholars build up a larger body of ethical case studies to *'enable researchers to anticipate difficulties and to establish useful guidelines before entering the field'* [2005]. This strikes me as an important project for social scientists in any nation; I hope this article will stimulate more such work by and for Russian ethnographers.

Abbreviations

AAA   American Anthropological Association

ASA   American Sociological Association

IRB   Institutional Review Board

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