

Ethical challenges of micro-level fieldwork¹

Lee Ann Fujii
Assistant Professor of Political Science and International Affairs
Program Coordinator, International politics cohort, Women's Leadership Program
George Washington University
lafujii@gwu.edu

Paper presented at the workshop on Field Research and Ethics in Post-Conflict Environments, December 4-5, 2008, Graduate Center of the City University of New York.

What are the ethics of micro-level research in terms of protecting identities and confidentiality during fieldwork and publication? Unlike research conducted at greater analytic and geographic distances, micro-level studies involve close contact between the researcher and researched. Indeed, the powerful insights that micro-level research generates is a function of the face-to-face interactions that the researcher has with her subjects. Researchers sometimes maintain such contact over years, creating relationships that may blur the line between observer and observed.

Micro-level research is not confined to any single discipline or methodology. Anthropologists, historians, political scientists, sociologists, economists, and psychologists have all engaged in micro-level research. Their methods vary considerably; they include ethnography (Bringa 1995; de Lame 1996; Wood 2003; Nordstrom 1997), field experiments (Paluck in press), surveys (Straus 2006), and intensive interviews (Fujii 2009; Brass 1997). What micro-level studies have in common are not disciplinary norms or methodological preferences, but rather researchers' direct engagement with subjects from a wide range of social strata, elites as well as non-elites, urbanites as well as rural

¹ I thank Dvora Yanow and Oscar Salemink for pointing me to valuable sources used in this paper.

residents, dominant as well as marginal groups. Micro-level researchers seek out the most ordinary of citizens, not just the most articulate, educated, or cosmopolitan.

Because micro-level studies are often focused on interacting with and observing “ordinary people”—that is, people who may not be familiar with the rituals of “interviews,” the technicalities of “participant-observation,” and academic norms around publishing—micro-level researchers face ethical challenges that other researchers do not. This paper focuses on the researcher’s duty and obligation to protect subjects and to minimize harm, not only during fieldwork but also during write-up and publication. The paper argues that micro-level researchers need to view their responsibility to protect through a contextually-specific and long-term lens. This means remaining sensitive to how people in research communities are perceiving and interpreting the researcher’s presence, actions, and intentions, and adjusting one’s behaviour to allay fears and minimize unwanted attention. It also means remaining aware that the researcher’s duty to protect does not end with the researcher’s departure from the field, but continues, albeit in different guise, when the researcher begins presenting, publishing, and disseminating her work to a broader public.

The paper draws on nine months of fieldwork I conducted in Rwanda in 2004 and a four week trip I took to Rwanda in July 2008. During both trips, I spent time in two rural research sites and central prisons, located in the provinces formerly known as Ruhengeri and Gitarama.

When the “ethics” rooster comes home to roost

Ever since the formal instantiation of ethics rules and the establishment of Institutional Review Boards in universities across the United States, researchers have

had to comply with an institutional mandate to protect research participants from harm.² Chief among researchers' ethical obligations is the duty to minimize risks to participants by protecting participants' identities, privacy, and confidentiality. The formalization of ethics rules, guidelines, and procedures, however, does not guarantee more ethical research or more ethical behaviour on the part of researchers. The question of what constitutes ethical behaviour depends in part on the social, political, and cultural context in which the study is taking place and the researchers' sensitivities and abilities to respond to and operate within that context in ways that minimize risks to participants and to themselves.

The general risks of fieldwork are well-known. While in the field, scholars need to guard against theft or confiscation of their field notes or data; in the wrong hands, such data could pose great dangers to subjects (Nash 1981; Wood 2006, 381-82). Researchers also need to be wary of how their association with research assistants, interpreters, or other local staff might bring unwanted attention to these people. Neighbors can come nosy and jealous, while police or other officials may simply become suspicious. Researcher also need to protect people's identities while the research is ongoing, not a small task in small communities where everyone knows everybody or in settings where state-society relations are strained or characterized by violence. Meeting the obligation to protect sometimes requires ingenuity, quick thinking, and even courage. Brian du Toit (1980, 277-78), for example, writes about being followed by police while conducting a study on drug use in South Africa. Despite assurances from officials that no one involved in the study—including research assistants—would be harrassed in any way, two policemen stop du Toit and his assistant one day. One policeman asks his

² For a discussion of the effectiveness of these boards in Political Science, see the July 2008 issue of *PS: Political Science and Politics*.

assistant, who is African, to give his name. Du Toit instructs his assistant not to answer. The stand-off ends when du Toit phones the chief of police who, in turn, calls the two policemen's boss, who instructs the policemen to let the researchers go. By his quick thinking, du Toit was able to preserve his assistant's anonymity.

A somewhat similar incident happened to a colleague in Rwanda. While out in the field on day, the police arrested my colleague's translator. My colleague objected and accompanied both the translator and arresting officer to the nearest police station where the trio waited for a higher up to arrive. After three hours, the translator was released.

Elisabeth Wood (2006, 381) writes about the challenges of getting her data back to the capital from the countryside, during her fieldwork in El Salvador on the then ongoing civil war in that country. As Wood recalls, travelling back to the capital often required passing through military and sometimes insurgent checkpoints. The precautions she took included writing in what she calls "nearly illegible longhand" (Wood 2006, 381) and leaving out names and details that she would fill in later once safely back in the capital. Luckily, the men at the checkpoints never took an interest in her notebooks even when they searched her other belongings.

All three stories illustrate the lengths to which researchers will go to protect assistants and associates. They also illustrate the lack of actual protection that IRB regulations and guidelines provide to subjects and research assistants. For no institution would advise its researchers to risk their own safety for that of their subjects or staff. Yet, "on the ground" conditions oftentimes call for researchers to go beyond institutional requirements to protect.

Obligations to protect continue as scholars go to press with articles, books, and reports of their findings. This stage of research presents new and no less troubling ethical dilemmas. Stories of authorities using published findings to track down, harass,

and exact retribution on project participants are not uncommon. After publishing a book on Vietnam—in French, no less—anthropologist Georges Condominas found, to his horror, that the US Department of Commerce, without authorization and hence in direct violation of international copyright law, had translated the book and distributed copies of the translated version to green berets fighting the war in Vietnam. Condominas learns of this from one of his subjects whom Special Forces had tracked down and tortured (Condominas 1973, 4).

Wood relates a similar experience. Fearing that publication of sensitive material would bring risks to her informants (despite their consent that she could use the material in published work), she decided against publishing such data. Her fears were well founded. As Wood (2006, 382n14) writes many years later: “My caution was recently confirmed when a review of my second book appeared in an issue dedicated to understanding insurgency of a publication [sic] (*Special Warfare*, December 2004) of the US Army’s John F. Kennedy Special Warfare Center and School, which may well be read by Salvadoran military officers.”

For me, the question of protecting subjects re-surfaced unexpectedly when I returned to Rwanda in 2008 and revisited my original sites in preparation for a new book project. For the new project, I wanted to continue interviews with the same people I had interviewed in 2004 to build on those prior relationships.

My return was unexpectedly emotional for me. Apart from my research objectives, I was extremely curious to see what changes had occurred since my departure in 2004. My former interpreter had emailed me months earlier that many of the prisoners we had interviewed in 2004 had been released. I was curious to know how their release had affected their communities of origin and their own lives. I also knew that *gacaca* had finally commenced in my two research sites after I left in 2004. Gacaca was a government initiative to establish local or community courts across the country to try

mid- and low-level perpetrators, thereby relieving the large backlog of prisoners who had been languishing in Rwandan prisons for years. I wanted to know how that process had unfolded. I was also intensely interested in how people were doing in general. Were some of the elderly people I had interviewed still alive? Were others doing better or worse than they were in 2004? Had previous interpersonal conflicts died down or did they continue?

People greeted me with a great deal of affection. They embraced me warmly and quickly welcomed me into their homes for a visit. They offered me food and eagerly asked about my news. I asked about their news in return. At that time, I knew my book³ would be coming out in the fall. I told people proudly that the research I had conducted in 2004—research in which they had played such a crucial role—would be coming out as a book.

In my excitement to share my news, I made new promises. I promised to return the following spring or summer and bring copies of my book. This new promise immediately gave rise to new concerns. It suddenly occurred to me that just about any local reader could recognize those mentioned in the book's pages. I knew enough about Rwandan resourcefulness to know that despite the prohibitive costs of pursuing an education, there would be at least one person in each research site who would learn English well enough to read the book. I suddenly faced the question of "protection" in a new and more immediate light. Would people view what I wrote as a violation of my promise to them that I would not share what they said with anyone but my interpreter? Faced—literally—with the people whose lives and experiences made the book possible, I also became nervous about what people would think of what I wrote. Would they think I was fair in my analysis and conclusions? Would they think I had listened closely and

³ See Fujii (2009).

understood what they were telling me? These were the questions that weighed on me as I went from house to house saying warm hellos to people I had not seen in four years. Given the negative experiences other researchers have had following publication, I have come to realize that my worries were not unfounded.

In perhaps an extreme case of underestimating one's ethical obligations to research participants, sociologist Carolyn Ellis (1995) writes about her rude awakening when she returned to the small American fishing village where she had conducted extensive research many years before. Ellis had maintained relationships with several of her key informants and felt that she had made true and lasting friendships with several of them. Upon her return, however, she was alerted that another researcher (a former anthropologist who no longer "practiced") had taken up full-time residence in the town and had been reading aloud excerpts from her book to townspeople—and not any excerpts, but those he disapproved of and felt others would disapprove of as well. These excerpts contained intimate details of people's private lives and their sexual mores and practices.

When Ellis returned, she was dumbstruck by the reaction of those who felt betrayed by what she wrote. She had assumed that people in the town, which she calls "Fishneck," would never read her book or would not have even known that she had even published a book. Because the literacy rates in the fishing village were low, she did not think anyone would take an interest in her book. But she was wrong.

Not all her former respondents objected to what she wrote. Many let her know that they were fine with the book but others clearly felt betrayed. What Ellis realized from this experience is that her unquestioned pursuit of academic credentials and career advancement had blinded her to the possibility that what she was writing in her book would or could be experienced as hurtful to the people she was writing about. This lapse was not ethical in the institutional sense. It was ethical in the personal or human sense.

She had told people she was there to do research and presumably she had obtained “informed consent” from people before talking to them. Her lapse of judgment, then, was not in failing to follow the formal procedures of ethical research but in forgetting the human dimension of her fieldwork. Ellis forgot that the very interactions and conversations that for her constituted “data” were expressions of friendship by those who provided that data. And friends do not betray friends.

In my return to Rwanda, I suddenly became hyper-aware of my duties and obligations to protect—not because I felt some strong allegiance to IRB regulations but because, I, too, realized for the first time that people would be reading the words I had written, which I knew would soon be published. I suddenly realized that they, too, would have certain reactions, perhaps negative, to what I wrote. Such reactions might also change how they viewed me or give them pause to talk to me again. How could I reconcile my professional pursuits with my ethical responsibilities and how far did I need to go to protect identities and confidentiality?

Dilemmas of proximity

Because micro-level researchers are often engaging with ordinary people living in rural areas, strategies for protecting identities and confidentiality must fit the context. During fieldwork, some protections are simply impossible. In small communities, locals know who is who and outsiders quickly stand out. In such settings, it is difficult if not impossible for the researcher’s presence to go undetected or to hide the identities of those with whom the researcher interacts. Through rumors, gossip, and word of mouth, everyone can know in a short time whom the researcher has visited and with whom the researcher has conversed and spent time, no matter the precautions the researcher takes. An outsider asking questions and probing for information is hard to miss.

The conspicuous presence of the researcher

In Rwanda, my presence was conspicuous for several reasons. It was clear from my appearance that I was an outsider and more specifically, a Western foreigner. It was clear from the fact that I drove a car (albeit a very old one) and spoke French that I was a person of high status—a *muzungu* (foreigner or wealthy person) from Kigali or beyond. It was clear from my activities that I was seeking information from local people. People could and did see me talk to local officials. They could see me entering and exiting government buildings and offices. They could see me walk and drive down certain roads and stop in front of certain houses. They could see me walking along certain paths accompanied by certain people. They could see me go inside a particular *urugo* (the enclosure of a house) and then exit after a period of time, accompanied by at least one person from the household. People could see with their own eyes or hear from others which houses I had visited, which people I talked to, which people knew me, and which people I knew in return.

The first dilemma of my field research, then, was not to protect identities, since it quickly became apparent that that would be impossible. Instead, it was how to make sure that my proximity (or perceived proximity) to specific people in my research sites would not bring these people any harm. I was mostly concerned with drawing unwanted suspicions or attention from authorities to those I interviewed.

As time wore on, I also became aware of another risk. I did not want to incite jealousies on the part of those whose houses we passed on a regular basis but never visited. As Philippe Bourgois (1990, 44) explains, one of the most basic ethical considerations for field researchers is being “wary of the social disapproval foisted on our primary informants when they become the objects of envy or ridicule from the rest of the community because of the resources, prestige, or shame we [researchers] heap on

them.” I was hoping that my recurring presence was not bringing shame, envy or suspicion on any individual, household, or group.

One way such envy could arise was if people perceived me as favoring certain groups, such as survivors. During my interviews, it became apparent to me that some resented the perceived benefits and attention that survivors garnered not only from the government, but more importantly, perhaps, from foreigners who were the main funders of programs aimed at helping genocide survivors. As I discovered in my interviews and from attending a few gacaca sessions, others, too, felt victimized by the violence and killing they suffered. But because this violence did not form part of the genocide (according to government definitions), these “victims” did not have the same forms of redress for their own suffering and hardship. Their resentment could be exacerbated by perceptions that I favored genocide survivors (or their stories) over them and their stories.

The main risk people faced, I believed, was being imprisoned. In a country that lacks a judicial structure that can ensure the rights of the accused, being imprisoned is no small matter. Since the genocide, numerous people have been thrown into prison on the word of a single accuser and have spent years in prison without being informed of the charges against them (Fujii 2009; Tertsakian 2008). Add to this situation personal motives that people might have to get rid of rivals or enemies through imprisonment and the threat to anyone becomes very real.

I could not hide my presence or the identities of those I spoke with, but I believed I could act in such a way that could help to dispel the worst suspicions and perhaps minimize jealousies. The strategy my interpreter and I adopted was one of transparency in everything we did and said. We gave detailed introductions and explanations of our project. We presented ourselves and provided copies of our letters of permission to officials at all levels of the administrative chain. We always showed up when we said we

would; on the rare occasions when we could not make an appointment, we would try to send word through someone in the community. We also maintained a predictable schedule, working one week in one research site and the next week in the other. We tried to act in ways that matched what we told people we were doing. Such transparency is crucial in post-conflict settings where suspicions of outsiders runs especially high (Peritore 1990; Sluka 1990).

I also used any opportunity to demonstrate my trustworthiness to individual informants. In one interview, for example, an interviewee asked me about what we were talking about with others. His question was innocent enough; he knew we could not divulge any specific information. He was just asking in general. I responded that I could not talk about what I talked about with others, even in general, aside from what my research design specified (which my interpreter explained at every initial interview).

In a different instance, I was able to dispel a rumor that we gave money to people we interviewed by interviewing someone by mistake. This man lived next door to a woman we had interviewed several times. I call this woman “Émilie.” We had been looking for a man with the same name as her next door neighbor so we went to interview this man. It turned out that he was not the man we were looking for. A week or two later, we returned to talk to Émilie again. The first thing she said was that she was glad we had gone next door because it finally dispelled the man’s belief that we were giving money to the people we interviewed.

Despite our conspicuous presence, we nonetheless strove to protect identities whenever we could. The rare times, for example, when people asked us how we obtained their name, my interpreter always replied that it was the *responsable* or local official who gave us their name. This was not always the case. Sometimes we were trying to locate someone whose name had come up in another interview, but we took care not to divulge the connection to another informant.

Did my attempt to behave as transparently as possible work? I had the opportunity to follow up this question when I returned to my two sites in July 2008, nearly four years after I had left the country. The people I visited in each site were those I had interviewed several times. They were people I had gotten to know and like quite a bit. During our return to one site, however, we unexpectedly encountered one of the most direct forms of “surveillance” that I had ever experienced in Rwanda.

In the northern site I call “Kimanzi,” my interpreter and I presented ourselves to the local authority. He looked at our letter issued by the Ministry of Internal Security and remarked that it gave us permission for prison interviews, not interviews in his *secteur*.⁴ He asked us if we knew to whom we wanted to talk and I replied, “yes, the same people I had interviewed before.” He then assigned us an escort to take us to the people we wanted to find that day. Our minder was a young man who was an ardent booster for his community and clearly ambitious. Unlike many Rwandans who resort to speaking Kinyarwanda when another Rwandan is present, this young man spoke directly to me in French. This struck me less as a courtesy than a demonstration of his level of education and the ease with which he could communicate with foreigners.

Our minder asked us who we wanted to talk to and I mentioned a man we had talked with many times, both formally and informally, in 2004. This man had been living in an *umudugudu* (government constructed housing for genocide survivors) in 2004 but the *umudugudu* was no longer standing. Our minder took us to the man’s new house. When we arrived, his wife told us he was not at home. We left a message that we would return the next day. Our minder then asked who else we wanted to visit. Our minder’s

⁴ This official was the equivalent of what would formerly have been the *maire* of the *district* (formerly the *bourgmestre* of the *commune*). Under the new system, the *commune* had been redesignated as a *secteur*.

presence made me extremely uneasy. I had never been shadowed like this before and his demeanor made me wary of naming too many people for fear he would know too much about my past and current project. This young man was precisely the kind of overly ambitious local-level authority whose attention I did not want on myself or my informants. I imagined, and my interpreter agreed, that he would be anxious to report to his superiors any and all of what we discussed with people as well as their identities.

After we left the first house, I told our minder I would like to find a man I call “Jean Marie.” We had interviewed Jean Marie in 2004 many times. When we arrived at his house, Jean Marie and I recognized each other immediately. It was wonderful to see him again. He immediately invited us inside his house, which was under construction. We sat in a half-finished room with a single light bulb hanging from the ceiling. The minder stuck close. I was so uneasy by the minder’s presence that I did not want to ask or say anything except the most banal or general comments. I remarked on how big Jean Marie’s new house was and admired the fact that all the new homes in the area appeared to be wired for electricity, a great leap in development since 2004. I also asked Jean Marie if he was surprised to see me back. Jean-Marie smiled warmly and replied “what took you so long?” Throughout our brief and truncated conversation, the minder jumped in to translate even though my interpreter was present.

I felt so uneasy by the minder’s presence, I had to search for more things to say. I then remembered an informal conversation Jean Marie and I had had in 2004 about the fact that he had only one child. In 2004, he had told me and my interpreter⁵ that he did not want any more children, that one was enough. I recounted my memory of this conversation and then asked Jean-Marie if he still had only one child. He smiled again

⁵ This was a different interpreter than the one I worked with in 2008 so only he and I would have known about this prior conversation.

and said, “yes.” We laughed at his “advanced”—nay, in the Rwandan context, radical—attitude toward family planning. At that, we decided to end our visit. I told my interpreter later that other than the two men we had tried to find that afternoon, I had not wanted to mention any other names to our minder. I was wary of our minder’s motives for accompanying us. In this instance, it was quite obvious to me that my presence did present a clear and viable risk to the people we visited that day.

The next day, our minder left us on our own to my great relief and surprise. So we went to look for the man who had not been home the day before. We found him at home and were able to have a long conversation with him. During our visit, I began asking about others I wanted to see. One man I knew only as “Gustave.” We asked our host if he knew which Gustave I was talking about but he could not place him. He asked for more identifying details which I tried to provide but still he could not place the man. I was disappointed but pleased at the same time. Gustave was a man we had interviewed seven times in 2004. The fact that someone like our host could not place him reassured me that many details of my field research, including the identities of those we interviewed, might *not* be an indelibly etched in people’s memories. I was hopeful that my efforts to hide people’s identities in print would provide adequate protection and that my prior association with certain informants like Gustave had not brought anyone unwanted attention or negative consequences after my departure in 2004.

When I returned to my research site in Gitarama, I was able to talk more freely with the people I visited. One of the first questions I asked the people was whether anything bad had happened to them after I left as a result of having talked with me. Not a single person indicated that they experienced any difficulties. This was extremely reassuring to me. It is certainly possible that in the context of a social visit—and one that was occurring after a long absence—people would not have shared any problems they had had, but I felt confident that they would have, even if tangentially or in vague terms.

While I was mostly cognizant of the potential risks that my presence brought to those I interviewed and worked with, it is not always the case that a researcher's presence brings unwelcome attention. Wood (2006, 377-78) attributes her ability to conduct field research during the ongoing war in El Salvador in the late 1980s and early 1990s to people's willingness and desire to share their stories with her. This willingness stemmed not only from their desire to tell the history of the war—Wood's stated research objective—but also to relate stories of their own triumphs during and after that war.

Philippe Bourgois (1990, 48), too, writes about the positive reaction he received when visiting a Honduran refugee camp to explore whether it would be feasible to do research there. Far from viewing Bourgois's presence as endangering them, the refugees saw it as an added layer of security and urged Bourgois to stay and do research in the camps.

Yet, even in situation's where the researcher's presence is welcome, the answer to what are the most "ethical" choices is not so simple. Just as his presence brings added security so will Bourgois's absence deprive the same people of this layer of protection. In other words, even if Bourgois had heeded their wishes and decided to stay and conduct research in the camps, at some point, he would have left and whatever added security his presence provided would have disappeared with him. This is not to argue that Bourgois or any other researcher should refrain from inserting themselves into threatening or insecure research settings. It is to argue that even attempts to bring succor or aid will likely give rise to new ethical dilemmas because the ethical circle can rarely be fully squared. Similarly, while people enthusiastically shared their stories with Wood and willingly gave their permission for her to use their stories in her published work, Wood still had to decide which material she could publish which would not bring harm to her informants.

Assisting informants

Bourgois's potential dilemma to help or not to help the refugees by basing himself in the refugee camp illustrates another common dilemma that micro-level researchers face in the field. When is it appropriate for researchers to help informants (usually by giving them money) or is it ever appropriate to help informants this way?

The standard rule in journalism and academia is not to pay informants. The assumption is that paying informants compromises the quality of data. A person who is paid to talk might say anything—to please the researcher, to keep the money flowing, or both.

When researchers are attempting sustained micro-level research in poor countries or communities, however, the dilemma of whether and how to help is omnipresent. Conducting an ethnography in a remote Indian village, McCurdy (1981) writes of how his innocent attempts to help people with medicines for their many illnesses turned his house into an unofficial health clinic and made him a kind of “on-call” doctor. The main strain McCurdy experienced as a result of his “medical aid” was the infringement on his time. Once he had helped a few people, others started coming to his house at all hours seeking medicine or medical assistance. McCurdy then found ways to limit his “clinic” hours so that he could maintain enough time for writing up his fieldnotes. Interestingly McCurdy never talks about how this act of helping people—by providing them with medicines which he bought with his own money and dispensed at no charge—compromised what people were willing to say to him. Indeed, for McCurdy, the only downside to his informal but burgeoning “medicinal practice” was that it took away from his anthropological time. Once he found a solution to that problem, no dilemma remained.

For researchers who do not reside in their research communities, the dilemma of whether and how to help is harder to resolve through some sort of compromise. I faced many requests for help (mostly financial aid) from many respondents. I declined all of them, sometimes with great reluctance. The worst moment came early in my fieldwork. I had come to interview an older woman who was visibly gaunt and had been suffering from a stomach ailment. At our first interview, she graciously answered my questions and then at some point, gently asked my interpreter if I could help her buy some medicine for her stomach problem. I did not know what to say. My interpreter, who eventually became quite adept at gently turning down all requests, asked me again (in French) how she should respond to the woman. I cannot remember what I said. Indeed, I do not think I said anything that she could translate and so the request was left hanging. My answer, in effect, was no answer.

I felt horrible turning her down but because it was the beginning of my fieldwork, I did not think I could accede to her request. I knew that news of any assistance I gave to her or anyone, no matter how small, would quickly spread—or rather that was my fear. I did not want potential informants to believe that I would pay them if they agreed to talk to me. Neither did I want to feed any rumors that I was paying people to talk with me.

Such dilemmas, however, are never easily resolved. When I returned to interview the woman a second time a few weeks later, the first thing she did was to apologize for having asked me—her guest—for help at our previous visit. Her apology only made me feel worse. While I believed that my research required me to forego any acts of monetary assistance to informants (a choice with which my interpreter agreed), it was never clear to me that privileging one set of ethics (professional) over another (human) was the right choice. After all, this woman gave me her time and hospitality. My choice to refuse her request and all others only reinforced the one-sidedness of the relationship, which was already tremendously power-laden and completely in my favor. As a Western

researcher, I could expect people to give me their time and tell me their stories while at the same time, claim that I could not help them under any circumstances.

Fulfilling people's requests, however, does not resolve the dilemma either. It may simply raise new and perhaps equally troubling ethical dilemmas. VanderStaay (2005, 372) writes of his attempts to study a young man who was dealing drugs and who eventually ended up in prison. He writes with obvious regret of having "inadvertently provided the funds [this] teenage cocaine dealer used to buy crack from his supplier." VanderStaay faced the constant difficulty of trying to help his subject and the subject's family; the larger dilemma he faced was whether his duty to protect included a duty to help to ensure his subject's well being. The problem was that the various gestures of help that VanderStaay provided did not result in positive outcomes. Indeed, as VanderStaay recounts, the money he gave his subject may have contributed to the shooting of two people.

I myself took a narrow approach to the question of help or aid, believing that that was the "right" thing to do. Still, it was difficult facing people who were willing to talk to me and telling them I could offer no assistance whatsoever, particularly when their requests were not particularly costly in monetary terms (even on my limited budget). To prisoners who asked me if I could help expedite the court's review of their dossiers, I explained that I was not a lawyer, that I had no connection to the legal system in Rwanda, and that I had no authorization to engage in such activities. To others, my interpreter explained that my university and the government prohibited me from engaging in any assistance activities. Everyone seemed to accept our explanation—what else could they do? I then re-obtained consent to make sure that none of the people who asked me for help were agreeing to be interviewed only because they believed they would receive something in return.

That the researcher's presence is both a boon to some and a liability to others means that researchers face different ethical dilemmas with different people. The three different scenarios I sketch here—the “medicine man” anthropologist in the Indian village, the “social worker” anthropologist in an American city, and myself as a researcher refusing all requests in Rwanda—are bound up in different socio-economic and political contexts. The basic question that all three researchers face, however, is the same. When is helping the right thing to do and is helping still an ethical act even when the “aid” ends up bringing harm to others? No formal set of regulations and guidelines can answer these questions. It is up to the researcher to decide what is best and best for whom. Researchers must also be aware that whatever choices they make will not likely resolve the dilemma completely.

Dilemmas of publication

While the process of fieldwork poses its own set of ongoing ethical dilemmas and trade-offs, the act of publishing can weigh just as heavily on the researcher. Here again, professional incentives and ethical obligations pull in different directions. Researchers must publish to advance careers and to be seen as “real” and productive scholars. As Gary Fine (1993, 270) points out, “Data are not proper ‘sociology’ until they are published.” At the same time, the researcher must still maintain her responsibility to protect confidentiality. Indeed, it is during the process of publishing and disseminating one's work to a public audience that this responsibility comes to the fore.

Like most ethical obligations, meeting the responsibility to protect confidentiality is not a simple task. Researchers have a responsibility not to reveal the identities of those who provided their data yet at the same time, they also have an obligation to remain faithful to the stories that people share with them. It would be another kind of betrayal if the researcher inadvertently lost or destroyed the data she collected, or if she

was not careful with the details in her analysis or written work. The question is how far should researchers go to protect the identities of speakers while at the same time remain true to the stories these speakers share with researchers.

Hiding identities

In my written work (which began as a PhD dissertation and came out later as a book), I tried to do my utmost to hide all identities. I worked from a level of near paranoia that some ambitious civil servant in some Rwandan ministry would take it upon himself to comb through the pages of my book or dissertation to figure out exactly where I was doing my research and to whom I spoke. I therefore tried to take all the precautions I could think of that would maintain the integrity of the data but at the same time keep any ambitious bureaucrat from figuring out the identities of those whose stories I include in the book.

I took multiple steps to hide locations and identities. I used pseudonyms for the research sites. I also deleted or altered any references in the data to neighboring locations or landmarks that could help to identify the two sites. There were two exceptions to this rule. I do mention the Virunga mountain range and its proximity to my northern research site. The mountains, I reasoned, were not a dead giveaway since the mountains ring many communities, not just my research site. I also did not use a pseudonym for Bugesera, a region to the east of my Gitarama research site. Both of these references certainly narrowed down the general vicinity of my research sites but not to the point where they could allow an outside reader to pinpoint the exact locations.

In addition to using pseudonyms for place names, I also used pseudonyms for informants I refer to in the book. I did not assign pseudonyms to everyone, only to those whose stories or interviews I quote at length. Pseudonyms not only helped to protect

identities, it also made the narrative more readable and allowed me to refer back to the same people throughout the book.

My rule for choosing pseudonyms was fairly straightforward. I used French names since for the most part, Rwandans of a certain age have French first names. I picked names that did not approximate the person's real name⁶ in any way but were among names that one could find in Rwanda. Choosing pseudonyms is not a task to be taken lightly. Ellis (1995) chose names that sounded similar to the person's real name so that she could more easily keep the names and pseudonyms straight. Unfortunately, this choice also made it easier for people from her site to recognize the people in her book.

In addition to using pseudonyms, I also withheld or obscured biographical details when doing so did not detract from analysis. One type of detail I could obscure without undermining the integrity of the data, for example, was the number of children a respondent had. Instead of referring to "Richard, father of five children," I might refer to Richard as having "several" or "many" children. Other biographical details, however, were not possible to suppress or obscure. If an informant had been the *conseiller* during the civil war or genocide, I could hardly change or withhold that information for as *conseiller*, that person's words and deeds may have carried great weight in the community. Another type of detail that I could not suppress was whether an informant had some tie to a local leader since such ties formed a core argument of my book. Thus, if a person had had a close tie (through family, friendship, or some other means) to the *bourgmestre*, I could scarcely afford to leave out this information.

⁶ We collected only first names. Our familiarity with people's first names and our lack of familiarity with their last names made it difficult at times to track the same person down again. We had to qualify common names like "Félicien" or "Jean" with the person's age, occupation, *cellule*, or indeed, all three extra identifiers to locate the right person.

With all these precautions, I realized that I could never obscure identities to the point of ensuring that local residents could not recognize some of the people in the book. Doing so would violate the integrity of the data themselves. So the question then becomes what is enough when it comes to protecting identities in written work? Did I go far enough? At first, I believed I had but upon my return trip, I had second thoughts.

Self-censorship

Researchers not only seek to publish their work, they may also wish to return to the same country or communities to extend their studies or start new projects. In many countries, researchers require government approval to conduct research. In such cases, researchers have an incentive not to get on the “wrong” side of government officials or policies lest they be prevented from continuing research in that country.

My ability to return to Rwanda, for example, depended on the consent of the government. While Americans can travel to Rwanda without a visa, the government can easily deny me permission to enter the country or to conduct research once I am there. The government could also obstruct my research in other ways. They could assign me a permanent minder, which would stop my research cold. They could instruct local authorities not to cooperate with me; local authorities, in turn, could instruct their residents not to talk to me. None of these possibilities is unique to Rwanda. They can occur in any country.

When aligned with the professional imperative to publish, the edict of staying on the government’s good side becomes doubly powerful. Researchers might even find themselves self-censoring. They might refrain from analysis that is overly critical of the government or remove references to practices that conflict with the government’s public image. The RPF, for example, is very sensitive to any references to war crimes it

committed during the civil war that brought it to power in 1994. To what extent do I refrain from including data on or references to RPF war crimes in my published work?

The pressures not to run afoul of host governments may be even stronger for junior scholars whose career trajectories often depend on returning to the same country. The costs of learning a new region, country, or community are high as are the benefits of building on one's prior knowledge and experience within the same country.

I did not consciously refrain from taking a position on potentially controversial subjects in my written work, nor did I remove data or findings that were critical of the Rwandan government. At the same time, my book did not focus on the current regime; its focus was largely on prior regimes and actors. Informants nonetheless made mention of RPF war crimes and abuses and I incorporated those data into my analysis where appropriate—that is, where it served the argument. But I was not unaware that including such material could in fact get me in trouble later.

My worry about publication was not only about maintaining my own access, however; it also had to do with protecting the people who worked with me. During the course of my fieldwork in 2004, I had heard stories of authorities imprisoning interpreters and research assistants who had been working with foreign researchers or NGOs. In one instance, the NGO employer was not able to get its employee out of prison. I did not want anything like that to happen to anyone who had worked with me. This did not make me alter any of my analysis, but it did make me consider carefully the potential risks of including certain pieces of data in my book, because I knew I could not always protect people from any risks, particularly once I had left the country. What the prospects of publication reminded me in no uncertain terms was that my obligation to protect did not end with my departure from the field; it continued, albeit in different form, during the period of write up and publication.

Verdicts and standards

Ellis (1995) writes about her uncomfortable return to the small fishing village where she had conducted her dissertation research. She had mistakenly believed that because literacy rates in the community were low that no one would read her book or even know that a book existed. She was wrong. When she returned years later for a visit, she found, much to her horror, that many people whom she had considered friends for years were angry at her for what she had written. At least one of her former “friends” refused to speak to her again.

Ellis’s article is a reflection on what she could have and *should* have done differently. For the first time, she realized that her goals as researcher came before her role as friend and adopted member of a small community. She realized that the one-sidedness of her work allowed her to skirt over the ethical dilemma of inhabiting the dual and sometimes contradictory roles of friend and researcher. In the latter role, it was her professional “duty” to disclose; in the former, her duty was to keep people’s stories between them.

While much of my experience was quite different than Ellis’s, her piece brings up an important aspect of field ethics that scholars rarely discuss. Thinking about the people in my research sites reading my published book made me realize that I had made the same choice as Ellis. I had adhered faithfully to professional norms that valued detachment (i.e., refusing requests for help) and publishing (i.e., disclosure) above all else, without ever contemplating what those in my community would think about my rendering of their community and their stories in written form. I had taken complete ownership of the data; I was the final arbiter of what to disclose and how to interpret the data. In fact, this was my job as a scholar.

What returning to Rwanda in 2008 made me realize, however, was that the sense of obligation to those who gave so generously of their time to me did not reside in institutional regulations or IRB dictates. Rather, it resided in my desire to maintain relationships with these people no matter what. While I was interested in collecting more “data,” I was also interested in maintaining people’s faith and belief in me as a trustworthy person. What my return trip taught me was that the fate of micro-level researchers and their work is in the hands of those we study. It is they who ultimately decide whether we have met the ethical standards to which we aspire.

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