

CHAPTER **16** **Adventure and Regulation
in Contemporary
Anthropological Fieldwork**

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To establish ourselves as professionals, anthropologists have long tended to downplay the adversities we encounter—distrust, opposition, calamity, irreducible ambiguity—in order to protect the credibility of our research. Even the rather attractive category of adventure usually finds a place only in our memoirs or popular treatments, not in peer-reviewed articles and books. Candid portrayals of adventures and the complications they leave behind could undermine the air of impartial authority for which most of us strive. Now that cultural anthropology has become absorbed in how our position as observers affects the knowledge we produce, the anthropologist as adventurer becomes pertinent in a new way. But anthropologists who boast of adventures in some contexts will have good reason to deny them in others.

The reason is a basic ethical dilemma in anthropological research—the power differential between ourselves and many of our subjects and what this can mean in a crisis. Perils that we can recount as an adventure, because we were able to escape, can spell death and destruction for our subjects, because they could not. When anthropological decision-making imperils the people we study, we violate the ethical imperative of doing no harm. Heightened consciousness of this and other inequities has led to a new era of regulation in anthropology, in which there is even less room for adventure than before.

One expression of the current regulatory era is the human subjects protocol, an externally imposed form of regulation to protect the subjects of

research from the researchers. However many reasons anthropologists may find to study adventure, having an adventure ourselves entails risks that violate human subjects requirements. A second, more internalized expression of the new regulatory era is epistemological shame, a systematic distrust of Western claims to knowledge that is often accompanied by reverential attitudes toward alternative sources of authority. The problem here is the kind of story that adventure generates and the claim to credibility that it makes, a credibility that depends on the construction of a heroic self.

This collection brings together fieldwork and adventure because they are both presumed to be heroic. They both involve venturing into novel situations that will test one's ability to survive. By stressing that adventure is a deliberate undertaking that requires conscious choice and awareness of risk, I am departing from Simmel's definition of adventure. Simmel regards adventure as an effect of accidental circumstances, a temporary lack of control that shakes one's sense of confidence, thus offering a striking contrast to everyday life and generating a powerful sense of meaning. In my opinion, he is actually describing the anxiety-discharging, gratitude-building effects of surviving a calamity. "Thank god we're alive!" Calamity is a necessary feature of adventure—no adventure fills the bill without it. But the experiences being explored in this collection are a larger and more deliberate endeavor than mere calamity.

If adventure were simply the result of accident, we could not embark on an adventure or go in search of it—customary usages in English—because adventure could only befall one, not be actively chosen. Adventures that are more than sudden calamities imply considerable effort to achieve a certain kind of experience. The search is similar to the quest, the pilgrimage, or the mission but is more open-ended than a journey fixed on a lost grail, ark, or tribe. Adventure is a deliberate undertaking that requires daring decisions, in which the adventurer chooses to enter into situations where he will have less control than usual, where he risks losing control completely and suffering dire consequences that could have been avoided.

This makes some kinds of fieldwork a subset of adventure, but the decision-making that goes into each is intended to produce very different narratives. As scientific research, fieldwork is supposed to be planned with care, be carried out with diligence, and produce carefully considered conclusions. Above all it cannot harm informants or collaborators—this according to the American Anthropological Association's Code of Ethics. In contrast, going in search of adventure requires actively courting danger, to the point

of inviting it into your canoe. Adventure minus any danger does not add up to adventure. It requires daring or foolish decisions whose unfortunate outcomes become the pivot of the story. Egregious mistakes and life-threatening mishaps are proudly owned because, without them, it would not be an adventure. The result is a narrative of survival that claims a very different kind of credibility than scientific research.

I speak as one who is presumed to have had adventures because I did fieldwork in a counterinsurgency zone in Guatemala. The truth is, I correctly calculated the risks as lower than they appeared to be, I cleverly followed in the path of aid workers who had gone before me, I figured out how to keep a low profile and, as a matter of principle, I managed to avoid having adventures with unfortunate outcomes. It is good that I have no exciting stories to tell because, among my advisers at Stanford University, there was frank skepticism that a counterinsurgency zone in Guatemala was sufficiently low-risk for fieldwork in 1988–89. They were more worried about my local informants than me. What if the Guatemalan army confiscated my notes and targeted the people who were helping me?

Of three other graduate students who did fieldwork in the western highlands during this period—Robert Carlsen, Linda Green and Judith Zur—all three managed to get through most of their planned stay like I did, but each of them had more trouble with the Guatemalan army than I. In the case of Carlsen and Zur, unwanted attention from the army forced them to end their research earlier than planned. A year later, in November 1990, army operatives murdered our Guatemalan colleague Myrna Mack. Mack was interviewing internal refugees whom the Guatemalan army regarded as a logistical base for the Guerrilla Army of the Poor. Unlike myself and the three others, Mack did not have a passport from a powerful, influential country. Also unlike the rest of us, she was involved in public challenges to the army's claims, through the Catholic Church's advocacy for the refugees in question, the Communities of Population in Resistance. This indeed was an adventure, which culminated in being knifed to death outside her office by army plainclothesmen working for the Presidential Military Staff (REMHI 1998: 292–93).

If I had experienced a serious adventure—such as getting arrested and causing the detention or disappearance of one of my sources—Stanford might well have refused to credential me as a PhD, and with good reason. Only a few years before, the Stanford Anthropology Department disciplined two graduate students for taking risks that it decided were unethical. In

1981, Steven Mosher denounced the Chinese government's enforcement of the one-child population policy by forcing women to have late-term abortions. His magazine exposé included photos showing the faces of his subjects, which made them identifiable and which Mosher himself has admitted was a mistake. The Chinese government expelled him and so did Stanford—although Stanford rested its case on Mosher's lack of candor with his committee. The same year that Mosher's criticism of the Chinese government put him under a cloud, in 1981, another Stanford graduate student named Philippe Bourgois took a sudden opportunity to visit a guerrilla-controlled zone in El Salvador. As soon as he arrived, unfortunately, the Salvadoran army attacked. Troops and helicopter gunships pursued fleeing civilians for days, with Bourgois himself surviving only because he was young and fleet. Like Moser, Bourgois took his experiences to the media. Mosher never obtained his degree; Bourgois did; but in both cases, according to Stanford, they had not informed their committees about important risks they were taking. In both cases (although not very plausibly in Bourgois's case as far as I can see), Stanford decided that they had endangered their human subjects (Beyers 1985; Coughlin 1987; Bourgois 1990).

I now hasten to add that pulling Mack, Mosher, and Bourgois into an essay on adventure is a misrepresentation of what they were about, which I do only to make the following point. Each made the decisions that they did for what they felt were compelling reasons, not for kicks. Moser wanted to embarrass the Chinese government into ending coerced abortions. Mack and Bourgois wanted to stop military bombardments of civilians. I certainly would not want my own research in a Guatemalan war zone to be described as an adventure. Why not? For the simple reason that counterinsurgency zones and coerced late-term abortions are life-and-death situations for the people who are trapped in them. Anyone who chooses to join these situations, then labels them as an adventure, is playing up his own bravado at the expense of others who have no choice.

Because the very term adventure underlines the cruel contrast between the haves and have-nots of this world, the people with passports and people without them, this ever more embarrassing gap has given anthropologists a professional interest in avoiding the term. Yet there is no denying that some anthropologists are drawn to risky situations, that our association with these can become an important source of professional capital, and that the entire profession has capitalized on the aura of adventure surrounding fieldwork.

Let me now distinguish two kinds of adventure which, when they occur in fieldwork, pose ethical dilemmas:

One is the calamity, the unforeseen peril that jumps out in front of you like an avalanche. Or like a zealous new lieutenant who takes a sudden interest in your research and assigns a couple of soldiers to follow you around town, causing you to panic that you, or even worse, the Guatemalans who have been helping you, will be arrested and interrogated. This kind of adventure is best described as a calamity because, while your decision-making placed you in harm's way, you were taken by surprise. You may have been vaguely aware of venturing onto dangerous ground but failed to plan for a dangerous contingency.

The second kind of adventure is a risk or challenge that you choose to advance your research, your career or personal development. Thus you might decide to approach the local army commander, in the hope of being able to watch how he uses his power vis-à-vis the local civilian authority. However, you realize that it will be risky because you will have to explain the purposes of your study. It could backfire by drawing unwelcome attention to yourself and the vulnerable local people who have been helping you.

Generally speaking, the adventure I was trying to avoid in Guatemala was a collision with a power structure. Cartoon anthropologists face adventure in the form of demanding topography or hostile natives, but what made my fieldwork adventurous was a military occupation in which I could trigger reprisals against myself or my informants. This was what worried my Stanford advisers: they couched the risk in terms of human subjects and we negotiated how I would deal with it in terms of my human subjects protocol. The protection of human subjects is a form of institutional supervision that originated in biomedical research and has now been transferred into social research, to the growing dismay of anthropologists.

In the U.S. human subjects monitoring began with the outcry over a syphilis experiment conducted by the U.S. Public Health Service at the Tuskegee Institute in Alabama. From 1932 to 1972, government researchers withheld treatment from 399 black men in advanced stages of syphilis in order to study the long-term effects (Jones 1981). They did so without their subjects' knowledge or consent, in disregard for the Hippocratic Oath, and even prevented their subjects from being treated by other doctors. So the impetus for the new regulations was to protect human guinea pigs from unethical medical experiments. Ever since, medical researchers have been

required to explain the purpose of their research to their subjects, to inform them of any risks they may incur, and to obtain their informed consent, all of which is to be certified with a signed consent form.

Since the 1980s, the federal government has extended human subjects requirements to social research including participant-observation as well as interviewing. The controls no longer apply merely to federally financed research. Any institution that receives federal funding must now set up an institutional review board (IRB) to supervise all research conducted by the institution. Thus IRBs now routinely require any researcher interviewing people one-on-one or in small groups to obtain signed consent forms from each human subject. Many social researchers question whether the biomedical model is appropriate for oral communication between researchers and their sources, let alone for the more informal styles of interactional research. For anthropologists like myself who work with preliterate people, IRBs typically waive the requirement for signed consent. But signed consent forms are unwieldy for many kinds of participant-observation research—for example, with any shifting social group at a party or on a street corner. Most risks in this kind of research are slight, they are usually created by many factors beyond the researcher's control, and they are typically impossible to predict. Yet a few cases of institutions losing all their federal funding, owing to regulatory overreactions, have prompted IRBs to start rejecting social research on the basis of improbable worst-case scenarios (Overbey 2001; Bruner 2004).

In the U.S., human-subjects regulation is driven by lawyers and fear of torts, but there are parallel developments in other Western countries that Marilyn Strathern (2000) and her colleagues have analyzed in terms of "audit culture." Audit culture is not just a set of verification rituals, or a politically neutral legal-administrative practice but, in Foucaultian terms, an instrument of new forms of governance and power (Shore and Wright 2000: 57). The rationale for many new expressions of audit culture is what Michalis Lianos and Mary Douglas (2000: 267) call "dangerization," that is, "the tendency to perceive and analyse the world through categories of menace."

Dangerization has gone on display in the multiplication of video monitors and other automated security devices to surveil public areas and to mark off private from public spaces. It can also be found in the projection of the biomedical human-subjects model far beyond the medical and psychological experiments for which it was designed. The open-ended, unpredictable quality of ethnographic research has become all too attractive to

human-subjects regulators. Once dangerization becomes part of an institutional routine, Lianos and Douglas (2000: 273) warn, it produces a constant “scanning [of] the environment for perceptual indices of irregularity, which are then perceived as menacing.”

Once risk has been magnified, meticulous routine is required to protect against it, overriding the judgement of the person doing the research. Two kinds of fieldwork could become impossible if the biomedical model for ethical research is carried to the logical but far-fetched worst-case scenario. The first kind of research is on illegal or dangerous behavior. For example, warfare and its substitutes—such as football hooliganism—are inherently risky whether or not a researcher is present. Once worst-case logic is applied, the presence of a researcher becomes even more risky. Just imagine how a hormonally attractive researcher could encourage belligerents to demonstrate their manhood even more strenuously than before, making the research unacceptably risky. As for the informed-consent requirement, imagine calling the adversaries together, asking them to put down their weapons or gear, delivering a human subjects talk, pointing out that the presence of the researcher could increase the risks to which they are subjecting themselves, and persuading every last one to sign a legal document.

A second kind of investigation that could be regulated out of existence by the biomedical model for ethical fieldwork is research on powerholders—like those Guatemalan army officers whom I usually avoided interviewing. Laura Nader’s (1969) idea of “studying up” in the power structure rather than “studying down” is one of the most valuable suggestions in the history of anthropology, but it could succumb to rigorous application of the biomedical ethical model. To echo a point made by Philippe Bourgois (1990), if my study had focused on army officers rather than on violence survivors, would I be obliged to inform an army officer that one of the risks of helping me is that his words could be turned against him? If I tape-recorded an interview in which he employed euphemisms for torturing and killing prisoners, would I be required to respect his anonymity? This is what biomedical research ethics could require. Yet to do so would violate my ethical commitment to other people I was studying—the population whom the officer was administering.

The biomedical definition of a human subject is too narrow for social research because it presumes that the subject is a powerless doctor’s patient. The premise of a human subjects protocol is that the researcher has power over the subject. Yet some subjects have power over researchers, to

say nothing of power over other research subjects, whose human rights they may be violating. This brings us back to the problem of adventure in fieldwork, of any kind that generates animosity toward the researcher or his collaborators. Since any kind of questioning of power figures can potentially lead to reprisals, IRBs could use the biomedical model of research ethics, and how it defines research subjects as powerless patients, to rule out research on power structures. The reason is that research on the powerful is too risky.

The *reductio ad absurdum* biomedical regulation I've been describing comes from outside anthropology. But the problem of how to protect informants in lethal political environments is no laughing matter. It will not go away; it requires the most careful attention by everyone in anthropology who deals with these situations. Meanwhile, there is a deeper sense in which anthropology is regulating itself against adventure, an epistemological distrust of the heroic self. This is not just a fad confined to postmodernists—it reflects a much wider lack of confidence. Elsewhere in the collection, David Napier describes it in terms of American culture but there are probably parallels elsewhere. At least in the U.S., parental protection of the single progeny, institutional fear of litigation, and consumer gratification have combined to undermine rites of passage from one life-stage to another.

Take academic life. There used to be plenty of room for pranks, drinking contests, and sexual adventure, or so the old-timers tell us. Now these are all risk behaviors. If you feel like indulging in any of them, your therapist will hold your hand and your insurance plan may even pay for a few sessions. But don't let your risk behaviors come to the attention of the human subjects committee or the diversity dean. Even in the supposedly rugged outpost of anthropology, the anthropologist in his tent has been displaced by the theorist in the armchair. Sanctimony reigns. Bug bites, dysentery, and blisters have been replaced by the stress of "writing"—laboriously reworking one's narrative through fingers on a keyboard.

In the wider consumer culture, among the more daring, rites of passage are being replaced by the optional personal adventure. Some of these experiences are so genuine that they kill the protagonist—you can read about them at the Darwin Awards, a website honoring people who kill themselves through their own stupidity. Because natural selection has removed their genes from the gene pool, they have contributed to human evolution. More often, of course, prepackaged adventures minimize the possibility of death with safety procedures.

Adventure in this sense functions as an individualized rite of passage. The adventurer has proved his fiber and achieved self-validation. He is a survivor, a veteran, or an old hand even if he had to pay for the test with a credit card. The resemblance of adventure tourism to anthropology is not just coincidental, as other contributors to this collection have pointed out. In anthropology, the high-risk decision to join a profession for which there is so little market demand is just the beginning of a lengthy and highly individualized rite of passage. But this is not a rite of passage that produces a muscular hero or explorer, at least not anymore. No longer can it produce the anthropologist as oracle like Margaret Mead—imagine one of our post-colonial divas allowing herself to be photographed in a grass skirt. Instead, the only kind of hero who is permissible in contemporary anthropology is the humble sage.

Consider Karen McCarthy Brown's account of her experiences with Vodoun healing in Mama Lola—this is the rite of passage as a religiously imbued growth experience. Or Orin Starn's account of fieldwork with peasant watch-committees in *Nightwatch*, which is more ironic and self-deprecating. Sophisticated ethnographers put themselves on center stage, but only for carefully measured intervals, to protect themselves against accusations of narcissism. When facing dangerous situations, they protect themselves against accusations of heroism by stressing that they were terrified. Anthropology becomes a lesson in humility.

What about the old-fashioned kind of adventure, in which the hero prevails against bandits, warlords, wild animals, or a typhoon? This would require a heroic narrative with the anthropologist, or at least a trusty sidekick, overcoming the adversary. But if the adversary is human, overcoming him violates his rights as a human subject, which has become unethical. Consider the case of Napoleon Chagnon. As suggested by Harald Prins, in one of the panels that led to this volume, the adventure tone of Chagnon's early writing spelled his doom in contemporary anthropology. Once Patrick Tierney dramatized the high mortality that the Yanomamo suffered, Chagnon's attitude seemed callous and unethical, quite apart from the many issues that need to be debated.

Contemporary anthropology is hostile to adventure because adventure is a way of investing oneself with sanctity, in the sense of certainty or unquestionability used by Roy Rappaport in his *cybernetics of sanctity*. Usually we think of unquestionability as a quality of religious thought, but it also can be found in political and scholarly discourse, and it also can be

discerned in the aura surrounding any credible survivor narrative, which is what adventure stories are—survivor narratives. When adventurers tell how they survived death and destruction, they are invoking the old association between death and unquestionability. The association between death and unquestionability is expedited by the fact that death is a definitive state, defined by the absence of life, that can be used to validate the kind of binary communication that we associate with yes/no, either/or, and surrender-or-die. This is why criminal syndicates and death squads leave bodies in public places, to send an unequivocal message. In other contexts as well, death is a subject that we all care about, that commands attention, and that is more likely to be taken seriously than any other. As an end-point that we all face, it is inherently significant. The invocation of death—or of a near-scrape with death, which is the point of a good adventure story—conveys a sense of unquestionability for which human beings hanker.

But this sort of maneuver cannot go unquestioned in a discipline dedicated to questioning Western power and epistemology. The only ethically permissible source of sanctimony is now identification with victims. Adventure is a heroic undertaking and anthropology has no room for heroics that require vanquishing a human adversary. What is left for anthropologists is education in humility, which is why the only voice left in which adventure can be performed is that of irony. This is a new form of regulation, far more internalized than human subjects regulation, and it is becoming hard to imagine doing anthropology any other way.

