SPECIAL ISSUE: POLITICAL ETHNOGRAPHY I

The Ethical Challenges of Field Research in Conflict Zones

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Abstract   Drawing on 26 months of field research in El Salvador during the civil war, I analyze some ethical challenges that confront field researchers working in conflict zones. After briefly summarizing the purpose and general methodology of my research, I discuss in detail the research procedures I followed to implement the "do no harm" ethic of empirical research. I first analyze the particular conditions of the Salvadoran civil war during the period of research. I then discuss the procedures meant to ensure that my interviews with people took place with their fully informed consent—what I understood that to mean and how I implemented it. I then turn to the procedures whereby the anonymity of those interviewed and the confidentiality of the data gathered were ensured to the extent possible. Throughout I discuss particular ethical dilemmas that I confronted, including issues of self-presentation and mistaken identity, the emotional challenges of field work in highly polarized settings (which if not well understood may lead to lapse in judgment), and my evolving questions concerning the researcher role and its limitations. I also discuss the dilemmas that arise in the dissemination of research findings and the repatriation of data.

Keywords   Ethics in research · Civil war · Political violence · Qualitative research methods · Field research

Field research in conflict zones is challenging for both methodological and ethical reasons. In conflict zones, the usual imperatives of empirical research (to gather and analyze accurate data to address a relevant theoretical question) are intensified by the absence of unbiased data from sources such as newspapers, the partisan nature of much data compiled by organizations operating in the conflict zone, the difficulty of establishing what a representative sample would be and carrying out a study of that sample, and the obvious logistical challenges. Similarly, the ethical imperative of research ("do no harm") is intensified in conflict zones by political polarization, the presence of armed actors, the precarious security of most residents, the general unpredictability of events, and the traumatization through violence of combatants and civilians alike.

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As should always be the case, researchers in developing their research design and methods should take account of ethical imperatives from the beginning of the project's development. In the hope of contributing to further research in conflict zones, in this essay I discuss ethical dilemmas that confront field researchers working in conflict zones and assess the extent to which research procedures can adequately address those dilemmas. I argue that whether research procedures can address many such dilemmas depends critically on the particular conflict setting; there are some settings where research cannot be ethically conducted and should not be attempted or should be curtailed. In many settings, on the other hand, research procedures can address many dilemmas reasonably well but ethical research always depend critically on the judgment of the researcher; following abstract rules will not be sufficient. Thus training of field researchers should explicitly prepare them for anticipated ethical dilemmas and also instill ethical principles to guide their judgment in the field.

For this essay I draw on my experience of 26 months of field research in rural areas of El Salvador during the civil war. After briefly summarizing the purpose and general methodology of the research, I first discuss the particular conditions of the war during the period of research. I then discuss ethical dilemmas that I confronted and analyze the adequacy of the research procedures I followed to implement the "do no harm" ethic, with an emphasis on the protocol meant to ensure that my interviews took place with the fully informed consent of those interviewed and the procedures with which I sought to ensure the confidentiality of my field data. I identify dilemmas that I did not encounter, stressing the relatively constrained violence in the latter period of the Salvadoran civil war. I also discuss the dilemmas that arise in the dissemination of research findings and the repatriation of research. As those procedures do not address all dilemmas the researcher may encounter in conflict zones, ethical research inevitably depends on the informed moral judgment of the researcher. I identify some of the emotional challenges of field research in highly polarized settings that may undermine the researcher's judgment.

The research: high-risk collective action and democratization as a way out of civil war

In El Salvador I carried out research on a variety of issues, including why democratization resolved violent political conflict in that case (Wood, 2000) and why some poor residents of the countryside actively supported the leftist insurgents despite the very high risks they thereby ran (Wood, 2003). In five case-study areas in contested regions of the countryside, I interviewed more than 200 civilians, eight insurgent commanders, and approximately two dozen staff members of non-governmental organizations of various political allegiances. The case-study areas varied in the degree of mobilization in support of the insurgency and in the form of agrarian production.[1] Through interviews and observation of meetings, I documented the history of the conflict in the local communities, the political trajectories of individuals (both those who supported the insurgents and those who did not), political divisions within communities, the relations between civilians and the armed actors, the pattern of insurgent

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[1] The five case-study areas met the following research criteria. Each area had to be reasonably accessible to me in my small truck and should be politically manageable (in the Salvadoran context this meant that only one or two of the five insurgent factions should be active there). Across the case-study areas, I sought variation in the social relations that prevailed before the war (wage labor, share-cropping, peasant agriculture, etc) and in the patterns of political mobilization (that is, areas where some civilians actively supported the insurgents and some the government). However, this research design emerged through a more convoluted process than this indicates, as explained in chapter 3 of Wood (2003).

land occupation during the war, and the origins, evolution, and political affiliation of non-governmental organizations active in the area. I also gathered material to analyze the origins and evolution of the reconstruction of a bombed and abandoned village (Tenancingo) under a unique agreement between the insurgents and the governments to allow its repopulation.

In San Salvador and provincial cities, I gathered data and documents on the evolution of the economy during the conflict, the course of negotiations between the insurgents and the government over land transfer to insurgent supporters, landlords' perceptions of the conflict and its economic consequences, and military officers' analysis of the origins of the war and the proper strategy of counter-insurgency. I interviewed government officials, military officers, USAID staff members, UN staff members, insurgent political officers (after the cease-fire), as well as approximately a dozen landlords of properties in the case-study areas.

While I was able to gather a number of primary documents and databases and to observe many meetings of different organizations, my principal research method was that of the semi-structured interview in which I asked open-ended questions from a prepared list and pursued topics in depth as seemed appropriate and relevant. Returning several times to interview many respondents was essential to the quality of information eventually gathered.

It was important for the purpose of the research to gather perceptions on these issues from social groups across the political spectrum (including the armed actors). As I discuss in more detail below, except for high-level public officials, I assured all those I interviewed that their identity would be kept confidential (in many cases, their participation was anonymous). Initial contact with USAID officials led to interviews with Salvadoran government and military officials as well as landlords. My initial entry to my first case-study, Tenancingo, was facilitated by the non-governmental organization carrying out its reconstruction. The local Catholic nun introduced me to residents. My affiliation with the Jesuit university facilitated contact with pastoral workers in other areas, with whom I stayed in the countryside. Various contacts in San Salvador introduced me to members of non-governmental organizations working in the other field sites, who then facilitated my entry to the other four case-study areas. In the countryside, I was vetted by the insurgent faction present in each area. After listening to my explanation of the project, field commanders checked with urban contacts to confirm my identity.[2] I assume a similar process took place among government officials.

Thus prospective interviewees were identified through the construction of several parallel and evolving networks of contacts (urban and rural, insurgent-affiliated and government-supporting, and rural residents who supported neither side). In the circumstances of political violence and polarization, I did not attempt to construct representative samples of local respondents but did my best to interview members of a wide variety of organizations, both those affiliated with the government and those affiliated with the opposition. Local priests and nuns introduced me to a variety of local residents, including people who supported neither side (otherwise not easily identified). I did not carry out research in areas controlled by the insurgent army, as not supporting the insurgents was not an option in those areas. Nor did I attempt to conduct field research in an area where insurgent activities were entirely absent (which would have been the ideal research design, as it would have added a clearly contrasting case). Ethnographic research on such politically sensitive questions in areas of uncontested government and landlord control would have been dangerous for those interviewed (and perhaps for me). That is, ironically, field research in the conditions of the Salvadoran war during the period of my research was generally safer in areas contested by armed insurgents.

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[2] Once that process was completed, as far as I was aware there were no restrictions placed on my movement or research.

The reason is that a military and political stalemate of sorts prevailed in the case-study areas in the latter half of the war, a political space that reflected the inability of the government to reimpose the hegemony of security forces and landlords but also the inability of the insurgents to prevent government troops from controlling the area. In the shadow of that stalemate in the conditions of this particular war, rural residents with a variety of political loyalties could live and work, and field research on patterns of collective action was possible.

I also drew on another method. In 1992 insurgent activists from four of the case-study areas drew maps for this study. I asked representatives of a dozen cooperatives to draw with marker pens on large sheets of butcher paper maps of their localities showing property boundaries and land use before and after the civil war. Drawn collaboratively by at least two and usually several members in a process interspersed with much discussion of the history of the area as well as gossip, jokes, and teasing of one another (and of me), the resulting maps document how cooperatives of insurgent supporters literally redrew through their collective action the boundaries of class relationships.[3] Drawing maps was not a familiar task; only a few said they had ever seen a map. Insurgent field commanders had a few, well-worn maps held together with tape (the cover photo of this issue shows insurgent political officers using a map as they discussed with me the evolution of the conflict in the local area). One non-literate elderly leader traced property lines with his forefinger while his grandson drew the line in its wake. Each pair of maps took two days to draw, a sacrifice of time that I understood as an indication of their enthusiasm that the history of the war in their area be documented.

In retrospect I am aware of many mistakes I made during my field research, of which two deserve mention. I wish I had better recorded the comments, jokes, and discussions that occurred during the map-drawing sessions. I was slow to understand the value of those interactions as I initially had an overly simple understanding of what the maps represented. Only later did I come to understand the maps as documents demonstrating the emergence of a new political culture rather than as illustrations of land occupations. I also came to wish that I had interviewed more residents of the case-study areas who did not support the insurgency, as the material I gathered from this group was generally less rich than from supporters.

Field conditions in comparative perspective

Field research across this range of political allegiances during a bitter civil war proved possible because of particular aspects of the Salvadoran war. My research began in 1987, after the intense violence of the early years of the war had declined. The principal reason for this decline in violence was the US decision in late 1983 to convince Salvadoran military leaders to rein in the military's human rights abuses. The large majority of violence against civilians in the Salvadoran war was carried out by the state; the UN-sponsored Truth Commission attributed more than 85 percent of civilian deaths to state actors and their agents. While the military did not dismantle the death squad networks responsible for the violence, violence decreased as the military reluctantly embraced counterinsurgency measures that emphasized winning the "hearts and minds" of civilians over the indiscriminate violence of the early years of the war. The insurgent organization also reorganized its forces into small more

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[3] The accuracy of the claims by these cooperative leaders to occupy extensive areas of land in 1992 was confirmed by my own travel and observation in the case-study areas and by examination of the land claims data held by the insurgent group, the government, and the United Nations during the postwar land transfer process.

mobile and autonomous groups. This new strategy soon proved effective and the insurgents slowly extended their covert presence in both rural and urban areas, carrying out brief sorties against military forces and intensifying economic sabotage.

In this latter period of the war, state violence was generally much more selective, although extensive and egregious violence recurred when the regime felt threatened. For example, during the insurgents' 1989 offensive, the government's Atlacatl Battalion, on the order of the High Command executed six Jesuit scholars, their housekeeper, and her daughter on the campus of the Jesuit university, and the Air Force bombed civilian neighborhoods of San Salvador. Nonetheless, the changes in government and insurgent strategies made it possible for many displaced peasants to return to the countryside. Despite ongoing violence against activists, insurgent supporters in some areas began forming insurgent cooperatives and formally declaring abandoned land (some of which they had been working covertly) as their property.

Thus my field research occurred during a period of relatively organized and limited violence, a sharp contrast both to the indiscriminate violence earlier in the war and the more criminalized violence after the war. On occasion, people on both sides of the conflict with whom I had contact would suggest that my planned trip to certain areas be postponed. I always took such advice without further questions; I noted that fairly frequently, violence would occur in my case-study areas during those times.

In particular, my initial field research in Tenancingo took place in a context of ongoing international pressure on the armed actors to end their violations of human rights. This pressure, together with the fact that the military admitted bombing the town on two occasions made possible a unique agreement between the armed groups to allow the reconstruction of the town and its repopulation in the midst of a very conflicted area. The project received significant European funding and press coverage, which may have raised the costs to both armed actors of hostility to an academic researcher. That I was not from El Salvador but from the United States may also have contributed to the feasibility of field research in the town for two reasons, the importance of US funding to the government and the attention given to harassment (by either side) of US citizens.

Field research in the other case-study areas took place later in the war; indeed, much of it (though not all) occurred during the cease-fire that began in early 1992. Although political tension was often very high in those case-study areas, the presence of UN observers and the separation of the two armies to distinct sites made travel to those areas much less dangerous than it would have been earlier. And, with a few minor exceptions, I had excellent luck: I was never caught in the wrong place at the wrong time. Such luck is a not-to-be-underappreciated aspect of fieldwork in settings of political violence (Sluka, 1995).

However, in addition to these reasons particular to the second half of the Salvadoran war, I believe my research in contested areas was possible for a more profound reason. My inquiries met with the enthusiastic collaboration of many residents of the case-study zones (and of nearly all those approached in San Salvador as well), irrespective of class, occupation, or political affiliation. Residents acted on a willingness (perhaps even a need in some sense) to discuss with an outside researcher their own history and that of their families and communities. Perhaps this willingness is a measure of the trauma and change brought by the war. Those interviewed frequently expressed a desire for their story to be told, that some account (or accounting) be made of the local history of the civil war. For example, one Tenancingo resident not allied with any political faction told me in 1987, "The people here are suffocating from the cries and shouts that we cannot speak. It suffocates. It does me good to talk to someone—I can't speak to people here about these things." A longtime insurgent activist remarked

I understand that you are asking us to consider participating in the construction of what we might call a history of the war in the conflicted zones. [pause] There are no more hidden things. We have suffered so; it would be right that there be such a history. What a period we have lived through! The *campesino* [peasant] does not have the capacity to do it; you engage with such things more there [in the U.S.; in U.S. universities]. But it is something we have lived and we are still living. I don't know where to start . . .

This willingness of many residents of contested areas to talk about their personal and community histories at length with a researcher (given the right introduction and setting) is common to many other ethnographies of civil wars.[4]

Yet the statements that civilian members of the insurgent cooperatives made in interviews were concerned not only with violence. While most stories they told began as histories of injustice, violence, suffering, and loss, many continued as proudly told stories of the achievements of opposition organizations during the conflict—of land occupied and defended, of new organizations founded, and of new identities asserted. The *campesinos* recounted these achievements with enthusiasm. With groups I interviewed repeatedly over months and sometimes years, support for this project was particularly evident on my return after an absence. I would often be met with shouted greetings such as "Well, Elisabeth, do we have something to tell you!" or (to each other), "What did we say we should remember to tell Elisabeth?"

These assertions of pride contrast sharply with ethnographic material gathered in many other studies of civil war, for example, the ethnography of war widows in Guatemala by Linda Green (1999). The felt achievements of insurgent civilians in my case-study areas was an enabling condition of my field research: many residents who supported the insurgency felt they had important and triumphant stories to tell. The terms of the political settlement that ended the war allowed both sides to claim victory of a sort: the insurgents claimed they had earned the right of participation in a fully democratic polity, while government supporters parried the insurgents' economic demands (Wood, 2000).

Thus the Salvadoran conflict differs from many civil wars in several relevant ways.[5] After the initial years of the war, state violence became much more selective. Both because of their ideological commitments and their reliance on civilian support, particularly for intelligence, the insurgents were particularly selective in their use of violence and built extensive networks of civilian support (Wood, 2003). The fact that there were merely two actors, each with a fairly coherent chain of command, meant that the vetting process was apparently thought by the armed actors to be an adequate basis on which to decide whether or not to cooperate with the project. This pattern of limited and well targeted violence by two coherent actors during the latter half of the Salvadoran conflict contrasts sharply with the patterns of violence in many other civil wars. In many wars, leaders exercise little control over armed followers, armed factions perceive few ideological or practical constraints on their use of violence, armed groups rely on the use of indiscriminate terror (particularly if their goal is to control resources or to carry out a campaign of ethnic or political cleansing), or armed groups rely on the kidnapping of foreigners to finance their activities.[6] In some wars, the multiplicity of factions makes difficult any sustained presence in rural areas. And in some wars, neutrality on the part of rural residents and researchers is rejected by one of more armed party to the

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[4] See for example Green (1995), Nordstrom (1997), and Das (1990).

[5] For further discussion of the challenges of ethnography in wartime, see Wood (2003: chapters 2 and 3). See also Nordstrom (1997), Peritore (1990), and Smythe and Gillian (2001), and the essays in the collection by Nordstrom and Robben (1995).

[6] See Kalyvas (2006) and Weinstein (in press) for discussion of patterns of violence in civil wars.

war. In particular, in some conflict zones at the present time carrying a U.S. passport may invite violence whereas in the Salvadoran context it raised the costs of such hostility.

Other conditions less directly related to violence might also make ongoing field research during other wars exceedingly difficult if not impossible. Where there are a variety of local militias and other armed actors, gaining adequately authoritative permission to carry out research may be impossible. The logistics of field research in some conflict zones may also be prohibitively difficult.[7] I was able to identify field sites to which I had adequate access most of the time by public transportation or riding with members of non-threatening non-governmental organizations (Tenancingo), or driving myself in a small truck. In many wars, such relative ease of access and autonomy of movement is not possible. Rather, researchers must negotiate transport with non-governmental organizations that some actors may perceive as biased. Finding secure and reliable sources of shelter, water, and food that will not bias the research may be very difficult. Those providing hospitality and transportation may impose restrictions on where the researcher may go and with whom he or she may interact. Other factors also enabled the logistics of field research in El Salvador. That very nearly all rural residents speak Spanish meant that fluency in one language obviated the need for a translator. In contrast, in some conflict zones the field research must conduct interviews in a wide variety of languages and dialects and therefore will probably need to engage translators, with attendant practical and ethical challenges.

Ethical challenges

In my research, the most important challenges in following the "do no harm" imperative were to ensure that those I interviewed and the organizations I observed had given me their informed consent to my research project, to protect the politically sensitive data that I gathered, and to decide what material to publish. In short, I sought to ensure that those who participated in the project did not run any greater risk as a result and that potential research subjects made their own informed decision to participate. I discuss each in turn, along with some lesser challenges and dilemmas. (I did not work with research assistants or "key informants" whose security might require particular consideration.)

For field research to be ethical, research subjects must consent to their participation in full understanding of the potential risks and benefits (Kelman, 1972; Belmont Report, 1979[8]). In the context of my field research, this norm of *informed consent* meant that those I interviewed should understand the purpose of my research and the potential risks that they ran in talking with me (as well as any potential benefits) so that they could make a fully informed decision as to whether they wanted to speak with me.[9] The challenges of implementing this norm were numerous: what were the risks and benefits of participation? Would illiterate and marginally literate rural residents understand the informed consent process or would it alienate potential participants? Research protocols, including oral consent procedures, were

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[7] Severine Autesserree, personal communication.

[8] See National Commission for the Protection of Human Subjects of Biomedical and Behavioral Research (1979), known as the Belmont Report.

[9] Such procedures are required for researchers based in the US through a mandatory review of research proposals involving human subjects by institutional review boards, which must either approve project procedures or rule the project exempt. Although ethnographic research that poses minimal risk to human subjects can be reviewed under expedited procedures, that is clearly not the case for research in conflict zones. See National Research Council (2003) for discussion of the conditions for expedited review.

approved by institutional review boards at the University of California at Berkeley, Stanford University, and New York University.

The approval of and adherence to protocols is of course not sufficient to ensure adequate ethical judgment; such protocols cannot anticipate the many dilemmas other than issues of informed consent and data security that arise in the course of research, particularly in conflict zones. I found my ability to judge field conditions and to understand the challenges and dilemmas of field research increased over the course of my work. I was aided significantly in this process throughout my field work by the fact that the rural residents I interviewed had a more highly developed sense of the evolving risks of violence in their area than I did. Their political expertise in this far exceeded mine, which was too inexperienced and too naive, and I did my best to learn from them.

The consent procedure that I used with rural residents was oral, not written, as any written record would link participants to my project and would comprise a risk to participants. (A secondary reason was that the majority of civilians interviewed in the case-study areas were at most semi-literate.) The stated purpose of the project was the writing of a history of local communities through the war for a US university that I hoped eventually also to publish in El Salvador. I presented myself as an academic researcher working for an academic degree in the United States (initially an MA, later a doctorate), affiliated also with the Jesuit university in San Salvador.[10] The stated benefit was the writing of this history; I explicitly stated that no other benefits were available.[11] Near the end of my field work, various international aid agencies were beginning to explore development projects in my field sites so an explicit denial of any connection between such benefits and my project was important. Of course I cannot know that I entirely succeeded; in any case, no such benefits materialized over the many months of my field work, and those I approached for subsequent interviews nonetheless all agreed to be interviewed again.

The principal emphasis of the oral consent procedure was on informing those who partici-pated in my project about potential risks. I emphasized that I would be gathering information for the history from all actors in the conflict, including the armed actors of both sides. I assured potential interviewees that their identity would be kept strictly confidential, that I would in no way identify them to anyone else, verbally or in print, as having been part of my project or as having said particular things. The one exception was high-level govern-ment officials who were often interviewed by journalists. I also assured interviewees that they could choose what to tell me and whether it was for publication (without attribution to ensure confidentiality of identity) or merely for my own information (not to be used in publication) and in the later case whether or not I could write it down. I stated explicitly that they could refuse to participate at all and to decline to answer particular questions. I assured them also that they could change their mind at any point and decline to participate further (and withdraw permission to use previously disclosed material).

The consent procedure included a statement that was meant to ensure my accountability to local residents. I assured potential interviewees that if they had questions or complaints they could approach local pastoral agents who knew how to contact authorities of my home university. I gave two nuns and a local priest, chosen because they appeared to be respected by residents who did not support the insurgents as well as those who did, a letter informing

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[10] I initially feared that this affiliation would discourage government officials and landlords from speaking with me: the government and rightist elites throughout the war repeatedly denounced the university as supporting the "terrorists." While some landlords shared their opinions of the university with me, none declined to participate in the project on these grounds.

[11] I did, however, provide lunch for those drawing maps in the workshops.

them how to contact the relevant body at my university (in US parlance, the institutional review board).[12] To my knowledge, no one ever approached them with concerns or questions about my research.

While the discussion of this consent protocol initially caused some interviewees some confusion, once the idea had been conveyed that they could exercise control over the content of the interview and my use of it, participants demonstrated a clear understanding of its terms. In particular, many residents of my case study areas took skillful advantage of the different levels of confidentiality offered in the oral consent procedure. This probably reflected the fact that during the war residents of contested areas of the Salvadoran countryside daily weighed the potential consequences of everyday activities (whether or not to go to the field, to gather firewood, to attempt to go to the nearest market) and what to tell to whom. Moreover, I had an abiding impression that many of them deeply appreciated what they interpreted as a practice that recognized and respected their experience and expertise. Although for many telling their histories involved telling of violence suffered and grief endured, I did not observe significant re-traumatization as a result, as have researchers in some conflict settings (Bell, 2001). I believe the terms of the consent protocol may have helped prevent re-traumatization as it passed a degree of control and responsibility over interview content to the interviewee.

The second fundamental ethical challenge was to ensure the security of data gathered, particularly sensitive data that might have political implications if in the wrong hands. In my case, the most challenging aspect of this dilemma was how to manage the security of data gathered in the countryside and brought to the capital, often through military checkpoints and occasionally through insurgent checkpoints. Such data included interviews in which the person's political preferences were evident, particular biographical details such as participation (past or present) with one of the armed groups, indications of covert membership in an armed group on the part of apparent civilians (including staff members of non-governmental organizations), information about the relationship between non-governmental organizations and the insurgents, data concerning land occupations, and criticisms of the armed organizations by residents (including by their supporters).

In addressing this challenge, the confidentiality of the material gathered and anonymity of my research subjects was of course key. Names were often not recorded at all, if they were recorded they were always recorded in a separate notebook, usually after my return to the capital. I did not tape interviews in the countryside (and only on two occasions in the capital); rather, I took sketchy notes in nearly illegible longhand, filling in missing details once back in the capital. I used fresh notebooks on every trip into the countryside so as not to expose to risk previously gathered data once again. In each notebook I first took notes on newspaper reports, government documents, and so forth in the capitol, leaving some pages empty for interview notes. While my belongings were occasionally searched at checkpoints, no one ever gave more than a cursory glance at my notebooks. The notebooks were kept in a locked cabinet in a locked office at the university; copies were sent home every few months and the originals carried out of the country in hand luggage whenever I left. (Of course field researchers today protect their laptops with passwords and encrypt their notes and send them home by email.)

Rural residents themselves chose to reject anonymity in drawing the maps for the project. After extended discussion in the map-drawing workshops, those who drew the maps decided to write the names of their cooperatives on the maps. And in some cases, again after

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[12] In the Salvadoran setting, it might not have been appropriate to ask the local priest or nun to support the project by taking on this role: in other areas, pastoral agents were clearly allied with one party or another.

discussion of the potential risks, the map-drawers decided to write their own names on the map. As the names of all cooperative members had already been passed by non-governmental organization to the government land transfer office, I judged the risks of including names on the maps to be minimal. I interpreted their decision to do so as a claiming of agency in re-drawing the boundaries of properties, class, and land use in the Salvadoran countryside.

Lesser dilemmas also arose in the course of field research. For example, I was occasionally mistaken for a nun or a lay religious pastoral representative. I always corrected any such misunderstanding before I spoke at length with anyone, but I did not always go out of my way to correct it during passing encounters in the field. For example, at times combat occurred in the immediate proximity of Tenancingo and on occasion either the insurgent or military forces occupied the town. I did not tell combatants who saw that I was staying with the local nun and talking with a range of civilians that I was not a church worker. So while I never directly misrepresented myself, I did not actively correct all misunderstandings of who I was and why I was there.

Field researchers often have to decide whether or not to challenge lies that they are told in the course of their work. This is both a practical and ethical dilemma: should the researcher confront the liar it might result in hostility toward the project and perhaps toward participants. This dilemma occurs with particular force in interviews with perpetrators of violence. My resolution was not to challenge lies told to me but to invite elaboration in a bland and naive way, which incidentally often led to extremely useful material reflecting the speaker's ideology, values, analysis of events, and so on.

The third fundamental dilemma I experienced was the extent to which I would include sensitive field materials in publications. Some decisions were simple as they were essentially dictated by the conditions given by research subjects (some material was excluded from publication by the interviewee). I distributed very few copies of my MA thesis on the repopulation of Tenancingo until well after the end of the war.[13] And I waited nearly a decade after the end of the civil war to publish some of the most sensitive material, particularly the relationships between various non-governmental organizations and the insurgents. But I decided not to use some material, although I had permission from those interviewed to do so, because it seemed sensitive even in light of El Salvador's sustained peace since 1994.[14] A particular dilemma arises when interviewees insist that their name be used and the researcher judges that to do so may run a risk of harm to that interviewee.

A fourth and related dilemma was how to thank those who made my research possible. This was an ongoing discomfort for me as I felt that I could never in any meaningful way reciprocate the generosity of those with whom I worked, be they residents of the case-study areas or government officials. This was particularly troubling with respect to the residents of my case-study areas who generously lent their scarce time to my project. I attempted to reciprocate in shamefully minor ways such as giving people rides in my small pickup. I came to believe that for many of the rural residents I interviewed, sharing their life story with an engaged listener was some sort of service that I provided in the course of my research, as described above.

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[13] I gave copies to the non-governmental organization that had carried out the reconstruction and to members of my MA committee. The official copy of my MA thesis held by the University of California at Berkeley was held by the library under an agreement that it would not be made available for five years after its filing.

[14] My caution was recently confirmed when a review of my second book appeared in an issue dedicated to understanding insurgency of a publication (*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.

Anthropologists and other ethnographers endorse a particular form of reciprocity: materials gathered in the field should be returned to the community of origin. Some years ago this was understood as making publications available to the academic communities of the countries where field research was conducted. This was easily achieved: copies of the two books were given to major national libraries. An article-length version of the first book was published in Spanish in the leading academic journal in El Salvador. The second book is now being translated for publication in Spanish in El Salvador. Increasingly, however, ethnographers agree that the field researcher's obligations extend far beyond the dissemination of publications to include the return of field materials themselves. But the norm says both too much (not all material should be returned, for example, confidential material should not be) and too little (returned to whom and when?).[15] In my case, there was no question of returning field notes given the confidentiality of much of their content. I did return the maps to those who drew them, after photographs had been taken.[16] I should add that I have yet to pursue what I believe would be a truly reciprocal act, to publish a version of my second book accessible to semi-literate people.

A final, diffuse, and ongoing dilemma during my field research (and after) concerned my understanding of my role as researcher. In carrying out research in conflict zones, the researcher inevitably comes to wonder why the research is worth pursuing over purely humanitarian relief work. And in settings where violence against civilians is overwhelmingly carried out by one party to the conflict, as in El Salvador, researchers may wonder whether they should actively support the other party rather than continue with less politically engaged forms of research. Some researchers take the long view and argue that research is nonetheless justified because a sound understanding of conflict is essential to successful intervention and the recreation of social fabric (Smyth, 2001, pp. 3–4). I agree with that but note that my own belief in the value of what I was doing was sustained by the ongoing endorsement of the project by rural residents willing to spend many hours telling me the history of their families and communities. Their belief in my project helped me stay on course in many ways. I persisted in my role despite attractive offers to become involved in policy work, such as serving as a consultant on land issues to the UN mission. Their belief kept me clear that my core value was the purpose of the project, to document the history of the war in the case-study areas.

Yet there were many ethical challenges that I did not confront during my field research. I did not have to make a decision whether or not to intervene to attempt to prevent or mitigate an attack on civilians, for example. I did not have to decide how to leave an area under attack at short notice, retreating with one force or seeking shelter from another. I never faced direct threats that I turn over material I had gathered. I did not have to judge how far to press respondents about violence they had suffered or observed because the focus of my research was on the emergence of voluntary collective action in the high risk circumstances of the war, not directly on patterns of violence. The absence of these dilemmas of course reflects the relatively benign and coherent conditions of my field research. To carry out research in other civil wars, researchers may need to enter the field with and reside with a non-governmental organization that may impose explicit restrictions or implicit constraints on the research,

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[15] See the essays collected in Jaarsma (2002) for extended discussion of the difficulties in implementing this norm.

[16] I initially regretted this decision (although I never doubted it was the right thing to do) as the photographs when developed showed merely slightly colored lines on a background of light gray, as might be expected of a photograph of a hand-drawn map. Fortunately, once the photographs were digitized restoration proved possible (thanks to Carolyn Resnicke of the Santa Fe Institute).

thereby compromising the researcher's independence in order to carry out the research at all. Conditions in many civil wars simply preclude ethical field research.

Emotional challenges in field research

In carrying out field research, ethnographers often go through predictable periods of loneliness and perhaps depression during which they question the meaning and feasibility of their project and whether they are adequate to the task. Such fieldwork "blues" typically occur a few months after arrival once the first excitement has lessened, and also after their return home. In part, these periods reflect the stress and loneliness of making a transition between cultural settings, leaving family members and friends behind. Many ethnographers find the emotional reserve necessary in field research (to engage subjects empathetically while retaining one's scholarly purpose—what question must I be sure to ask, to what extent should I steer this interview) emotionally draining.

Those carrying out extended field research in conflict zones are likely to experience additional and intense emotions in the course of their work, including fear, anger, outrage, grief, and pity, often through observing, suffering, or fearing the effects of violence. Indeed, field researchers in extreme cases may suffer from "secondary trauma," the sustained effects on witnesses of observing gross human rights violations. And field researchers often feel tremendous stress in their efforts to keep their data secure (as when carrying their data through check points.) In sharply polarized settings, researchers may also feel it stressful to "manage" information from both sides and to engage in interviews with all parties. I experienced all these emotional challenges during my field research despite its setting of relatively limited and coherent violence.

I mention these emotional dynamics because I am persuaded that inadequate attention to them may lead field researchers to make errors in judgment that may have significant consequences for their research subjects as well as themselves. In such emotionally challenging circumstances, most people are susceptible to flattering invitations to share their experiences (and inevitably their data), to entertain new friends with stories (and data) from their field site, to embark on friendships or relationships that may be perceived as compromising the project, or to "make a difference" by passing on field data "confidentially" to some (supposedly responsible) person.

Good field researchers find ways to manage these challenges and take care to shelter their ongoing research from their emotional vagaries. In addition to the affirmation I took from the enthusiasm of residents of the case-study areas to participate in the project, I found that a few close friendships with those who were similarly positioned as sympathetic "outsiders" but whose own interests were far from mine (technical assistance and pastoral work) were helpful in my maintaining the confidentiality of my material and staying in role among my research subjects. Occasional weekend holidays gave me perspective as well as rest. During my most extended period of field research (18 months), it was particularly important to return to my university to consult with colleagues not just for academic advice but for some particular political advice about some of these ethical challenges, and to reaffirm a sense of engagement with my academic community.

Conclusion

Informed consent protocols and security procedures help ensure that the field researcher "does no harm." However, conflict zones differ enormously in their conditions. The contested

areas of El Salvador during the second half of the civil war saw forms of violence that were generally more limited, better targeted, and more predictable than was the case earlier in the war and in many other conflict zones. So I was spared facing some ethical dilemmas that other researchers confront.

The research practices described appears to have been sufficient to address the ethical dilemmas encountered during my field research. But they might well not have been sufficient for conditions at other times and in other places. I did not attempt field research during the 1989 insurgent offensive, for example. There are field conditions in which ethical field research is not possible.

Even with research practices and protocols tailored to specific field conditions, inevitably field researchers rely on their judgment in interpreting those norms. Yet very often academic training does a poor job preparing us for field research, particularly in conflict zones. Training in field research methods and in the ethical dilemmas frequently encountered in the field should comprise part of graduate training for social scientists and other professionals who venture into such settings. In particular, such training should also instill an awareness of the emotional dynamics in field research in conflict zones and their possible effects on the judgment of the field researcher.

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