

Methodologically Becoming: power, knowledge and team research

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ABSTRACT *This article explores the path of methodological and epistemological negotiation travelled by a team of four geographers conducting research among people with transnational connections between northern New Jersey and El Salvador. Having illustrated that all data are contextual, feminist scholars have explored the power relations in which data collection is embedded in order to situate knowledge. The relationship between the dynamics of research teams and the broader political struggles with which they engage, however, remains a blind spot within feminist field methods and writing strategies deployed to ‘see accountably’. The authors argue that there is an undertheorised relationship between the politics of academic research projects and the broader political movements with which they engage that may serve as a fertile intersection for feminist research. They explore relationships between team, field, and institutions in the context of negotiating difference among team members and their aspirations for the project. The article contributes to discussions of power, knowledge construction, and the politics of conducting fieldwork as a team by relaying experiences both from the perspective of individuals on the team and the team as a whole. The authors depict their objectives, successes, failures, and research politics; all part of a process of methodological becoming.*

Introduction

A research presentation is in every respect the very opposite of an exhibition, of a show in which you seek to show off and to impress others. It is a discourse in which you expose yourself, you take risks ... The more you expose yourself, the greater your chances of benefiting from the discussion and the more constructive and good-willed, I am sure, the criticisms and advice you will receive ... I will on occasion—I may do it next time—present the research work that I am presently conducting. You will then see in a state that one may call ‘becoming,’ that is muddled, cloudy, works that you usually see only in their finished state. *Homo academicus* relishes the finished. (Pierre Bourdieu, in Bourdieu & Wacquant, 1992, p. 219)

We should begin by saying that some aspects of our work were radical, dynamic and exciting. We decided to conduct research that would, among other things, contribute to progressive social change, more specifically towards more equitable access by Sal-

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vadorans to the American nation state via political asylum. Other elements of our experience, however, conveyed the reality of inertia, of things tending to continue on in the same direction. An object in motion tends to stay in motion, until it faces considerable resistance. The object, in this case, is the methodological approach we adopted.

Members of research teams frequently come together from diverse backgrounds, with different skill sets. Academic norms and the sets of institutions and individuals with which they interact in the field also influence research behaviour. As a research team conducting fieldwork with Salvadorans leading transnational lives, we grappled not only with various power dynamics operating between the team and the field, but also amongst us. Scholars engaged in collaborations, however, rarely publicly revisit such methodological difficulties of a completed project [1]. Rather, most methodological essays tend to be solitary reflections on experiences in the field. This article attempts to break some of the silences surrounding academic collaborations.

Our contemplation of collaborative models of data gathering/knowledge production and the politics of team research settles around competing influences and objectives of activism and academia (cf. Blomley, 1994). Signalling a broad definition of ‘politics’, we now find the links between the operation of the research team and the politics of the broader movement to which it contributes to be fertile ground for conceptualising new starting points for feminist models of collaborative knowledge production. We illustrate substantive problems that provoked us to reflect upon our methods to situate knowledge production within relations saturated with power (Katz, 1992). We explore these relationships to ‘see accountably’ (Haraway, 1991), to understand and present data as contextual, and to advocate for change in future collaborations. Like Bourdieu, we conceptualise research as knowledge production in a perpetual state of becoming (Bourdieu & Wacquant, 1992).

After addressing the literature on collaborative research, we discuss the politics of data collection in the context of contributing to political struggles. We explain that some of our methodological challenges related to the background of the research project and team and resulted in a struggle over quantitative and qualitative data collection. Cross-sections of time in the field reveal encounters saturated in power that often lurk, discursively muddled, in cleaner presentations of data. They illustrate the recursive relationships between the research process, the politics of the field, the politics of the research team, and the broader political and geopolitical contexts in which they unfolded. Interspersed throughout the text are individual statements by each of the four researchers that show not only how dynamic the methodological process can be, but how dynamically it can be interpreted. Finally, we discuss how post-colonial and feminist ideals might shape research teams in an effort to advance the conceptualisation of collaborative, flexible models of knowledge production.

The ‘Black Box’ of Knowledge Produced by Research Teams

Might showing some of the junctures where power operates in an analysis open up new ways of accountable seeing? (Kamala Visweswaran, 1994, p. 82)

Population geographers, and particularly those studying migration, increasingly advocate mixed methodological approaches to research problems (e.g. Lawson, 1995; Graham, 1999). Mixed methods often involve individuals with varied skills, distinct training, and epistemological biases. Difference is a productive asset to any team; but to be a creative

force, differences among members must be acknowledged, discussed and valued. Fieldwork embodies a process of continuous negotiation wherein respondents and researchers move through a mutually constructed field of exchange, each determining only partially the direction in which to travel. Team research introduces another layer of relationships to fieldwork, complicating decision-making, communication and data collection.

The mechanics of a research team are complicit with knowledges produced: what data are collected, where, and how. Research teams can be variously constructed (Waters, 1999), but most function hierarchically. A review of papers about team research revealed explicit discussions about the operation of power in relationships among members to be rare [2]. In 1982, Bradley wrote, 'Up to this point, our understanding both of team research itself and of its problems has been seriously hindered by an almost complete lack of systematic research on the sociology of team research' (1982, p. 93). Reflecting on the challenges faced by research teams in the field of sociology, Bradley identified the vulnerability of individuals within the collective imbued with professional codes, including the exploitation of graduate students and junior faculty, which he called 'an unnecessary but nonredressable evil of team research' (1982, p. 87). He advocated a written agreement that outlines the terms and conditions of research, arguing that 'to be forewarned is to be forearmed' (1982, p. 88).

Twenty years later, the internal dynamics of team research remain largely unexplored. Since 1982, the paradigms within which social scientists discuss methodology have shifted. While interests in career advancement such as those identified by Bradley (1982) linger, feminist scholars have modelled contributions to social justice through research (see Fonow & Cook, 1991; Gottfried, 1996). This paradigm shift is notable in Grossman *et al.*'s (1999) reflection on a feminist research project that involved participants who had been sexually abused. The authors outline the challenges to operational equality on research teams that attempt to confront researcher subjectivity, intimacy and relationships directly.

Donna Haraway's call for situated knowledge has had a tremendous impact on discourse surrounding feminist methodologies (1991). Few research teams, however, have taken up her requests for a 'doctrine of embodied objectivity that accommodates paradoxical and critical feminist science projects' (1991, p. 188). Rather than address challenges on the team, in the field, and in the write-up, most scholars advance in their published works what Bourdieu identifies as 'the finished' (Bourdieu & Wacquant, 1992, p. 219), akin to Latour's 'black box' (1987, p. 4) of science. In other words, scientists divorce product from messy process for the sake of a clean performance (cf. Latour, 1987; Barnes, 2001). This, though, is to gloss over other important aspects of the story, and to decontextualise data.

In contrast, we mull over the murky process of team research to do justice to our research presentations. Thus, rather than provide readers with 'just the facts', we try to depict here dynamic, animated 'science in the making' (Latour, 1987, p. 4), deploying Haraway's metaphor of sight (1991) as a means by which to expose the contents of Latour's black box (1987). Audiences of research presentations can only 'see' data accountably by learning more about the standpoints from which research teams see and the webs of meaning connecting multiple, partial knowledges and perspectives of team members. Following Kamala Visweswaran's attempt to 'see accountably' by exposing 'the junctures where power operates' (1994, p. 82), we enter the black box of team research and attempt to situate our interactions within the context of various hierarchies operating within and beyond the framework of the academy that brought us together (cf. Castree, 1999) [3].

My expectations were high. I thought we had a good idea. The ‘transnational literature’ was emergent, and I had become very excited about transnationalism via earlier research with Alison. El Salvador had a lot to offer. The tragedy of the civil war and the flight of so many people was a story that had not been told well. Adrian had experience in southern Mexico working with Salvadoran refugees and had developed an exciting hypothesis on refugees as circulators (Bailey & Hane, 1995). In addition, I was opposed to American intervention in the war through ‘military advisors’ and massive financial aid. I was shocked at the reception Salvadoran refugees faced in the US and what I thought was the hypocrisy of the US government toward Salvadorans.

I was concerned about power relations from the start. There was gender, rank, and time-of-arrival-on-the-project that were there front and center from the beginning. I wanted to establish at that first meeting that everyone would be an author on everything. In hindsight, I am very glad I did that.

I was keen to collect my own data set. I had worked, and continue to work, a lot with census data and know its limitations. A goal of this project was a data set that had detailed quantitative data on migration histories that we could match with socio-demographic and economic information and that we could marry with qualitative data that would tell complementary stories about the ‘refugee’ experience (cf. Silvey & Lawson, 1999). We underestimated how difficult data gathering was to be. These difficulties exacerbated the problems of distance between Dartmouth and New York and how difficult doing *any* fieldwork in El Salvador would be.

There were different levels of trust among members of the team, which intersected with methodological tensions. And changed gradually over time. Data was not ‘flowing in’. So what happened was this series of compromises between our proposal and the on-the-ground reality of collecting data. This is one layer of the story. Another layer was that we were all getting to know each other and develop relationships as the project unfolded. This too affected how we proceeded—interpersonally and methodologically.

Richard

FIG. 1.

The Politics of Data Collection and Methodological Inertia

Where We Began

Having two or more researchers on a team both enhances and complicates the process of knowledge creation. Differences among us were embedded in relationships formed across cloudy fields of power influenced but not exhausted by sex, age, professional rank, language ability, institutional affiliation, job status, access to resources, time and manner of entry into the project, and research and life experiences.

One difference occurred because project membership took a particular order. Adrian and Richard first submitted a co-authored proposal to the National Science Foundation (NSF). After they submitted a revised proposal, the NSF Geography and Regional Science Program Director recommended funding a pared down version of the plan only with the addition of a third co-principal investigator, fluent in Spanish and based near the field site. Adrian and Richard quickly invited Ines, whose previous scholarship entailed work with Hmong refugees (Miyares, 1998), to become a co-principal investigator and to contribute to rewriting the proposal. While Richard and Adrian planned to hire Alison as the principal fieldworker from the start, she was not involved in the planning until formally hired, once funding was in place. Ines then invited Alison to apply to the Master’s programme at Hunter College. When we met as a team for the first time, we agreed that each person was an equal member, and we held to equitable ideals as much as possible for the duration. This meant, for example, that every person would be involved in decision-making. We devised *modus operandi* in an attempt to democratise our research team, determining, for example, that we would have co-authorship on all publications (see Fig. 1).

The geography of the team itself was unavoidably gendered. The two male researchers were located on the same campus 300 miles north of the field site where the two female researchers were based. The former took primary responsibility for business management, while the first author conducted most of the fieldwork. The women on the team spoke Spanish, had conducted prior inductive qualitative fieldwork, and were in close proximity to the field site. We underestimated the impact of this distance. We found that face-to-face meetings as a team were essential in resolving communication problems that occurred regularly.

Despite our desire to empower and assist the Salvadoran community, we were inhibited in doing so. This community came to the US with the hope of rebuilding lives after escaping a civil war and yet functions in a state of legal limbo with few rights, limited political voice, and a fear of deportation. No matter how much we studied their situation, we could never appreciate the impact of distance created by our security in our status as citizens or legal residents and the freedom we had to travel to El Salvador to visit *their* families.

Finally, while the project always intended to use 'mixed methods,' only I had extensive experience in mixed methods with refugees and only Alison had conducted transnational qualitative fieldwork. None of us anticipated how resistant the population would be to respond. Alison invested hundreds of hours volunteering, working with community groups and telephoning contacts, and still was only able to convince 56 households to participate. One major issue in one of our face-to-face team meetings was lowering expectations of sample size and thus of statistical representation. This was counterbalanced by the richness of the information collected. As research progressed, we found ourselves shifting away from deductive quantitative survey-based data collected in formal interviews and toward inductive qualitative data that Alison captured. The most powerful 'data' were the stories themselves and the way they were told to us when we listened to respondents' own voices, not the socioeconomic measures we initially thought so important. Due to sample size, we could only make limited inferences statistically, but the stories transcend data limitations and probably are representative of Salvadoran experiences, regardless of sample size.

Ines

FIG. 2.

Although we established democratic ground rules, other factors inhibited a process of equitable team participation in decision-making and task responsibility. The gendering of our team involved two men who began the project and were both tenured at its inception [4]. They were distanced from the day-to-day operation, whereas the two women, located significantly closer to the field, were responsible for almost all of the fieldwork. Despite our intentions to challenge hierarchically structured team research, the distances between us sometimes reinforced these structures. Complex resistance to methods in the field highlighted disparities and distances among team members as we worked to communicate, analyse field progress, and come to methodological decisions.

Ines and Alison were more able to access the field because of their Spanish language fluency and geographical proximity. Despite the ease with which we all had use of telephones, fax machines and e-mail accounts, we underestimated how the distance between Hanover, New Hampshire and New York/New Jersey would compromise communication between team members [5]. All felt frustrated by the absence of co-researchers that served to limit perspectives on the field research. Although we met as a group periodically in New York, New Jersey and New Hampshire, methodological decisions were frequently mediated by impulses operating with hundreds of miles between them. There is an interesting irony here. Gender, time of entry into the project, rank and distance from the field can all be measured quantitatively in months, miles and other units. Grossman *et al.* (1999) note that it is easier to identify and discuss power differentials imposed by the world beyond the research group than it is to discuss those generated internally by the group (p. 129). Personality, experience, and relationships cannot easily be captured by quantitative classification. Such complexities prompt us to ask what form geographies of equality on research teams might take (see Fig. 2).

Contributing to a Political Struggle

Believing we could participate in social change, we began by conducting informational interviews with community leaders of organisations. We sought organisations with which to collaborate. Salvadorans in northern New Jersey were undertaking grass-roots organising at a frantic pace, consumed by the struggle to achieve amnesty or permanent residency for a population exceeding 300,000 with only temporary permission to live and work in the USA. Their experiences were underrepresented in academic and popular media (cf. Espenshade, 1997). We were interested in working with these organisations, and they were interested in our participation. Whereas we had planned initially to explore the transnational activity of the Salvadoran diaspora, community leaders instead explained a truncation of transnational involvement, entwined with their struggles to change temporary legal status to permanent residency. Initial informational interviews influenced our research questions and methodological trajectory. We would work with organisations to create survey questions, to meet people in the community, to learn about issues, and to contribute volunteer labour. We decided to begin with the survey and, secondly, to conduct open-ended, biographical interviews (Halfacree & Boyle, 1993). Nearly a year after we began research in New Jersey, we travelled to El Salvador where we conducted additional open-ended interviews. The final stage of research involved an intercept survey conducted on buses en route to a community picnic in August of 1998.

This context played a key role in knowledge production. Our political involvement grew out of our collaboration with community organisations, framed our research questions, and eventually *became* the framework within which we conducted research. Meetings with community leaders and pilot interviews with several respondents led to significant modifications of survey content, order and language. We sought to gather data that would be useful to the organisations and worked not to compromise their needs with practical survey implementation issues. With many competing interests, the survey evolved into a long and detailed document. As we became more immersed in the community's struggle for residency, we began to introduce the project as an attempt to support their efforts.

The manner in which we would participate, however, changed over time. While we agreed as a team to contribute collectively to the political movement, within our group we differed in our interpretations of such commitments. These discussions frequently revolved around whether to invest in quantitative or qualitative data collection, each powerful, political acts related to histories of relationships to data (Lawson, 1995; Mattingly & Falconer-Al-Hindi, 1995; McLafferty, 1995; Moss, 1995). While Adrian and Richard proposed a mixed methodology from the start, limited resources pushed us to decide where to invest more time. Feminist methodological discussions informed our approach in conflicting ways, which Mattingly and Falconer-Al-Hindi call 'the contradictory politics of counting' (1995, p. 443).

One form of political involvement, tied to participatory forms of data collection, entailed community work in the day-to-day activities of grass-roots community organisations. Community partners directed this involvement according to their needs and priorities. Alison taught English as a second language, participated in political organising and protests, and provided assistance with immigrant services. This provided a means to 'give back' through ongoing participation, to build trust, and to continue to design the research to collect data valued by people from both academic and activist communities. Participants who knew Alison through community involvement were more willing to participate in the research than were those recommended by a third party such as a

friend or relative. Respondents with whom Alison built trust over time were also more willing to reflect critically on the methodological process.

Whereas this commitment to the day-to-day work was central to Alison's participation, the collection of quantitative data remained important, particularly to Adrian. Quantitative data could be utilised by the organisations with which we were involved to attest to realities of asylum applicants in numerically powerful ways. Community leaders and government officials from El Salvador asked us to formulate numerical accounts of the Salvadoran presence in New Jersey in support of their efforts to secure more resources. We could employ quantitative data, for example, to support asylum cases and grants submitted to fund organisations, given that policy-makers respond more readily to quantitative results (cf. McLafferty, 1995).

Academic research teams gravitate toward the collection of quantitative data for other reasons. National funding bodies tend to support projects that exhibit 'scientific merit' [6], often a euphemism for quantitative rather than qualitative data. Accordingly, funding possibilities shaped preliminary drafts and subsequent rewrites of the subject and nature of our inquiry. Journals, too, a venue where we hoped to perform our acts, respond distinctly according to their own audiences [7].

This latter set of influences, coupled with professional training in statistical analysis and the desire to secure funding, encouraged Adrian and Richard to strategically foreground the acquisition of numerical data via survey-based interviews in the initial proposal. Although we wrote the survey in conjunction with community leaders to tailor it to the circumstances of the local Salvadoran community, surveys are not flexible methodological tools. Even after Alison piloted and altered the survey several times, we now agree, to different extents, that this tool was not fully appropriate for the particular research environment. It became gradually apparent that conducting survey-based interviews was difficult given the experiences of the Salvadoran refugee community. As in the proposal preparation, however, we continued to foreground the survey, statistical representation, and quantitative modes of analysis. To different degrees, Richard and Adrian thought that these methods provided the best possibilities for advocacy and publishing. The privileging of quantitative data collection and the marginalisation of qualitative data corresponded with the gendered geography of our research team.

As four scholars with unique personalities and skill sets, we trusted and valued knowledge forms distinctly. Ines and Alison had experience with qualitative methods and were more easily inclined than Richard and Adrian to move from quantitative to qualitative data collection. In the field, Alison responded to participants' desires to tell their own histories in their own terms. Many had not anticipated such detailed questions about their residential and work histories and expressed disappointment that they had been unable to share other aspects of their lives outside the parameters of our questions. Within the context of the political movement to acquire permanent residency in the USA, respondents wanted to record histories of displacement that related closely to US foreign and domestic policies.

We placed initial limits on ontological construction and renegotiated those boundaries over time. This approach differed from our previous experiences of team research and co-authorship and prompted us to question whether a research project can respond flexibly to competing interests. Our research questions changed easily, and we changed as individuals each on a journey of methodologically becoming something that we had not 'been' when we began. Frequently, however, we found ourselves without the tools to discuss these changes. To address the mechanisms needed for research teams to become methodologically flexible, we will now elaborate upon the enactment of research methods

on the ground and the challenges we faced in an attempt to expose the power ‘oozing’ (Katz, 1992) not only among us in our academic milieu, but also within the field. We track methodological moments through the research process. These vignettes, imbued with both the resistance and trepidation of research subjects and researchers, question clean methodological narrations by exposing decidedly messy negotiations.

Writing in the Margins

Although many aspects of the ethnographic encounter revolve around these everyday issues of the social construction of identity, it is also a peculiar relationship—unequally initiated, situationally lopsided, spatially dislocated, temporally isolated, extrinsic in purpose—it oozes with power. (Cindi Katz, 1992, p. 496)

All placed me in situations of inauthenticity, dissimilitude, and potential, perhaps inevitable, betrayal, situations that I came to understand are inherent in fieldwork research ... The inequality and potential treacherousness of this relationship is inescapable. (Judith Stacey, 1996, p. 91)

The Salvadoran civil war waged violence on Salvadorans from 1979 to 1992. During that time, 75,000 died, and one million of the approximate population of five million fled El Salvador. The expression of opinions or allegiances or the mere accusation of either often resulted in death. Both guerrillas and soldiers exercised tactics of coercion, repression and terror to advance their cause. Participants in this study reported fears of men in uniform, of aeroplanes flying overhead, as well as difficulty sleeping, depression, alcoholism and drug addiction, which they connected to their flight from the past. Many suffered from post-traumatic stress syndrome (PTSS) (Aaron *et al.*, 1991; Agger, 1992). Even in the context of careful piloting and dialogue with respondents, and despite the development of friendship and trust, the interview remains a neo-colonial process wherein the interviewer intrudes, attempts to take possession of something of value (information), and thus provokes anxiety in the interviewed (cf. England, 1994; Stacey, 1996) [8]. Although interviews facilitate negotiation between interviewer and respondent, we initiated these dialogues and thus caused respondents to discuss fear, anger, sadness and anxiety that had been dwelling inside. Alison left most respondents depressed, and left depressed herself. As the interviewer, she internalised this process and came to dread intrusive telephone calls and interviews perhaps as much as some respondents.

In the USA, Salvadorans face a slim chance of gaining political asylum in court. In response to a lawsuit filed to challenge unfair asylum practices towards Salvadorans throughout the 1980s, the US Immigration and Naturalisation Services (INS) began in 1990 to place Salvadorans in programmes that assured Temporary Protected Status (TPS) [9]. Approximately 52% of participants in our study had applied for political asylum and held temporary legal status in the USA. For most, this entailed several years of waiting, in many cases over a decade. Conditions of these temporary programmes place restrictions on the daily lives of Salvadorans. They have temporary permission to work, but cannot become permanent residents unless and until they have a successful hearing (are accepted for asylum). They may not leave the USA until their cases have been resolved, at which point they may be issued a deportation order. For the ‘temporary’ Salvadoran population, this has meant stress, anxiety, uncertainty, separation from families, lives of limbo, and the prolonging of the effects of PTSS. Interviews with INS and court appeals caused extreme anxiety, and many participants in the project

I want to assure you definitively that your answers are confidential. I will not at any time write your name or any other information that would make it possible to identify you. If I ask something that you prefer not to answer, there is no problem. We will go on to another question ...

B3. 'When did you leave that residence? B4. Where did you move? B5. Why did you move?'

January 1998, Orange, New Jersey. One crisp winter Sunday morning as I interview Juan, his brother Wilfredo wanders in and out of the kitchen of their basement apartment, dropping sporadic remarks along the way. As I write Juan's residential history, he reports that he entered the US during one year, only to be corrected by his brother. Wilfredo reminds Juan that he lived in Mexico for a few years prior to coming to the US. Juan, flustered, announces that he will have to consult his records, and disappears into the back room. After ten minutes pass, I excuse myself to use the bathroom and find Juan deliberating over a spread of papers. I suspect that Juan, like other respondents, would like his responses to the survey questions to match the information in his application for political asylum. He does not tell me as much. I suggest that we stop for the day and meet again the following week, and he suggests in turn that he will provide an answer then.

November 1997, Union City, New Jersey. I sit in a room that grows smaller as I 'conduct' a survey-based interview with Luis, who grows angrier and louder. Having lost his patience with the tedious survey questions and with the hypocrisy between US foreign policy that displaced Salvadorans and domestic asylum policy that denies them entrance, he embarks on a monologue. I cease my questions and simply write. As he walks out of the room shaking with anger, he tells me that people at the community meeting where I had presented the study wondered what I was really doing, whether it would perhaps be a book. He states that he does not care and requests that I tell his story in a book for all to hear.

FIG. 3.

associated our survey-based interview with the asylum application procedure. They wanted their responses to correspond.

We will never know the extent to which histories of violence influenced relationships in the field. The prevalence of caller identification among participants was one visible expression of PTSS. Caller ID, now available in most parts of North America, has forever changed the nature of telephone interaction. A small box installed on telephone lines enables the receiver to identify callers by telephone number and either name or location. Telephone call recipients make more informed decisions to answer or not to answer by knowing the identity of the caller. Most interview respondents subscribed to caller ID to screen callers.

The methodological moments, culled from Alison's field notes, in Fig. 3, occurred during survey-based interviews and imply a fear of sharing biographical information [10].

Recording histories terrorised some respondents, all of whom survived a war that many wished not to recall. Several times, participants agreed to interviews on the condition that we would not discuss a past in El Salvador too painful to recount; one that included war, the loss of family and friends, and the need to leave children behind in the care of others. Respondents and interviewers thus drew on different sources of power within the interviews. The collection of personal information was reminiscent of the surveillance typical of the military response to guerrilla warfare. Despite assurances of confidentiality, the possibility of recording conflicting histories of mobility alarmed respondents. Occasionally, respondents reported having intentionally provided incorrect information in the interview. They insisted, nonetheless, that the information be recorded incorrectly in case the study was to one day 'backfire'. Fearing that the data could then

be exposed, the respondents subjected to deportation, they wanted survey answers to correspond with personal information recorded in asylum applications.

The decision to provide information was a personal, political act that outweighed the trauma of testimony for those who chose to participate. An important lesson we learned as the framework of our research project evolved to support the struggle for residency was that the meanings of the survey were negotiated in multiple and sometimes conflicting ways. Participants chose to respond for different reasons. Some wealthier immigrants wanted financial success documented; others who had become US citizens aimed to document their legal success. Still others sought to represent the experience of being trapped in the limbo of temporary status programmes.

Many participants were pleased that attention was being paid to the plight of Salvadorans, marginalised in so many ways in the USA. While we had roughly the same political agenda, our relationships with respondents and amongst each other continued to ‘ooze’ with power. The incompatibility of the survey method always remained in tension with the desire to ‘tell the story’ and ‘bear witness’. Indeed, the survey method was incompatible with the terms of participation of respondents like Juan and Luis, who exercised control, interrupted and usurped the interview structure when the compulsion to relay histories in their own terms overcame the more rigid structure of the survey within the moment. Still others chose to set those parameters prior to the interview by qualifying participation.

Our team dynamics affected the process of data analysis, and we literally marginalised these methodological moments, recorded frequently as ‘stories in the margins’ of the survey. Qualitative data are relatively more difficult to share among research colleagues, and particularly among those accustomed to working with quantitative data. Regardless of the messiness of collection, quantitative data travel more readily through layers of interpretation and masquerade as clean, unbiased sound bites of knowledge (see Woolgar, 1988; Hanson & Pratt, 1995, p. 89). These bites were more mobile and adaptable knowledge forms for the research team and others, including consulates and community groups. Thus, it was easier to share, discuss and evaluate statistics than to dwell on the factors inhibiting high rates of survey responses. Indeed, qualitative data feel ‘out of control’ in the context of a quantitative framework. Our own positioning in relation to respondents and circumstances biased insight (see Hanson & Pratt, 1995, p. 89). Months later, additional research assistants whom we hired to enter data on spreadsheets added their contexts to interpretation. Eventually, we waded our way through layers of interpretation and attempted to present the research and our various standpoints in accountable ways.

Within the context of a political struggle, the power of knowledge lies in the rate at which it can be mobilised to audiences such as policy-makers, media, politicians and funding institutions. Knowledge can be measured by its ability to rally people and marshal resources. In this context, statistics often appear as ‘objective’ information. What happens to data not recorded in the survey, but recorded in field notes, visible in one sighting but not the other? Those comments made outside of the parameters of our tools, while marginalised in the sharing of information, are central to interpretation. They function simultaneously as the lens through which we see our own data and the screen through which we filter data for other audiences.

These stories in the margins of the survey—the ambiguities, miscommunications and qualifications—provided key context to responses. While we wrote the survey collaboratively with respondents, many larger field studies entail the implementation of research surveys in immigrant communities where researchers with no relationship to the

April 1998, Newark, New Jersey. Ana asks me to explain each question before I turn on the tape recorder, at which time she chooses which ones she will answer.

April 1998, Newark, New Jersey. As I turn on the tape recorder at the beginning of a long interview, the participant, a friend, jokes about the possibility of deportation as a result of the interview. He remarks that if INS picks him up, he will know why, and he laughs.

FIG. 4.

neighbourhood knock on doors to find participants (e.g., DaVanzo *et al.*, 1994). Such efforts have significant points of weakness. Haraway insists that 'situatedness is not given, its technologies must be revised and invented' (Rose, 1997, p. 308, citing Haraway 1991). The challenge for feminist research teams doing participatory research is to work alongside community partners to devise methods and questions relevant, meaningful and appropriate to the collection of information in varied communities. Researchers must render themselves less powerful and more responsive to resistance in the field. Our experiences of conducting survey-based interviews reveal that the conditions and meanings of participation provide essential insight into the experiences being recorded. Resistance to certain types of knowledge production produced alternative bodies of knowledge that would not be recorded on a 21-page survey, but rather, in its margins, on the blank spaces of the page, and in the awkward phases of conversation. Over time, we shifted methodologically and began to move those stories from margin to centre.

Methodological Movement: flexibility or compromise?

From the start, we had planned a mixed methodology that would include open-ended interviews and focus groups. Mixed methods would incorporate a range of interests and outcomes. Struggles over knowledge production among team members and respondents continued with other methods, such as tape-recorded, unstructured interviews (see Fig. 4). Even the beginnings and ends of interviews are academic productions, often inconsistent with the nature of conversations, storytelling, and other productions in which social scientists play a complex role. And of course, some of the most telling comments were made after we turned the tape recorder off at the end of interviews.

Meanwhile, as we shifted from quantitative to qualitative methods of data collection, some felt as though they had moved forward, while others felt they had lost control. 'Flexibility' felt like 'compromise' to Adrian, who found most utility in quantitative data, and 'progress' to Alison and Ines, who valued qualitative data the most (see Fig. 5).

Feminist methods look to silences for meaning (Anderson & Jack, 1991), and our collective, retrospective discussions of the history of and tensions in our project have been illuminating. We grew to understand that the fear and mistrust that we negotiated with potential project participants was not a barrier to overcome, but rather, an instructive part of the research process. We also explored retrospectively the dialectic of the team and the field while negotiating the differences among us and our aspirations for the project. The challenge to feminist research teams is to incorporate conflict, to come up with flexible methodological strategies that understand these experiences as 'data' (cf. Pratt, 2000).

Of course, feminist social scientists have argued that, like the distancing of quantification (Brown, 1995), fieldwork is also a masculinist practice of distancing (Rose,

As the team member most present in the field, the onus was on me to provide insight into methodological experiences that I did not fully understand myself. I learned to anticipate the widening of eyes at the beginning of every interview when I pulled the 21-page survey out of its plastic envelope. Alone afterwards, I turned the survey over and found relief by writing notes on the blank white pages.

I struggled to articulate to my colleagues the difficulties that I faced, from the logistical to the emotional issues surrounding interviews. I was almost, but not quite, an equal member of the team. Challenged to represent resistance in the field, I received telephone calls from co-researchers perpetually asking for 'n'. I waffled, embarrassed to relay the 'n' that I knew all too well. N kept me up at night. N was my nemesis. But how to explain that 'n' was engaged with the method, the context, the history, and not only with research abilities?

After previous ethnographic research with primarily undocumented Mexican migrants, I wanted to contribute to political struggles through everyday field practices. Previous research had also made me sceptical of quantitative data collection and moved by the power of oral histories. Respondents echoed my own scepticism, disappointed by the tedious details asked of them, rather than the opportunity to relay histories as they chose. Eventually, I left the project with a healthier respect for the power of quantitative data, but with the same scepticism regarding its collection. I also left with a heightened awareness of the support that research assistants need in the form of communication, infrastructure, management, the defining of roles and realistic expectations, and the presence, time, perspectives, and encouragement of principal investigators.

I worked to balance the interests of respondents and principal investigators and felt continuously repositioned 'in between' (Moss, 1995, p. 445), the only person required to maintain relationships with all parties. I learned from my team members and value them highly as colleagues and friends. While I was frustrated many times, I do not blame them. I see our interactions as a product of the academic industry; one that encourages interdisciplinary collaborations, and one that values product over process. Our experiences continue to shape me as a researcher and as a mentor to others, and they raise important challenges for all social scientists working collaboratively.

Alison

FIG. 5.

1993; Behar, 1996; Katz, 1996). But participatory research provided an important context for the data collected. It enhanced our understanding of political organising, the dissemination of information about shifting asylum policies and practices, and issues important to the community more generally. Data collection connected with uneven asylum processes, geopolitical relationships, political movements, and personal histories of violence. In retrospect, a post-colonial methodological approach would respond more directly to the geopolitical frame of US imperialism and military involvement in El Salvador. It would also take shape according to respondents' modes of telling.

Feminist methods aim to disrupt hierarchical relationships through collaboration and generally assume that epistemological quandaries never occur in isolation from the implementation of methods on the ground. This opens new possibilities for understanding knowledge construction in the field and forms an important part of one objective of feminist research: to destabilise power relations in practice.

Reconceptualising the Research Process in the Context of Political Struggles

A field project is a story perpetually in the process of becoming in which we are all vulnerable (England, 1994; Behar, 1996; Coffey, 1999). We have managed to excavate some things that succeeded and have embarked on a collective working through of those that did not. In this section of the article, we join others (Bradley, 1982; Grossman *et al.*, 1999) in suggesting operational strategies for feminist research teams. Our hope is that

this will leaven discussion among other collaborators. We immerse our own struggles in broader politics to help produce more organic, fluid models of research.

Feminist theories address inequalities with the deconstruction and subversion of binaries constructed between nation state and its others, between academia and its others. Striving for democracy within and beyond the research team therefore entails dissolution of the boundaries between researcher and researched, between professor and student. Such democratic ideals require attention to the mechanisms of team management, with close attention to logistics and communication. Careful management is essential to equitable and effective participation in team research. Feminist team management means working productively with differences among team members, accentuating positive contributions, creating adequate modes of communication and decision-making, and enabling equal platforms for voices.

To the extent possible, all team members should have the opportunity to partake in any part of the process, including planning, decision-making, fieldwork, data analysis and writing. Team members might participate in a retreat at the outset of the project to establish *modus operandi* and to get to know one another. They should make preliminary trips to field sites, rehearse the survey together, and hold routine face-to-face meetings and telephone communications.

Feminist methods understand difference, complexity, uncertainty, resistance and conflict as productive aspects of field research and as resources for social change [11]. Here, we refer to differences among all participants, including members of the research team and those with whom they collaborate. Our distinctions, some more material than others, such as possession of a US passport and hence the ability to travel to El Salvador; and other intersections, such as gender, 'race,' ethnicity and class, provide fertile areas for discussion and the exchange of ideas.

Our identities as researchers were constituted reflexively (Rose, 1997) in relation to the shifting identities of participants. We positioned ourselves and were positioned strategically, which in turn determined our political participation. We were variously constructed as an American audience, members of an exclusive nation state, advocates in the struggle for permanent residency, and academic researcher partners. As these identities shifted, the collective interpretation of our involvement in disparate geographical contexts also altered power relations. Within these strategic positionings, we discovered new political alignments and came to better understand distinct forms of participation in social change.

The organic feminist research project might attempt to accommodate within it a range of agendas and products. This might mean a thick but creative research process in which all parties involved contribute to the construction of knowledge. This commitment to participation could include learning other languages, to various forms of support in community projects, to greater political voice and empowerment in local, state, national and transnational forums. Mixed methods enable richly varied research products that might connect to a broader set of audiences. This goal can incorporate different political objectives and the creation of various forms of knowledge.

Post-colonial team fieldwork entails a flexible methodology, constructed collaboratively with participants and changing over time according to the narration of histories and other political forms of expression. For us, locally, this meant the deconstruction of frameworks imposed upon neighbourhoods from neo-colonial, bird's-eye views of census tracts. Regionally, it meant participating in networks of grass-roots movements gaining momentum across time and space. And transnationally, it meant framing disparities in contexts of globalisation, colonial and cold war geopolitical relationships.

There remains, however, a paradox, identified by Grossman *et al.* (1999) in the relationship between the politics of research teams and the broader political movements to which they contribute: 'Perhaps the fact that the feminist egalitarian ideal existed in the context of substantial power differences made it very hard to address the issue of power within the group' (p. 127). While hoping to disrupt power relations through participatory research, we were challenged to address them within the research team.

There is movement in the academy toward ever-larger interdisciplinary, cross-institutional, international research projects. The projects usually consist of principal investigators who employ research assistants to conduct fieldwork. Research assistants usually work more closely with data than do principal investigators whose primary role it is to oversee the project and 'write up the results'. While these relationships form the core of the successful implementation and presentation of research, discussion of the institutionalised hierarchies upon which they rely remains counter-intuitive to polished academic performances. Such large research machineries are less likely to accommodate feminist ideals. As with methodological moments excavated from field experiences, research teams must also explore the distances taken for granted in large projects. For us, such distances lay at the core of our struggles over quantitative and qualitative data collection and over how to contribute politically.

On Becoming

Every journey conceals another journey within its lines: the path not taken and the forgotten angle. (Jeanette Winterson, 1996, p. 9)

The story never stops beginning or ending. It appears headless and bottomless for it is built on differences. Its (in)finitudes subvert every notion of completeness and its frame remains a non-totalizable one. The differences it brings about are differences not only in structure, in the play of structures and of surfaces, but also in timbre and in silence. (Trinh T. Minh-ha, 1989, p. 2)

We find ourselves still, as we write, in the same quandary, struggling to articulate the more subtle aspects of methodological becoming. This article has been difficult to write and years in the making. In the process, we learned not only of the challenges and rewards to collaborative writing (cf. Women in Geography Study Group, 1997; Laurie *et al.*, 1999), but why so few have written about team research. We did not know how to compose a history that belonged to so many people. Alison wrote the first draft in fragments with the hope that the blank spaces where there might have been a narrative would provoke the reader to sew the pieces together. Each time the ground beneath us shifted, the words changed. We still find ourselves in a process of becoming. As we write, we disrupt, dispute and rewrite each other's history of what took place. This project has multiple tellings, and each rewrite has concealed another storyline, suppressed and compressed into Latour's 'black box' (1987). The academic aesthetic still reigns.

While acknowledging the impossibility of comprehensive discussions of power and politics in the field, we do hope that this cross-section of experiences has exposed some of the complexities that scholars often write out of papers. The operation of the research team is an important part of the story, not to be dismissed as navel-gazing. As geographers laud mixed methodology, we also must mind methods for the successful enactment of a diverse research team. Each member must be empowered, knowing that their contributions are valued and utilised. Though equal participation may not be possible, forms of collaboration that destabilise power relations are. Team organisation

A friend has a reputation for posing his thesis students the familiar refrain: 'If you could do that project again, what would you do differently?' Like many of these students, I cannot now claim to be surprised by Alison's request to 'please reflect on the research methods we conducted as a team ...' Indeed, this is the second such request for an individualised and independent reflection on a group dynamic that, in some permutations, spans over 15 years. No, I cannot claim to be blindsided by this request, yet, as the pen continues to hover lifelessly over the page, it is increasingly apparent I am going to be unable to scribe any succinct, pithy, or meaningful phrases that begin to get at what 'methodologically becoming' actually means.

Aware of my inability to articulate any big themes, I am reminded of one moment near the end of the project. We had been invited, as a research team, to present and discuss our findings to community leaders, activists, organisers, and interested Salvadorans. Riding the NJ Transit bus through the Lincoln Tunnel I remember fragments of conversation about possible outcomes for the meeting, our ongoing roles, a sense of closure to the project, and so on. During the meeting our contributions, silences, and dynamics seemed to reflect all of the strengths and weaknesses we had experienced as a research team: length of time on project; sexism; racism; hierarchy; personal politics; academic training; language ability; and familiarity with the data collected to date, to name some. And to be sure, if we had hoped for closure, it did not happen.

But does this matter? Our own reflections about how we were involved in knowledge mediation in the field have, if anything, intensified since the project's formal ending date! Perhaps this kind of methodological becoming is an inevitable and necessary part of a project that involves a team with different strengths and weaknesses becoming immersed in a difficult, dangerous, and intoxicating field. Could I (we?) have been better prepared for this? I wonder if I am the only team member to have been anxious about the fluidity and slipperiness of the research directions pursued at different junctures. To give my friend an answer, for me, this project signalled the importance, now more than ever, of committing to an ongoing and sustained study of methodological awareness, across the divide.

Adrian

FIG. 6.

requires collaborative tending to communication, expectations, divisions of labour, networks of support, distribution of finances, geographical location, language, personality, position, and so on, none of which can be taken for granted.

Perhaps some things are better left unsaid. But to change how we do our work, other things must be said in order to fracture the silences of Latin Americanist geographies regarding the politics of fieldwork (Sundberg, 2003). In this project, we collected rich data, but they were not always what we expected. We each compromised and grew in different ways, and therefore so did the project itself, and we carry these important lessons with us (cf. Hondagneu-Sotelo, 1993). Academics have the potential to participate in social change, but we must find ways to collaborate effectively with each other and with those beyond the academic world.

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NOTES

- [1] There are exceptions, including, for example, dialogue between Susan Hanson, Gerry Pratt and research assistants (Hanson & Pratt, 1995).
- [2] Exceptions include Bradley (1982), Hanson & Pratt (1995) and Grossman *et al.* (1999).
- [3] Recognising the need to participate in the deconstruction of hierarchies within and beyond the academy (cf. Castree, 1999), we attempted to dissolve the boundaries between ‘field’ and ‘university’ (see Staeheli & Lawson, 1994; Sparke, 1996). Yet implicit in our argument is the reification of these boundaries by the cultural, material and geographical disparities between academic and grass-roots operations.
- [4] Ines was promoted to Associate Professor with tenure one year into the project.
- [5] Adrian taught at Dartmouth for the duration of our project and moved to the University of Leeds after the completion of fieldwork.
- [6] The weight of this influence varies by national context, and we thank an anonymous reviewer for the reminder.
- [7] While some newer journals publish more qualitative work on migration and transnationalism, such as *Diaspora*, *Identities* and *Global Networks*, the journals considered most ‘prestigious’ by the broadest cross-section of people studying immigration, such as the *International Migration Review*, still privilege quantitative research over qualitative or mixed methodologies.
- [8] There are, however, interesting examples of rethinking the interview process around geographical fields of power. Agger (1992), for example, redesigned and devoted a room in her home, ‘the blue room’, to the ritual sharing of refugees’ stories.
- [9] These include Temporary Protected Status (TPS), Deferred Enforced Departure (DED), the American Baptist Church settlement program (ABC), and the Nicaraguan and Central American Relief Act (NACARA). See Mahler (1995), Coutin (2000), and Menjivar (2000) for histories of the legal experiences of Salvadoran asylum applicants in the USA.
- [10] Names are pseudonyms and details have been altered to protect participants.
- [11] Thanks to Gerry Pratt for helping us to recognise this key objective of this article.

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