

THE FOUR SQUARE LAUNDRY: PARTICIPANT OBSERVATION IN A WAR ZONE*

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I awoke one morning to see him in my bedroom pointing [a submachine gun] at me. "Right," he said, "you're a Four Square Laundry job." This was an allusion to being an army spy.

—Frank Burton, quoted in Jeffrey A. Sluka, 1995

Thanks to his dangerous and frightening experiences in West Belfast, Frank Burton's ethnographic research on Northern Ireland is considered legendary.¹ At first glance the incident Burton describes would seem mad to anyone who has not spent time living and working in the Catholic ghettos of Belfast. However, as alarming as this event may seem, it speaks more to the rapport Burton established with his respondents than to the perils of fieldwork. In actuality this was a prank brought about by one of his Irish Republican Army (IRA) informants.

The hazing of researchers is a common practice in Belfast, and anyone who conducts inquiries of this nature is bound to collect a few such "war stories" (Sluka 1989, 1995). The obvious reason for such a vetting is that the IRA feared that a British undercover operative disguised as an academic would infiltrate the organization. Having said that, I believe that researchers are not only checked out as potential spies but also tested to see whether they have the "salt" to stick it out when the political atmosphere makes day-to-day life difficult. In other words, the researcher has to prove that, when placed in a life-threatening situation, even for just a moment, she or he won't simply pack up and go home.

There are obvious dangers in conducting participant observation in a violent social context. However, the researcher "not only observes the behavior of the group that she or he is studying, but also participates, as much as possible, in the daily lives of the community members" (Dowler 1999, 195). When I lived in Belfast, it was still a turbulent and violent study area.² The violence notwithstanding, uneventful encounters with one's respondents, even notorious ones, were the earmark of participant observation (Dowler 2001). When you live and work within a community for an extended period of time you acquire local knowledge. To my surprise, community members would often point out individuals who were "involved," which was usually accompanied with a humorous tale that, interestingly, never related to their

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status as an IRA volunteer.³ A neighbor might point someone out as a volunteer but then proceed to tell how he or she sings off-key after “a couple of pints.” Many former IRA volunteers readily identified themselves to me once I started frequenting a local prisoners’ club (a private club for former political prisoners). I heard about which university their children would attend in the fall and their holiday plans (Dowler 2001). Such open-ended interviews, rooted in respondents’ everyday lives, are more productive than the highly sensationalized image of the researcher in a clandestine meeting, with hooded and armed men, an image adopted by reporters and some academics to introduce “how they made contact with the IRA.”

Although there is a great deal of research on how to put a respondent at ease, there is little discussion about how to make the interview comfortable and safe for the researcher. By getting to know my respondents in a mundane setting I felt far more comfortable delving into more political and personal areas, including reflections on their participation in political violence. I simply would have never asked these types of questions in a more threatening setting. Knowing my respondents, I shed my preconceptions of them as agents of violence. The irony is palpable: Conducting participant observation, one might expect to be living on the edge, but instead one simply lives with the threat of violence as part of workaday life.

REPRESENTING THE FIELD

Asked to include in this essay a photograph of myself in the field, I canvassed the images I had collected over the years. Most of them were of landscape elements, such as political murals. Photographs of community members tended to be shot from a distance, usually in large, public gatherings. Closeups were rare and only acquired with the express permission of the respondent. Susan Sontag once argued that “to photograph people is to violate them” (1977, 1). This violation not only was an ethical consideration but also seemed incompatible with the method I was employing. As a participant observer, I lived and worked within this community. I firmly believed, then, that the collecting of individuals’ images would objectify them and render them exotic. This seemed to a sure way of reinforcing the static insider–outsider boundary that participatory researchers admirably endeavor to transcend (Back 1993; DeLyser 1999). I also felt that snapshots of me in the field would have yielded a similar result. Countless numbers of visitors to West Belfast have their photographs taken in front of the many political murals. I decided that this type of photography would not be in my best interest because it would mirror the behavior of tourists rather than locals, thereby reinforcing my outsider status.

Eventually I found a snapshot for inclusion in this essay, one that was taken by a member of the family with whom I lived and is illustrative of day-to-day life in West Belfast (Figure 1). In West Belfast, as a female researcher, the simple act of “hanging a proper laundry” was as much of a test of my credibility as was surviving hazing events such as Frank Burton’s Four Square Laundry job. While conducting fieldwork in a violent social context, the researcher for the most part

engages in ordinary actions even though she or he must always be prepared to negotiate the extraordinary.

REFLECTIONS

As I write this essay I am reminded of Ted Swedenburg's struggle when reflecting on his experiences of working with Palestinians. Swedenburg warns against "lapsing into this sort of autobiography merely as a narcissistic exercise" (1995, 27). For this reason, my intent here is not to simply tell war stories (Back 1993). Instead, I hope to encourage emerging geographers, despite the very real dangers involved in executing this type of research, to focus their studies in areas where they have immediate social relevance. Recently, as part of another research project, I attended the national meeting of Women in the Fire Service. At this meeting a female fire chief explained the motivations for women who enter the fire service: "We are not in this to be heroes, we do this as an act of service." In a similar way, I believe that academics who conduct ethnographic studies of violence are not on a quest for adventure; rather, they hope to foster social change. Swedenburg details two risks that confront us: "The first is that a sensational or heroic aura might, without justification, become attached to me because I have worked in treacherous, frontline field sites. . . . The other peril is that a researcher can sometimes be tainted with the dangerous images associated with his or her informants. . . . I hope that someday someone will document how aspiring graduate students, from various disciplines, were warned by advisers not to do research on Palestinians" (1995, 25–26).

Most researchers do not enter into violent social contexts to affirm a quest for adventure. More to the point, there is usually some personal impetus driving this type of research. One influence that contributed to my study of human suffering could well challenge my status as an objective researcher (Gilbert 1994). I was weaned on stories of my grandfather's involvement in the IRA during the revolution that resulted in Ireland's independence from Britain. My mother didn't describe my grandfather as a hero of the revolution; instead, she detailed the pain and suffering she endured as a result of his participation in the conflict:

One day I was walking down the street in Limerick City. I was only a young girl of about twelve, and it was not long after Dada had been killed. Pascal Ryan came up to me and said, "Ah Maura, it is a terrible thing about your Da, he was a very brave man. Limerick owes him a great debt. Do you know, a couple of years ago I was there when the Black and Tans had your Dada pinned in the church. I thought he had had it, but sure enough your Da stiffed those bastards and got away, There wasn't many men like Joe Ryan." Can you imagine how that made me feel? This was the man who put me to sleep at night. I would sit at his knee, and he would tell me the most wonderful stories. I know that Pascal meant well, but you just don't want to think of your Dada killing someone. I went home and cried my eyes out that night.

My mother's resistance to the everyday enactment of violence is not unusual. But in the academy, Western epistemological analysis of war has centered on sweeping generalizations which insulate academics from any research that is grounded in



FIG. 1—In the field. (Photograph by a respondent, July 1993)

people and the way they experience the enactment of violence. Yet exploring the everyday construction of violence is critical for the researcher who seeks social change. Too often people's experiences of violence are homogenized to meet the needs of nationalist agendas. Conflict in Northern Ireland is often reduced to masculinist geopolitical interpretations; women's issues are placed on the back burner in favor of an imagined nationalist solidarity.

That is why I traveled to West Belfast: I wanted not to speak and write of violence as something extraordinary but, rather, to examine the day-to-day construction of it. It was my hope that by examining violence "on the ground" I could destabilize the intractable explanations of this violence that reduced the nationalist question in Northern Ireland to British hegemony and Irish resistance. My goal was to demonstrate a commonality of experience through the gendering of political identities in war whereby all women, regardless of nationalist or religious affiliations, have been erased from the events of this conflict (Dowler 1998). I would argue that the reclaiming of women's identities presents a ripe opportunity for social change.

THE WAR ZONE OF THE ACADEMY

Swedenburg warns that the flip side to the researcher's being cast as a hero is being tainted by the dangerous images associated with his or her informants. He argues that working with Palestinians was not always considered the "best career move for

aspiring academics”; “guilt by association with Palestinians affected academic hiring and promotion,” and “the taboo on this subject severely circumscribed academic discussion of the issues” (1995, 26). I doubt that working with the IRA was any more politically correct than associating with the Palestinians. Yet, astoundingly, instead of my politics becoming suspect, my femininity came under attack. It was as if my exposure to agents of violence had made me more “manly.” Upon my return home, colleagues from both within and outside the feminist community mentioned that I should dress in ladylike fashion when presenting “dark and difficult” testimonies. In fact, one female professor, who conducted research in the former Yugoslavia, counseled me to wear skirts instead of slacks, so that I did not appear “overtly masculine or militaristic” (Dowler 2001). Another ethnographer explained to me that when she returned from studying the war in Mozambique she experienced a similar phenomenon. One of her older male colleagues tapped her on the nose “with a newspaper” and asked, “Why is a nice girl like you doing this kind of work?” Ironically, academics who construct the identity of the participatory researchers as masculine are in the end reinforcing the gender stereotyping of women and war, which is exactly what feminist researchers are endeavoring to dismantle (England 1994; Nast 1994).

At the time I dismissed these comments as minor annoyances. On reflection, perhaps geographers, as members of a discipline, need not only to interrogate the positionality of the researcher vis-à-vis respondents but also to question our positionality within the discipline. As Cindi Katz argues,

Whereas anthropologists engage in relentless autocritiques of the practices of ethnography, ethnographically inclined geographers must still defend these methods within the discipline at the same time as they guard against the naïve appropriation of this problematic methodology from anthropology. As a geographer I not only confront a legacy of field research that I want to stand on its head, but I also engage in a peculiar intra- and interdisciplinary politics concerning ethnographic methods and the nature of qualitative research. (1996, 178)

To earn interviews with individuals who are usually reticent is no small task. It takes a great deal of time—time spent just hanging around and being seen, setting the stage for your first question, for a misinformed question could negate all your work, if not your life. The investment of time constitutes a gamble that may not be immediately rewarded by the academy. While I was in the field, painstakingly waiting and watching for the right opportunity to conduct an interview, many of my contemporaries were back home, publishing from secondary sources. When screening job applicants, some search committees make their first cuts based on the number of publications and on how quickly candidates finish their degrees. For others there is a sentimental and favorable gaze over the field-worker as someone who has not yet been fully distracted by the postmodern moment, while still others simply argue that qualitative research is anecdotal and not a rigorous methodological approach. Before the 1970s ethnographic methods were lodged mainly in the disciplines of anthropology and sociology, but research in human/cultural geography

has changed significantly since those days. A review of two of our leading journals—the *Annals of the Association of American Geographers* and the *Geographical Review*—demonstrates that a presence of empirical studies that are qualitatively derived went as high as 24 percent in the *Geographical Review* and 20 percent in the *Annals*.⁴

Although there is an apparent burgeoning of qualitative studies, researchers still find trouble with some areas, such as promotion and tenure decisions. As Cindy Katz once warned, the time commitment in participatory research is “time that might be spent publishing but is instead spent perishing” (1996, 176).

As a result of the pressures to publish, time spent in the field in the early part of one’s career is usually reduced to short visits for verification and not for conducting new studies. Newly minted lecturers and assistant professors usually need to demonstrate that they have “moved on” from their dissertation research. Interestingly, this is sometimes equated to “moving out.” There may be pressure to abandon the site of one’s original research rather than to ask new questions in the same place—an ironic conundrum for a discipline which argues that “place matters.”

Not only is earning the trust and confidence of respondents time consuming, it cannot be transferred to another venue. For researchers who subscribe to an activist agenda, “moving out” leads to ethical questions of academic dilettantism. The restraints of tenure can compete with the needs of the communities we study. Can we as academics consider ourselves activists if we simply leave “the field” after a year or two? Geographers need to define and evaluate the very nature of participatory and emancipatory fieldwork as it relates to careers. This question is not new, of course; it has been a focus of many a panel discussion at meetings of the Association of American Geographers. For the most part, these discussions are empowering to young field-workers; at times, however, they can, to use Swedenburg’s words, lapse into “a narcissistic exercise” (1995, 27). Is the activist researcher’s primary commitment to social change? Should we also commit to social change within the academy? I grow concerned when I witness established researchers advising younger social scientists not to concern themselves with structural systems such as tenure. Rhetoric of unconcern does nothing to change the system within which we work.

A strategy that has allowed me to operate within the structure of the university has been to frame my research within ongoing initiatives. Most universities and colleges have programs that promote community outreach and service. By adopting the established institutional vocabulary I have been able not only to maintain the integrity of my research but also to tap into internal funding for field projects.

Swedenburg expresses concern that graduate students are being detoured from participatory and emancipatory work. I was never cautioned away from conducting this type of research. My graduate adviser, John Western, was extraordinarily supportive, even as he insisted that I proceed cautiously—a philosophy I am now endeavoring to adopt with my own students. I would advise any student who is considering this type of work to select a committee whose members are not only ontologically but also epistemologically compatible with his or her research objectives.

FIELD STRATEGIES

Among the practicalities of conducting this type of fieldwork are dealing with difficulties. Academics who choose this type of investigation may encounter threats. My work in a war zone posed not a threat not to me personally but possible jeopardy for my informants (Chrisman 1976; Cook 1997). I never recorded actual names, places of the interview, or physical characteristics of my respondents in my field notes. At the end of each day I would mail two copies of my notes to colleagues in the United States for safekeeping. I adopted this process upon the advice of the American vice-consul. A year earlier he had been called to intervene when security forces confiscated the notes of another American academic. To lose one's field notes clearly would be detrimental to the research project; it could also adversely affect one's respondents.

Upon my arrival in Belfast, I was advised by certain community members to meet with IRA representatives from the area. I simply walked to a neighbor's house, where I met with several local men and women who asked general questions about my work. I was asked to present my passport and university identification, which were examined in another room. I was then told if I needed anything to just let them know. Later, when I started to frequent IRA prisoners' clubs, I was checked out more thoroughly. One evening a known IRA member approached the group I was sitting with and announced that he had just received his orders to "shoot" several British operatives. He threw down a piece of paper with some writing on it, which he implied was the names of his intended victims. By no means had any of my academic training prepared me for a moment like this. I still wonder whether I acted out of sound methodological training or was just too frightened to move; the latter, I think. When I simply looked away, the people around the table roared with laughter! I, like Frank Burton, had just been had in the name of good fun. However, even being the brunt of a good joke didn't make me more worthy of trust for the women in this group. Only when I had experienced what it was like to be a woman in this conflict did they entrust their experiences to me, as this excerpt from my field notes reveals:

A car sped past us, and I remember hearing the screeching of the brakes. Sean screamed, "Run!" and grabbed me by the arm, and we ran. I don't remember much, but I do remember him pushing me over a wall and running through a field. I could hear the repeated screeching of brakes getting closer to us. I remember thinking maybe I should just stop and let them get a clean shot, maybe that way it wouldn't hurt so much to die. As I slowed down Sean grabbed me by the arm and said, "What are you doing? You'll kill us both." I don't remember much after that, just running and finally making it to a housing estate where we took shelter. . . . Since Sean's release from prison he had been placed on a Loyalist "hit list," and I, like so many of my female respondents, had almost been caught in the cross fire.

Let me interject here that I do not tell this story from any sense of bravado. The event demonstrates the effect it had on my work, especially my relationship with my female respondents. Right afterward, Sean and I went to a local prisoners' club. As

Sean recounted what had happened, all the men listened intently and poured him drinks. I was invisible; it was as if I had not been part of the event at all. As on so many other occasions, a woman was being written out of a war episode. At that moment, though, the women who had been so distant to me came over and brought me to their table. They gave me a vodka, and one woman said, "You'd think you weren't even there." I don't pretend to know what it's like to be a woman living in West Belfast—after all, I could have packed my bags and gone back to my safe office in the United States at any time. However, for these women I had, albeit for just a moment, experienced what it was like to be one of them.

CONCLUSION

In 1980 Father Faul and Father Murray, two Northern Irish Catholic priests, condemned academic research for remaining distant and thereby safe:

We want to tell the truth, to give the worm's eye view, now, not in twenty years time when the scholars can safely gain their Ph.D.'s from documenting the Irish question, not in fifty years time when the politicians can shout from safe eminence about the coverups and the ill-treatment of the poor. The scholars and the historians and the universities have stood back from the storm in academic safety. Let us speak from the sweat and tears and misunderstandings, the jealousies, and the quarrels of the situation as it is. Now is the moment of truth. (Faul and Murray, quoted in Sluka 1995, 3).

If academics insulate ourselves from the everyday experiences of war, do we not run the risk of perpetuating stereotypical images of life under those conditions? We need firsthand research, not filtered, received material. We must conduct work in the places where conflicts are occurring. There are about 120 armed conflicts in the world today. In the words of Fathers Faul and Murray, "*Now is the moment of truth.*"

NOTES

1. The "Four Square Laundry" was a front for an undercover intelligence-gathering operation in Belfast. The operation came to light in 1972, when the IRA killed five of the undercover agents involved (Faligot 1983, 30).

2. The Irish Catholic community of West Belfast is understandably reticent toward strangers, which is what led me to choose to repeatedly enter the area as a participant observer. This sort of observation, in conjunction with informal interviewing, provided me with the least threatening method of obtaining information in a community that distrusts outsiders. In this essay I discuss three research trips between 1991 and 1998, a period that covers times of heightened conflict as well as the promise of peace. Most specifically, in 1991 I lived in Divis Flats with a family for three months. In 1994 I returned to live with the same family for eight months, in one of the new, two-story brick houses that had replaced the high-rise apartments. In 1998 I returned once again to live with the same family for six weeks.

3. "Involved" is a local expression which meant that a person was either currently a member or had been a member of the IRA.

4. The percentages of articles that utilize ethnographic methods in the *Geographical Review* are: 1990, 4; 1991, 0; 1992, 0; 1993, 7; 1994, 7; 1995, 10; 1996, 11; 1997, 7; 1998, 24; and 1999, 0. The percentages of articles that utilize ethnographic methods in the *Annals of the Association of American Geographers* are: 1990, 4; 1991, 4; 1992, 13; 1993, 11; 1994, 0; 1995, 0; 1996, 11; 1997, 0; 1998, 4; and 1999, 20.

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