

Bureaucracies of Mass Deception: Institutional Review Boards and the Ethics of Ethnographic Research

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Ethnographers have long been unhappy with the review of their research proposals by institutional review boards (IRBs). In this article, we offer a sociological view of the problems associated with prospective IRB review of ethnographic research. Compared with researchers in other fields, social scientists have been less willing to accommodate themselves to IRB oversight; we identify the reasons for this reluctance, and in an effort to promote such accommodation, we suggest several steps to reduce the frustration associated with IRB review of ethnographic research. We conclude by encouraging ethnographers to be alert to the ways the procedural and bureaucratic demands of IRBs can displace their efforts to solve the serious ethical dilemmas posed by ethnography.

Keywords: research ethics; institutional review boards; ethnography; research methods

One score and seven years ago, the National Commission for the Protection of Human Subjects of Biomedical and Behavioral Research met in February to identify the ethical principles that would serve as guidelines for the protection of human subjects of biomedical and behavioral research. The discussions from those meetings culminated in *The Belmont Report*. In

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the summer of 2003, The Social and Behavioral Sciences Working Group on Human Research Protections convened a conference at, fittingly enough, the Belmont Conference Center. The title of the conference was *IRB [Institutional Review Board] Best Practices in the Review of Social and Behavioral Research*; the working group invited a collection of persons—with “considerable expertise in human research protections and the review of social and behavioral science protocols”—to join them at the Belmont Center. Topics for the various sessions of the conference included “IRBs that Work”; “Back to the Basics for Social and Behavioral Research,” that is, what is and what is not research, the meaning of generalizable knowledge to subjects, the concept of the human subject; “Challenging Protocols—Methods of Inquiry”; “Challenging Protocols—Contexts and Populations”; “Barriers in Reviewing Protocols”; “Best Practices in the Consideration of Consent, Disclosure, and Deception”; and “Research Risk and Best Practices in the Determination of Exempt and Expedited Review.” The Office of Behavioral and Social Science Research of the National Institutes of Health provided support for the conference.

It is difficult to fully assess how successful the participants were in identifying “best practices.”¹ Since the conference itself was held a mere six months before we began writing this article, it is too soon to say how effectively the results of the conference will be disseminated or, for that matter, to whom. If the goals of the conference were to identify a set of best practices, to operationalize those practices in a set of procedures, and to distribute those procedures as a set of guidelines to those in academic or commercial organizations with responsibility for administration and compliance with federal regulations, then there is no reason to expect anything less than a successful outcome. Working groups experienced in the production of such documents can produce guidelines on most anything. Administrators charged with translating vague principles into coherent procedures often find such documents a great comfort. At the very least, guidelines stored in a binder and filed on a shelf provide grounds for arguing later about whether institutions discharged responsibilities properly.² However, if the working group hoped that conference participants would help produce a document that quells the considerable resentment that the bureaucratic regulation of research has created among social scien-

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tists, then they are likely to be severely disappointed, no matter how sensible, adequate, and nonintrusive the recommendations are. In fact, for those whose behavior the guidelines seek to regulate, the mere existence of one more document trying to get right the vexing question of how to assure the proper ethical conduct of qualitative researchers through organizational oversight is a symbol and symptom of a deep misunderstanding of the realities of ethnographic research and an even deeper misapprehension about how conduct is effectively regulated. In the current environment, one more working group's recommendations are more likely to fuel rather than to extinguish the flames of discontent. The burden of this article is to illustrate why this is so.

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Some of the reasons for this are structural. First, social and behavioral research encompasses a vast amount of ground to cover with a single set of regulations. Just how vast this ground is becomes clear when we look at the membership of the Social and Behavioral Sciences Working Group on Human Research Protections, the well-intended conveners of the second Belmont Conference on the ethical conduct of research. The working group had representatives from demography, education, family and community medicine, sociology, and psychology.³ The working group also includes representatives from numerous federal agencies who fund or conduct social and behavioral science research: the National Institutes of Health, the Federal Bureau of Prisons, the Department of Education, the National Science Foundation, and the Agency for International Development. The executive officers of the American Sociological Association and the American Education Research Association are also members of the working group. Those charged with applying regulations are also represented in the working group composition—there is an associate dean for research, a director of ethics education, and a director of a Native American research and training center.

Even with this extensive disciplinary and organizational coverage, qualitative research is underrepresented in the membership of the working group. One member of the working group, who attended the conference, mentions in his or her biographical sketch participating in projects using a wide range of methodologies from clinical trials to ethnographic research but then claims a special expertise in “the distribution and use of public-use data files,” an important topic to be sure but not one that is reassuring to those who desire to see the special problems that

research regulations present to ethnographers fully understood, adequately described, and realistically addressed. In all fairness, the list of attendees at the conference supplemented the expertise of the working group—qualitative researchers from sociology, anthropology, and political science participated in the conference. But the influence of these participants in the documents prepared by the working group remains to be seen. This structural flaw in representation is a generic one for tasks where a consensus that satisfies a vast array of interest groups needs to be reached, but to promote effective deliberation, the size of the working group must be limited. Still, to assemble a working group without a symbolic representative of those practitioners within the social sciences—ethnographers, who have grumbled and groused the loudest about the hindrances that the current regulatory structure imposes—seems to be a grave tactical error. After all, conversion and co-optation are, as ethnographers of deviance and social control have demonstrated, two very effective strategies for neutralizing problem populations.⁴ The voices of social scientists who have complaints about IRBs and their operation deserve a fuller hearing.

If the problem of representation is a generic structural problem, the title of the conference reflects a second structural problem, quite specific to social and behavioral science research of the qualitative kind.⁵ To title the conference *IRB Best Practices* is to link the regulation of social science research with the current regulatory regimes of clinical medicine, supported by rationales drawn from evidence-based medicine. Conferences or research aimed at identifying best practices have become a staple of the modern world of medical practice, especially in those domains that attempt to link clinical practice to findings supported by “outcomes research.” The language of best practices is part of the Quality Improvement (QI) or Total Quality Improvement (TQI) movement in medicine. The aspirational goals of this movement are entirely laudable; it seeks to adapt some of the management practices from other industries into medicine to create a more efficient, effective, and safer system for health care delivery. The title of the conference, then, reinforces the link between the rules that regulate biomedical research and the rules that regulate social and behavioral science research. For many social scientists, this linkage is precisely the problem.

IRBs can develop best practices for routine review and approval of qualitative research proposals; however, many in the social science community feel that those best practices will never be good practices so long as research is modeled on the standard clinical trial. Even if the conference conveners intended to move away from the clinical model, the banner under which the conference was convened belies their intention. In the standard research proposal, a hypothesis or set of hypotheses about the effect of intervention x on condition y is being tested. The researcher generally has a clearly bounded relationship with the research subject. The procedures involved, their risks and benefits, and the alternatives can usually be described in some detail.⁶ Roles, rules, and procedures are clear and time limited. Subjects can be fully informed, a goal that is realized more in the breach than in the fact.⁷ For some types of social science research—the laboratory experiment, the fixed-item survey, the longitudinal panel study—the relationship of researcher

and subject closely tracks the biomedical model that informs IRB procedures: a highly structured, objective relationship.⁸

But ethnographic research fits this model poorly. For ethnographers, the primary data-gathering tool consists of the relationships that we forge with those whose lifeworld we are trying to understand. Few of us start with specific hypotheses that we will later test in any systematic way. Furthermore, to the degree that we can restate our disciplinary curiosity as a set of testable propositions, these hypotheses are likely to be trivial. We cannot state our procedures any more formally than we will hang around here in this particular neighborhood and try to figure out what is going on among these people. We want to know how they make sense of their world, how they navigate in it, and how understanding their world helps us better understand our own. Neighborhoods vary: this store that serves as front for the crack dealers (Bourgois 1995); these clubs where the blues musicians congregate (Grazian 2003); this neonatal intensive-care unit (Anspach 1993; Heimer and Stauffen 1998); these neighborhoods where people sell their kidneys (Cohen 1999); these laboratories where biologists seek to unravel the mystery of peptides (Latour and Woolgar 1986). Neighborhoods change but methods are remarkably constant—really, they have not changed much for sociologists since Whyte (1942) or for anthropologists since Malinowski (1922).⁹ We observe, we may tape-record, we may videotape, we all take notes, and we code those notes according to our own various schemas—some of us use computers to sort data, and some of us still cut and paste and shuffle file cards.¹⁰ We do not know in advance what questions we will ask or, for that matter, where we will draw a curtain and choose not to inquire—or decide not to report.

We cannot inform our subjects of the risks and benefits of cooperating with us for a number of reasons. First, the risks and benefits for subjects are not so different from those of normal interaction with a stranger who will become a close acquaintance, an everyday feature of a lifeworld, and then disappear, after observing intimate moments, exploring deep feelings, and asking embarrassing questions. There is the risk inherent in any fleeting human relationship—the risk of bruised feelings that come from being used, the loss when a fixture in a social world disappears, or the hurt of realizing that however differently it felt in the moment, one was used as a means to an end.¹¹ This risk is magnified by a certain unavoidable deception in every ethnographic investigation, a certain pretense that comes from trying to have both researcher and informant forget that what is going on is not a normal, natural exchange but research—not just everyday life as it naturally occurs but work, a job, a project—“No really, I’m interested in what you have to say, think, feel, and believe for more than my own narrow instrumental academic purposes.” To some degree, we cannot specify risks because we do not know what we will find, what interpretive frameworks we will develop for reporting what we do observe, and how the world around us will change to make those findings seem more or less significant.¹² Finally, we cannot define risk because few of us believe that being an ethnographic informant is a risky business. We believe this despite considerable anthropological and sociological evidence to the contrary.

IRBs also review proposals to make sure that the confidentiality and anonymity of study subjects is adequately safeguarded. In general, this last element is relatively easy to promise. However, some situations are highly problematic. For those of us who work with highly literate populations, confidentiality and anonymity are much easier to promise than to assure. We can do nothing to prohibit a sufficiently determined reader from trying to decode the text, to stop a figure in an ethnographic narrative from identifying himself or herself, or to prevent an institution from coming forward and saying, "We are 'made-up-ville'" (Bosk 2000).¹³ Typically, such decodings are harmless but not necessarily. If read as romans à clef, ethnographic works among hospital workers can make tense and fractious workplaces even tenser and more fractious for the workers within them, who now know what they previously only suspected, who see on the printed page what was once said only behind closed doors.

In anthropology, regimes already in place have used a rich lore of ethnographic works to root out troublesome populations. This has happened despite the best efforts of anthropologists to foresee risks to subjects and to veil identities. Political climates change. What seemed a harmless remark yesterday may appear to be an insurrectionary one tomorrow. A group that no one seemed to care about can grow central when regimes shift. In addition, the requirement of subject confidentiality and anonymity, if stretched too far, does not permit ethnographic work in public domains. The workings of government, for instance, at the local, state, or federal level, would be hamstrung by an overly rigid insistence on confidentiality and anonymity. Certain forms of public controversy could not be explored as well. Because of the risks posed when confidentiality and anonymity are breached, IRBs, at least in the minds of researchers, have imposed consent requirements that themselves are out of character with the type of research being proposed. Bruner (2004) provides two different examples: (1) being asked to get written consent from illiterate peoples and (2) having colleagues refuse to answer questions about difficulties with IRBs until they were assured that the inquiry itself had been sanctioned by an IRB.¹⁴

So to restate the obvious, social scientists and behavioral scientists, in general, and ethnographers, in particular, are not thrilled with current federal regulations that require prospective review of research projects; they are skeptical that such review improves the ethical quality of research; and they believe that such review does nothing more than hinder the pace of research. Truth be told, medical researchers have never been much thrilled with the regulations either. They have done their public grousing and grumbling, and generally, they have figured out what it is that IRBs want to hear and figured out ways to say exactly that. Since IRB review is almost entirely prospective, there exists very little check on whether researchers in clinical domains do exactly what they say they are going to do.¹⁵ Why have social and behavioral science researchers not done the same? Why have they not adopted a policy of weary, self-resigned compliance coupled with minor or major evasion? Or even, if they have used passive-aggressive strategies of compliance and evasion, why are there also so many public complaints and so many public calls for resistance?

Both the complaints and the calls for resistance seem peculiar on any number of counts. First, there is an inaccurate stridency to some of the complaints. Some of those who are opposed to the current regime of IRB research have complained that the regulations violate a First Amendment right to unfettered speech. This absolute interpretation of the First Amendment neglects the considerable jurisprudence that indicates that reasonable restrictions might be placed on the time, place, and manner of speech. Others have complained that journalists, especially those journalists who do investigative reporting, are not hemmed by requirements for informed consent or the need to respect confidentiality. Why should sociological researchers, they ask, be held to a higher standard? One deceptively simple

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answer is that perhaps we should aspire to a higher standard than the average journalist. Journalism is, after all, a commercial enterprise, many of the workers in which lack what are often thought of as the prerequisites of a professional occupation—long adult socialization in a specialized body of theoretic knowledge.¹⁶ Journalism is not supported by public funds. Because we are not journalists, however many similarities one can identify between ethnographic research and investigative journalism, we have no reason to expect the same rules to apply.

Next, something is vaguely uncollegial and distasteful to the social scientist's objections to IRBs on two counts. First, much of the social science critique of both the research and the clinical practices of physicians centers on how poorly patients or subjects are informed of what is being done to them, why it is being done, and what alternatives exist. Freidson (1970), for one, has argued that physicians have used their technical authority as scientific experts to make normative decisions, choices that patients or research subjects ought to make for themselves in a democratic society. The early sociological writing on death and dying (Glaser and Strauss 1968) stressed how much information control by physicians robbed patients of autonomy and dignity. Emerson (1969) characterized the desperate measures that patients used to try to gain accurate information on their conditions. Davis (1960)

described how physicians manipulated and feigned uncertainty to control and limit patient choices. Sociologists decried these practices of information control. To a degree, when coupled with the revelations of past ethical abuses such as those Beecher (1966) catalogued or those that revealed the suffering of vulnerable, minority populations at Tuskegee (Jones 1981), these critiques led to the recommendations of the first Belmont report (Rothman 1991) that stressed informing patients so that they could reasonably exercise decision-making capacity.¹⁷ There is more than a whiff of hypocrisy in imposing obligations on others—in this case, physicians and medical researchers who cannot be trusted because their self-interest makes unreliable their judgments of others' best interests—while resisting those very same obligations for oneself because our work is harmless, our intentions good, and our hearts pure.

A second dimension of the uncollegial spirit of the social and behavioral reaction to IRB review is its uncharitable nature. If we were honest, we would recognize that IRBs and the regulatory and administrative regimes for research now have a fair amount of bureaucratic momentum behind them. How much good IRBs accomplish and how much harm they prevent remain open questions. How procedures might evolve so that they better meet the realities and contingencies of social and behavioral research as it is actually done in a real world of real people is certainly open. What is not open is whether a prospective review of research will exist.¹⁸ So at the very least, we social scientists ought to recognize that the committee work involved by our colleagues on IRBs is like a lot of other committee work. It is thankless; it is done in pursuit of some communally shared objectives, even if these are poorly articulated; it is underfunded; and if it were not done by these colleagues, we might have to do it ourselves, which would mean that we would have less time to do our own research. We should all, then, be a bit more forgiving of the imperfections of the whole structure even as we remember them—all the better to correct them. If all this were not reason enough to be a bit more constructive and cooperative in our critique of IRBs, we can add one more: none of us truly objects to the goals of IRB research—we all wish subjects to be treated with respect, protected from harm, and saved from embarrassing exposure.

Therefore, we need some constructive suggestions for making the system work better, we need to educate IRB members about the nature of qualitative social science research, and we need to do a better job of educating ourselves about the regulations—if only because such education will make clear to us that the system is less onerous than we fear. We need to explore the different ways that the system can be streamlined and made more efficient. It is in this spirit that we offer the following suggestions, all of which have been floated in the vast literature on IRBs.

Encourage more and better studies of how IRBs work. Given the continued and high level of displeasure with IRBs, we would expect ethnographers to be studying the way IRBs work. When it comes to IRBs, social scientists are cobbler's children. Like the hapless waifs whose feet remain unshod while their parent makes shoes for others, social scientists complain about the many shortcomings of IRBs while

using their skills to describe and analyze nearly every other sphere of human activity. Reports on the shortcomings of IRBs are rarely supported by data. A recent American Association of University Professors (2001) report is based on anecdote, a nonscientific survey of social science researchers, and testimony given to government committees; the report of the National Research Council lists only eight studies in an appendix, "Selected Studies of IRB Operations: Summary Descriptions." One might assume this small number is the result of careful selection, but as the authors of the study note, "There is little regularly available systematic information about the functioning of the U.S. human research participant protection system" (Citro, Ilgen, and Marrett 2003). We need to better understand what IRBs look like and how they work (De Vries and Forsberg 2002).

Increase social scientist participation on IRBs. In institutions with multiple subcommittees of IRBs, create a specialized IRB for vetting social science research. IRB composition has specific requirements: multiple disciplines must be represented, there must be a community representative, and there must be at least five members. There is no reason why one subcommittee cannot specialize in social science research. This will assure that research is reviewed by colleagues who have some understanding of social science methodologies.

Increase social scientists' knowledge of IRB rules. We all need greater understanding of what the rules require of us. A good deal of ethnographic research falls under the category of expedited or exempt research. We need to familiarize ourselves with these categories of research. We need to make clear to IRBs why we believe that our research falls into one category or the other. We need to be familiar with those situations where the regulations allow something other than written consent; generally, in situations of minimal risk, alternatives to written consent are permissible. We also need to know which populations are defined as vulnerable and the set of special protections afforded such populations.

From experiences as a member of study sections for the Ethical, Legal and Social Implications part of the Human Genome Project, one of us often found that researchers submitting proposals using qualitative research methods are defensive when specifying what they will do and are incomplete when discussing consent procedures. As a consequence, methods are left vaguer than they need to be, and the requirements for consent appear to be evaded rather than embraced. We understand why this happens: it is a pain (really, there is no other word for it) to try to shape a proposal for a qualitative research effort into the format required by a National Institutes of Health RO1 Grant application. But if we wish funding for our projects, it is a pain we must endure. Specifying to an IRB how we will meet the requirements of informed consent is likewise a pain, but it is one that can be borne with a bit more grace than we have managed so far. We can begin to engage the process creatively if we know the rules. The more familiar we become with those rules, the more we can work around them, find the loopholes, and yes, even amend them so that they make sense given the kinds of inquiry in which we engage.

Educate IRB members. Social scientists have complained about the “mission creep” that occurred when IRB jurisdiction expanded from biomedical research to all research involving human subjects. Whether this truly was mission creep or just a natural extension of a mandate is for others to debate and decide; however, the fact that IRBs began to review proposals with biomedical research in mind has had a number of implications for social scientists using qualitative methods. First, the personnel who serve on IRBs are, as a consequence, overwhelmingly drawn from the ranks of biomedical researchers.¹⁹ Research protocols themselves are then reviewed in narrow terms: What was the risk/benefit ratio? How adequate was the consent form? When faced with qualitative research proposals, whatever normal operating procedures and rules for deliberation committees had evolved break down. In addition, the template of the research being proposed makes little scientific sense to committee members who have a trained incompetence when it comes to the inductive methods of qualitative research. Under these conditions, too many IRBs have members who decide that qualitative research has no scientific validity; hence, it can offer no benefit; and as a consequence, there is no risk worth contemplating for human subjects. Qualitative researchers then have reasonable proposals turned back with what seems like a set of unreasonable objections and appears to be a willful obtuseness about the nature, conduct, and purpose of the proposed research. To avoid this situation, two types of education for IRB members are required. First, IRB members need to learn to extend beyond their own disciplinary boundaries. They need not like, approve, or be advocates for qualitative research. They do need, however, to have enough education in its methods and the theories of social life that undergird them to appreciate that this is a legitimate form of inquiry. With this understanding comes the knowledge that there are better and worse ways to proceed with qualitative projects, that there are criteria of judgment that can be applied, and that reasoned decisions can be made rather than positivistic prejudices enacted. Second, IRB members, like their social science counterparts, need to be familiar with their own rules of operation. They need to know which research qualifies as minimal risk, which projects are exempt from review and which can receive expedited review, and which research projects are allowed alternatives to written consent. If IRB members are not well versed in the rules that they are charged with applying, then they will have difficulty applying those rules correctly.

Have in place a speedy appeals process. One thing that frustrates researchers, whatever methods they employ, is delays encountered when IRB approval stalls the start of projects. For qualitative researchers, this frustration is multiplied when it seems that a failure to win approval grows out of a misreading in what was involved in the research process. Resubmissions that require answering nonsensical objections are not an adequate remedy. For the requirements imposed to make a research proposal pass muster may also make that same project impossible to do. Beyond that, the delay between original submission, response to objection, and resubmission may erode whatever access to a field setting that has been negotiated. Finally, there is little confidence that the same folks who made such a seemingly

arbitrary decision so recently will now act with the requisite wisdom. Institutions need to have in place appeals mechanisms that allow researchers a speedy hearing before a body of colleagues who will not be made defensive or be constrained by their prior rulings, who understand what qualitative methods entail, and who are familiar with the rules governing research with human subjects. IRBs need to

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remember that few researchers design projects with the intent of harming people. Researchers need to remember that IRBs have a specific job to do. An appeals process that puts the ball in a different court reminds each of them that neither of them is infallible.

Explore other ways of organizing review of social science research. The United States was a pioneer in the creation of ethics review boards, but we have much to learn from the ways other countries have responded to the need to protect the subjects of research. There are other ways to organize ethics reviews and other ways to approach review of social science research. In the Netherlands, for example, social science research is exempt from review unless it places a demonstrable physical or psychological burden on its subjects. Paying attention to the way our colleagues in other countries have approached this problem will create solutions to our dilemmas.²⁰

So that is the gist of the argument. IRBs are here to stay. The review process may impose more harm than benefit, but it is hard to imagine turning back now. Social scientists are not bureaucratic incompetents or mutes. They can find ways to work within the system at the same time that they work to change it. When the requirements for approving research with minimal risk are looked at, the entire review process seems to impose fewer burdens than what we actually complain about. The system is highly imperfect. We can make it better. The system is underfunded and understaffed. Given this, a spirit of cooperation rather than belligerence seems the appropriate way to respond to our colleagues who have either volunteered or had their arms twisted to perform this onerous task.

All that said, we cannot help but feel that one more thing is worth saying. We do not think that the system of prospective review that we have adopted does much to

protect subjects from harm or guarantee ethical conduct from researchers. The ethical problems that we meet in the field are so complex and the situations are so fraught with the moral and existential dilemmas of leading a life that consent does little to assure our subjects or ourselves, for that matter, that we will do the right thing when the situation presents itself. What should the sociologist studying faith healing in suburban New Jersey do when parents refuse to take a sick child to the physician? What does the sociologist studying an intensive-care unit say when a doctor, nurse, or family member contemplating treatment withdrawal asks what he or she should do? What does the anthropologist studying the drug-addicted homeless do when an informant, copping some heroin, is a few dollars short and asks for a loan? In his book *The Secret Army: The IRA, 1916-1974*, J. Bowyer Bell (1970) includes a series of photographs depicting a bomb attack.²¹ Four plates detail the loading of the bomb into the car, the moments before the explosion, the explosion itself, and the damage done. It turns out that these photographs were not taken by Bowyer Bell, but one can easily imagine that the qualitative researcher studying a group like the IRA, or a teen gang, or professional gamblers, or sex workers would come upon such scenes. In scenes when the last item in the sequence is “the damage done,” it is certainly reasonable to ask what are the obligations of the researcher to his or her informants, as well as to third parties. Resolving this sort of ethical dilemma is not the sort of thing that is amenable to prospective review, to an abstract calculation of risks and benefits, and to consent. The problem with IRBs and qualitative research is that they are such a distraction from the real difficulties that we face and from the real ethical dilemmas that confront us that we may not recognize and discuss the serious and elemental because we are so busy with the procedural and bureaucratic.

Notes

1. One of us (De Vries) was present at the conference. Attendees generated useful and long lists of (1) the problems of reviewing the research proposals of social and behavioral scientists and (2) the solutions to those problems; staff members of the working group are in the process of refining these lists.

2. A wise law school faculty member once told one of us (Bosk) that contracts do not regulate working relationships among parties. When there is an adequate working relationship, there is no need for a contract—the parties can work out differences on the hoof as it were. In fact, that ability to make adjustments and move on is as good an operational definition of a working relationship as one might ever hope to find. Contracts set the ground rules for the dispute when working relationships fall apart. Guidelines are a bit the same; it is only when disaster fails to be averted that one needs to check if guidelines were followed. One consequence of this is, as Snook (2000) points out, that there is a great deal of organizational drift in everyday practice as the need for practical contingencies gets dislodged from formal organizational rules.

3. Anthropology, political science, social work, and nursing are nowhere to be found in the individual disciplines of the working-group members or among the professional associations with representatives on the working group. A note from the editors in the January 2004 issue of *Anthropology News* informs readers that “work on a position paper defining ethnographic practice with reference to IRB [institutional review board] guidelines has been begun by the AAA [American Anthropological Association]” (p. 10).

4. There are two other groups that went unrepresented. Here, the lack of representation is likely to go unnoticed, but it is not unimportant on that account. First, there were no representatives from the increasing number of organizations that provide institutional review board (IRB) review of protocols for a fee. Second, there were no representatives of commercial research organizations, a fast-growing organizational segment

within the research marketplace that is responsible for a good deal of social and behavioral research in the clinical/medical domain (e.g., research on compliance with medication regimes; Petryna, forthcoming).

5. Identifying this problem, as one that is specific to “social and behavioral research of a qualitative kind,” points to another larger, definitional problem, hiding behind the problem of representation. There are multiple kinds of qualitative techniques. Calling interview studies ethnographies, or identifying focus groups as a technique for providing a “thick description,” does little to solve the confusions induced when trying to write rules that assure that firsthand observational research conducted in settings where behavior naturally occurs—research that cannot be done effectively without rapport, trust, assent, and consent of subjects—is “ethical.”

6. Here, one might need to exercise caution. Even with the medical model, the identification of risk to both research subjects and IRB members often depends on the thoroughness of the investigator, the completeness of the literature review, and the willingness of the researcher to provide what Weber called, in a far different context, “inconvenient facts.” A healthy subject died in an asthma trial at Johns Hopkins when researchers failed to discover for themselves or failed to inform the IRB and, surely, as a consequence, failed to inform a volunteer in a clinical trial that a pulmonary antagonist intended to induce asthma so that the efficacy of a new therapeutic agent could be tested had caused death in animal trials. Even in the most straightforward and mechanical of research designs, the adequacy of regulations rests on what Parsons (1951) called the “institutionalized integrity” of the profession—its willingness to face inconvenient facts. To state the same point a slightly different way, Freidson (1970, 1975) long recognized that the monitoring, surveillance, and social control of those who possess highly specialized, abstract, esoteric, theoretic knowledge depends on the cooperation of those with that knowledge. To Plato’s old question, “Quis custos custodes?” (Who governs the governors?), the answer appears to be the *custodes* themselves.

7. Since the implementation of IRBs, there has been a body of evaluative research that documents the gap between the goals of the regulatory schema and their fulfillment. This research documents that consent forms are difficult to parse (the standard aimed for is that of the average eighth-grade reader), that subjects fail to understand the nature of research (double-blind nature of clinical trials is particularly problematic), and that subjects conflate the roles of researcher and clinician, as well as the nature of research and therapy. So when we say that ethnography poses special problems for IRBs that clinical research does not, we are not comparing a system that works perfectly in one domain but fails to fit another.

8. Even here, the relationship is not perfect. For example, forced-compliance studies like the Asch or Milgram experiments—and here we are leaving aside all the other ethical questions that surround these experiments, as well as the enormous lore that mis-describes them—could hardly have been practiced without deception. This, we suppose, means they are great fodder for the conference session on “Best Practices: Consent, Deception, and Disclosure.” For a hilarious send-up of what it must have been like to be a subject in the Asch experiments, see the description in the novel *Kinfflicks* (Alther 1976). In fact, the general requirement of informed consent—disclosure of the purpose of the study—poses problems at many levels for social scientists of all stripes. A great deal hinges on how complete the disclosure needs to be to satisfy the requirements of informed consent.

9. We could have invoked Park and Boas. Origins are tricky things. We cannot speak at all authoritatively about anthropology. We are not historians of social science. But as sociological researchers using qualitative methods, self-consciousness about what is happening here seems to us to begin with Whyte’s second edition of *Street Corner Society*, published in 1955.

10. Our methods have not changed, but in both sociology and anthropology, there is a considerable amount of self-reflexiveness that has developed relatively recently about those methods. There is rather more of this in anthropology than in sociology, but in both fields, there is a rather thorough chewing over of such questions as what does it mean to represent “the other,” whose voice does the ethnographer authentically represent, and whose interests do ethnographic representations represent.

11. This is phrased to deliberately echo the violation of Kant’s categorical imperative. Careful readers will notice that the word *solely* has been omitted; this surely makes an ethical difference.

12. Two examples serve to illustrate this point. One is drawn from reality; one is hypothetical. Jeffrey Goldfarb’s (1982) work on art and theater in Poland obtained a salience when the Solidarity movement became politically active and visible that the work did not have when he designed his study. Second, imagine the ethnographer studying the integration of the Arab American community in the United States and its continuing ties to countries of origin before 9/11 and after 9/11.

13. All of these forms of decoding have consequences. A reader who decodes a text, incorrectly or correctly, may cause others to give pause before consenting to observational studies. An individual who, or an institution that, comes forward vitiates the promises that the researcher made to all the others in the study who have not chosen to come forward (Bosk, forthcoming).

14. This second example highlights the difficulty of what is research and what is not. We wish to know if a group of colleagues share a problem with us because we wish to know if we are dealing with a “private trouble” or “a public issue.” At what point does my inquiry shift from purely personal curiosity to a formally organized search for generalized knowledge? Or, to put it another way, are we, as Bruner fears, reaching a point where “we will not be able to have conversations about research with our colleagues without IRB approval”?

15. A new bureaucratic level of scrutiny, the data safety monitoring board (DSMB), has been created to make sure that research is as safe as promised; how well these boards will work in practice remains to be seen. Once again, like all such reporting schemes, the efficacy of DSMBs depends on the institutionalized integrity of researchers both recognizing and reporting adverse events.

16. Compare the importance of graduate training to on-the-job training in sociology or journalism. It is hard to imagine sociologists without Ph.D.s holding down positions at major research universities. On the other hand, journalists need not have attended journalism school or have advanced degrees to obtain positions of respectability. Of course, lines that are drawn sharply can be just as easily blurred. Robert Park, one of sociology’s first advocates for the primary observation of social life in natural settings, came to sociology from journalism. But he did attend and receive a doctorate from a German university.

17. In a recent online article in *Spiked*, Richard Shweder has argued that the historical evidence does not support the accepted narrative of Tuskegee as racist, deceptive, and unethical research. Shweder offers a counternarrative that attempts to argue that Tuskegee was not so bad as to justify the regulatory regimes justified in its name. Whatever the merits of Shweder’s argument, it is hard not to feel that the point of the article is not to correct the historical record but rather to undermine the justifications for the federal oversight of research with human subjects (available at <http://www.spiked-online.com/articles/0000000CA34A.htm/>).

18. We put up with quite a bit of bureaucratic nonsense as academics. We sit on strategic planning committees whose documents are often ignored or risible—we will strive for intellectual excellence and equip our students with the skills they need to excel in the world of the twenty-first century and with the skills necessary for lifelong learning. We participate in external and internal departmental reviews. We sit and try to figure out what the general requirement in liberal arts education means and how it can best be accomplished. We go to faculty meetings and discuss the same inanities in the same inane way month after month, year after year. We may feel the occasional moment of self-pity about all this; we may joke about it. But, at some level, we all recognize that these sorts of meetings only seem pointless; we recognize that they are a way of discussing what kind of community we have been, we are, and we hope to be. The discussion of research ethics in IRBs is the same kind of community-making discussion, but curiously, we social scientists have some trouble recognizing this even as we debate among ourselves how much and which Durkheim our students ought to read.

19. So much was this the case that when Bosk came to Penn and became a member of the IRB there, he was designated an outside community member. This seemed strange not only to Bosk but to a federal regulator, although it took two years for Bosk to be redefined as a token for disciplinary breadth. In their recent survey of IRBs, De Vries and Forsberg (2002) found that IRBs are dominated by those working in medical occupations.

20. De Vries (forthcoming) contrasts the requirements for membership on IRBs in the Netherlands and in the United States, observing how the regulations in the United States are based on *identity*, while those in the Netherlands are based on *expertise*.

21. We would like to thank Michael Walzer for pointing us to this volume.

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