

Locations

Homework and Fieldwork

Research on the everyday experience of social sustainability encounters the ordinary pitfalls associated with workings of power, positionality, complicity, and friction between researcher, subjects, and contexts. Since experiencing social sustainability entails direct and indirect submission to various modes of being and becoming, intimate interactions between the researcher and subject are far from being immediate. The researcher's relationship with her respondents and her presence itself is a palimpsest of power and desire steeped in long histories of exploitations, hierarchies, and differences. Caught in the mire of very complex histories of marginality, Indian Nepalis may perceive someone like me as one who holds the position of the mainstream oppressor caste/class. Being a Bengali middle-class and upper-caste *bhadramohila* (respected lady) residing in the United States, I had to engage with the awkwardness of my presence in some of their lives over a decade with my caste and/or class privileges intact. The "data" that fills this book then rests on a layered process of negotiation and positioning with forces in the field and also forces "at home" (in academe).

I also find it paramount to emphasize the importance of grounded feminist ethnographic research for a nuanced engagement with sustainable development discourse and practice. Such an emphasis will enable scholars and practitioners to consider not just the complexity of where the fieldwork is being done, but acknowledge that our academic choices (theoretical and methodological) are also deeply political and may have consequences for our particular ontologies of engaging the sustainable. One needs to be reflexive about such methodological and theoretical selections and contemplate more deeply the perception of such research methods in the fields in which one's book will be read.

Such “locations” often remain under the radar in these days of producing activism and policy friendly research. I believe that the arguments that I have made in this book are possible because of my academic location and the way I approached my field, as a feminist interlocutor deeply mindful of complicities and contours of power.

I begin this chapter detailing factors that shaped my fieldwork experiences; how they affected the minutiae of interactions I had with the participants in the field. This is followed by discussion of this book’s methodology which I call a feminist longitudinal ethnography.

Fieldwork: Pressures to be a “Conventional Anthropologist”

Very early on in my fieldwork, I felt the pressure of being a conventional anthropologist. For instance, even before I had landed in Sonākhēti plantation, the owner told me that he had a plan for me. I had already sent him a formal letter explaining my research and its goals so that my intentions would be clear. But he insisted that I study the “cultures and customs among the Gurkhas.”¹ He placed another challenge before me stating: “I am not going to give any of this information to you up front. You anthropologists are trained to investigate these cultural things, so please come back after you are done and tell me what I have not found out in my years here.” He soon realized that my objectives were different. He was not the only one. Other bureaucrats or officials in Darjeeling town whom I met had similar responses. At social gatherings, they would introduce me as someone who would write a “book on Darjeeling,” which was true. At other times the introductory sentence would be “she is here to study the tribal culture of the Gurkhas.” I did not object to any of these depictions of my work and my presence out of respect for my hosts’ generosity and time. This way I also got access to powerful people’s extremely pejorative opinions about plantation workers, tea production, regional politics, and Darjeeling’s future. I would frequently find myself in situations where extremely problematic statements would be made, assuming that my caste and class position would make me party to their nuances. Many of these interactions confirmed the existence of popular stereotypes about Nepali/Gurkha men and women, which influenced localized development and Fair Trade interactions as amply documented throughout *Everyday Sustainability*.

There was another reason why I went along with their depictions of my research objectives. Whenever I mentioned to bureaucrats or

plantation owners in detail about my comparative work on Fair Trade, they would quiz me on why I was interested in small tea farmers. Most plantation authorities and tea bureaucrats were in denial about the presence of small farmer-grown tea. Some old officers of the Darjeeling Planters' Association (now called Darjeeling Tea Association) worked with local NGOs that had become liaisons for transnational Fair Trade bodies. I soon realized that they had very little idea of Fair Trade's history and association with small farmers even when the state tea board had already begun making inroads into these non-plantation areas for tea farmer training (see chapter 3 for details).

Usually, I handed these people my business card followed by my much-practiced research sound bite. Immediately there would be comments on how local tea farmers were numerically insignificant and yet foreigners (and U.S.-based scholars) were the only ones interested in them. They would ask me the reasons behind this spurt of interest, which had soared between 2004 and 2011 when the bulk of my ethnography was conducted. I would have a tough time convincing them that I was on to something, and the small farmers were not only significant for Western buyers but also for plantations in Darjeeling.

Once the plantation owners realized that I was not studying "Gurkha customs" per se or "tribalism," they made an effort to monitor my interactions within the plantation. Mapping "Fairness" through plantation ethnography was a real test of patience, irony, and every process deeply fraught with classed, gendered, raced assumptions about my leanings as a Bengali, middle-class *bhadromohila* from Kolkata via the United States suddenly speaking good Nepali and eating *gundruk and dalle* (local vegetables)!

People had very little spare time and the workers who were articulate and informative were hesitant to share their views in spite of knowing that I did not intend to put them at risk. This was understandable considering that Bengalis also tend to perceive Nepalis in stereotypical ways, such as they are simple people, they drink a lot, Nepali women are outgoing and wayward, and they are fickle. Nepalis tend to see Bengalis as sharp, privileged, and not as well dressed as them, despite their inherited class privilege of having cushy jobs in the government. These are the dominating representational tropes shaping the friction-laden interactions between two groups. Being a Bengali and discussing politics with Nepalis was tricky. My fluency in Nepali combined with repeated visits without any "project" idea was perplexing for my interlocutors, but they knew I was someone they could eventually trust with their views. I always explained to them that the places and people would be protected with pseudonyms. They

knew that I had lived the first five years of my life as an infant in Darjeeling. Once I had stayed in the plantation and cooperative area long enough, the elderly women would refer to me as “*Darjeeling ko Chori*” (“Girl from Darjeeling”) when they introduced me to strangers and cracked jokes about town traders mistaking me for a stupid Bengali tourist until I opened my mouth to bargain in Darjeeling Nepali.

In the plantations, the strict discipline of field and factory life was made unbearable by the presence of “*chamchas*” (henchmen of the planter) who literally reported on every aspect of workers’ and “visitors’” work and leisure routines. Considering the owners’ presumptions about the liberal and left-leaning politics of academics, they consciously discouraged me from “wasting my time with the union goons.” I would be surprised when the plantation managers would meet me at some corner of their large (more than 400 or so acre estates) and know the details of my previous day’s movements, although we had not met in weeks. They would tease me about becoming too close to Nepalis as if I had transgressed borders of respectability for an upper-caste Bengali woman. In an indirect way a compliment about how well I spoke the local language was always a reminder of their suspicion about my interactions with workers in a setup of intense disciplining of the labor force.

Whenever I asked the planter about collective bargaining, I was directed to closely observe the activities of the Joint Body and was rarely encouraged to talk to key labor union members. The union was not a taboo topic between the plantation administration, and me, but they made a tremendous effort to convince me that the plantation had no “union problems” common in the 1980s and that NGOs were doing such amazing work (see also chapter 4). Some management folks even told me, “Please let me know what you learnt from members of the Joint Body about their feelings about Fair Trade.” One planter’s standard statement was, “The sustainable revolution has already been unleashed in Sonākheta through organic farming and Fair Trade. It was supposed to produce new energy, prosperity and improve plantation life, but we do not know what good things are in store, and I rely on you for all this emerging information.” Apart from politeness, this statement was a reminder that nothing unethical could happen in a Fair Trade–certified sustainable plantation. It was also a reminder of an unspoken expectation of not raising suspicion about this plantation in exchange for the owner’s generosity toward me. He still wanted me to follow closely the “tribal culture” of various Nepali groups present in his estate. He considered it natural that as an anthropologist, I would be interested in tribal customs, as we study culture. He even

joked about my lack of excitement about meeting local healers and shamans (*jhākri*). He told me “All the white researchers and visitors here get so excited when I invite them to meet *maila baje* (the local shaman) while you have no interest, you seem to be more interested in local gossip.”

Plantation workers in the beginning were extremely hesitant to talk to me, even when I had started living in their homes. I took advantage of homestay programs at the plantations and then gradually went beyond the homestays to interact with other workers and living with them. Naturally many plantation workers saw me as an acquaintance of the privileged plantation management since their bosses often spoke to me in English, Bengali, and Hindi. Some saw me as the owner’s *pāonā* (guest), but they soon realized that I was far more interested in plantation life under the radar, away from the performances workers put on for other visitors. Compared to how quickly I established rapport with the members of the tea cooperative, it took longer to break ice in the plantation because of the intense gendered surveillance. In Phulbāri the owners were non-resident Marwari, but the resident director was a Nepali gentleman. We frequently met in his factory office and our class status and conversations in English often raised suspicion among workers in Phulbāri as to how much they could trust me. I was an “other” in a community where Bengalis are seen as outsiders.

I detail the history of these fraught ethnic relations in the next chapter. Bengalis and Nepalis have deep animosities because the latter feel exploited by Bengalis or *madeshis* plains people. The Nepalis in Darjeeling consider themselves to be *pāhāDi* or hill people. For both Bengalis and Nepalis, the perceived differences result in thinking about each community through stereotypes. Such hostility also exists between Nepalis, Marwaris, Biharis (other non-Nepali communities in Darjeeling), and other dominant regional ethnic groups that exist in Darjeeling. The animosity and ridicule of Bengalis of course has a particular historical backdrop. Darjeeling is a district in the state of West Bengal, and the Gorkhaland movement was aimed at separation from Bengal to form a separate Nepali state within the Indian nation. Bengalis were thus envisioned as immediate oppressors reflecting the neglect of the Indian state toward its regional and ethnic minorities.

There was no denial of my class privilege. Although I was upper caste, being Hindu helped on many occasions considering so many of my informants were Rais and Chettris. I also got along very well with people of Buddhist leanings because I did not practice the strict dietary restrictions like a quintessential Hindu—I was not averse to eating beef or pork. It should be noted that I rarely had Tibetan (*bhote*)

research subjects since Tibetans are mostly residents of urban areas in the Darjeeling district and are not farmers; they are more active in trading and tourism. Sometimes my open dietary practices were a cause of concern for very orthodox Nepali Hindu families (especially the *Chetri-Bauns*). But soon they realized that I was not exactly invested in being a devout Hindu, and of course, I ate whatever they cooked at home as an exercise in respect for their time and taking care of me. I was, however, forbidden from touching the kitchen *chulā* (cooking fire) in some Hindu homes even though I was upper caste. The fact that I was married to a Hindu man went in my favor, but not without complications. The mark of a married Hindu woman was wearing vermillion, which I did not use. Initially this resulted in some serious doubt about whether I was actually married. I had some wedding pictures in my laptop which came in handy. But being married without children was another serious issue and a trigger for much ridicule. My respondents advised me to have children before I returned to Darjeeling after my dissertation fieldwork. Being a married woman made it easy for me to discuss delicate household matters, and women assumed that I would understand their personal problems. There were many things I learned about these women that they asked me not to write about, especially details of abuse or violence in family life, and I have kept those details out to honor these requests.

Informant, Interlocutor, Researcher, or Activist?

There were times during fieldwork when I found myself to be like an informant and translator, but I would not call myself an activist (even if some see me that way) for reasons that will become evident in this section. Lower-level bureaucrats would try to understand many things about the Fair Trade movement by quizzing me since none of the training material was available in Nepali between 2004 and 2006. The most difficult questions were from tea farmers and some plantation workers who wanted to know why all of a sudden white people were interested in helping them. Many tea farmers initially saw Fair Trade as an NGO aid or government aid program. Some joked, saying that there was an overflow of money they got from selling their tea. They would use the English word “flow” to interpret FLO (the certifier with which they had become familiar). This was a very useful way for me to engage people on how they viewed Fair Trade and if at all they thought they benefited from it.

My assumptions about what women in the plantations and non-plantation areas should do to scale up their political and entrepreneurial ventures were reigned in from time to time by women reminding me that I would not be there after a few months when they would have to carry on their entrepreneurial ventures and associated community-level strategic interventions. Once I had asked Chitra Rai (an active member of the Women's Wing of the local political party Gorkha Janamukti Morcha and resident of Phulbāri plantation) why they do not build alliances with women's wing members of other political parties or activist organizations in New Delhi. She replied with sarcasm:

You think everyone is like you, leaving your home and husband and spending time with us eating pork, joking in Nepali about our lives. Most other educated Indian women think that we are sexually promiscuous, that we are not interested in education or stupid and that we, Nepali women, are best suited to be servants in their home. We will never join with a mainstream women's organization, this is our fight for dignity of Nepali communities within India.

Sunita, another activist went further to explain why such alliances were not possible. The December 2012 protest against the Delhi rape was the most visible backdrop of her comments. She stated:

[L]ook at all these women out on the streets of Delhi, our country's capital, fighting for injustice for this young woman who was raped. But every time I think about these protests I cannot forget that Nepali women are killed and abused in Delhi because people think we are outsiders and our girls are prostitutes, sometimes the police refuse to note down FIRs. But we cannot expect anyone to cry out on our behalf, take the streets. Nepali women do not matter. So how can we expect these same NGOs to suddenly support our cause?

The past eleven years of engaging with women in Darjeeling has left me with no option but to engage the "everyday" with the hybridity of an "insider-outsider" (Chowdhury 2011). Women and men have freely critiqued my privilege, at the same time opening up about their intimate lives in ways that I never imagined possible as a fledgling graduate student when I landed in Darjeeling in 2004. The sustained engagement of hybridity has not only shaped what I did while collecting

data, but made me conscious about my positioning as a scholar and my representational strategies when I wrote about women in Darjeeling.

In recent years feminist scholars, especially feminist scholars of color (Davis 2013; Chatterjee (2009); and transnational feminists (Alexander and Mohanty 2013; Chowdhury 2011, Nagar and Swarr 2005) have raised rightful concerns over what passes as feminism, gender sensitive research, feminist activism, and, pertinent for this book, feminist ethnography. Such concerns stem from the acknowledgment of highly charged contexts of poverty, violence, marginality, and other gender-based oppression that find their way into research agenda. There is concern with what feminist anthropologists are supposed to do with the “data” they collect and how they manage the desires of their subjects who expect the ethnographer to do something for them in return for their time. There is also unease, bordering on who can call himself or herself a feminist activist ethnographer (Davis 2013, 26–27). These discussions parallel the ones I experience in the interdisciplinary graduate program where most of my graduate teaching is centered. There is a certain kind of celebration of “practice” (perhaps for good reason) in that space but little reflection on the implications of such emphasis and even less reflection on the playing field in which such “practice” or “activism” will play out.

I am not arguing that practice-oriented, applied, and activist projects are not academic enough, but one must reflect on the playing field of practice where complicities are part of the efforts to engage. The tenuousness of the insider/outsider dilemma cannot be resolved by mere conviction nor by the soundest research design; one’s research instead stands on these tensions and informs it. All forms of engagement take place in the context of ethnographer’s/researcher’s complicities. One must not lose sight of “homework” (Visweswaran 1997) to reflect not only on what one does in the field but how one locates one’s research within academic politics and activist practice, in my case this politics of doing. As a way out of this impasse I would like to clarify again that I see myself as an academic, as an interlocutor who is engaged in a feminist ethnographic longitudinal study for rural Darjeeling—not an activist in the sense that has become very popular in U.S. academic feminist circles (see also Fernandes 2013). I still believe that there is a place for academic feminist research that can produce policy or “practice” or “activism” supporting research, as evident from where my work is being cited. Interestingly, I notice that the academic content of this book may fit some contemporary definitions of feminist activist ethnography since the feminist ethnography presented in this book “draws on methodological strategies that embrace the everyday

experiences of people—especially those forced to live on the margins” and treats these experiences “as epistemologically valid” (Davis 2013, 26). My approach to fieldwork and writing this book is best expressed in fellow feminist anthropologist Piya Chatterjee’s (2009: 132) words:

Epistemological divides are geopolitical divides, and some bridges between “activisms” and “scholarship” span the incommensurable. It is through these impossibilities, then, that I counterintuitively attempt to build an “imagined bridge.” In doing so I make no claims about chasms crossed: incommensurabilities made commensurable; the strange, familiar. Rather through careful ethnographic reflexivities on my own positionalities and knowledge claims, and through careful tracking of difference and power, I seek to build the possibilities of both understanding and solidarity. These require, in the deepest ways, leaps of faith, hope, and tenacious optimism.

Over this decade-long engagement I witnessed small glimpses of such leaps of faith in the friendships that I formed. Here’s a picture of a fifty



Figure 1.1. Price of Friendship.

rupee bill that one of my interlocutors asked me to take a picture of a put in my *kitab* (book). She wrote both our names on this bill and the place where we were and then paid the shopkeeper for the stuff she bought from him with this particular bill. When I asked her why she gave it away she told me “maybe one day this note will return and I will remember our friendship that has stood the test of time and the pressures of the world.” It is my hope that in writing this book, I would have at least tried to be a true friend, if not totally successful.

Note on Methodology

My impetus to talk about “homework” (Visweswaran 1997) in the last section comes from keen observation of research methods terminology used to describe and slot interpretive and qualitative research found in standard social science classrooms popular in the United States. Since I insist that research on social sustainability must be grounded in the minutiae of everyday gendered existence, it is even more important to engage this issue of “homework.” Very early on in my academic training in the U.S. I encountered phrases, such as “backyard research,” in methods textbooks (Creswell 2013). While intended as a cautionary exercise in emphasizing research ethics and objectivity, I perceive in the use of this terminology (backyard) a potential to “other” forms of enquiry perceived less objective and with lesser universal appeal. A less pejorative term used for ethnographic work is “exploratory.” The rhetorical power of such research terminologies can directly impact the scope of making policy recommendations when it comes to sustainable development as well. The reason why questions of social sustainability have remained in the backwaters of sustainability or sustainable development-related literature is often a result of “homework” related politics. Since feminist methods, let alone feminist anthropological methods, did not figure in my own graduate training or taught in standard research methods classes, I feel the need to situate my work as a certain kind of academic feminist anthropology involving longitudinal ethnographic fieldwork, which is much more complicated than just being in my backyard, i.e., another place in my home country India.

The use of “backyard” has the potential to valorize the research of those who have traveled a distance to do fieldwork and “other” work of those who return to their “native” countries to do research after having lived as a legal alien in a foreign country for more than a decade. The complexities of inhabiting these different worlds of citizen, alien, and

research are somewhat lost when one designates certain research as “backyard” work. When I read Kirin Narayan’s seminal article “How Native Is a ‘Native’ Anthropologist?” (1993) I realized, that although these terminologies may be well positioned they do not reflect the complex positionalities of researchers. Not acknowledging such complexity in turn harbors the danger of misrepresentation in this media saturated world. Thus, there is a dire need to develop an intersectional approach (Cho, Crenshaw, and McCall 2013) to deconstruct the use of these terminologies in our academic “home.”

To comprehend these “locations” one also needs to attend to the dynamic nature of the “insider”/“outsider” position of any researcher. Nancy Naples calls it the bipolarity of the “insider”/“outsider” status while complicating feminist standpoint epistemology, requesting scholars to tackle issues of power and privilege directly as part of designing and implementing research. An essential part of “home-work” then is to recognize these bipolarities and what Narayan calls hybridities in the research process and write about them as part of methodology. Such processes are important since, as Naples notes, “Women and others who are not typically found in positions of power within educational institutions are objectified in academic practices and constructed in ways that distort or render invisible their experiences and their everyday activities” (2003, 52). It is through these experiences of locating oneself, ones work, and ones interlocutors/research subjects voices that data on any topic emerges. Instead of treating these experiences as particular, I posit that for any grounded research, these locations become nodal points of understanding the diversity of sustainability practice and its relations to local and global power play. It helps us understand whether gendered forms of being and becoming articulate (or not) with emerging market based justice regimes like Fair Trade and organics.

It is important that we make these methodological signposts such that more than a decade of grounded reflexive longitudinal ethnography in one of the poorest places in South Asia cannot be easily read off as exploratory “backyard research.” As the previous sections in this chapter demonstrated, I had to make a great effort to not become willfully complicit with the hegemonic forces at play in one’s field site to collect “data” and engage in negotiations which respect the desires of ones interlocutors. Most importantly college students need to understand the politics of objectivity, witnessing, and evidence when reading our books and encountering their mentors as complex subjects. As Nancy Naples so eloquently points out, “. . . a belief in the value neutrality of social scientific and other intellectual practices, in fact,

serves to mask the relations of ruling embedded in the production of knowledge in the academy” (2003: 52; see also Harrison 2007).

Having such nuance will enable everyone to appreciate what research one is about to do, what methodology one is going to use, and how the process and product are going to be evaluated and situated within the academe (anthropology and beyond). While in feminist anthropology, and in anthropology more generally, the post '70s call to being reflexive, acknowledging one's privilege and “outing ones politics” is now considered an exercise in objectivity, such nuance in methodological choice and approach is still lost to colleagues in social sciences who fail to understand that every piece of research is political. People who do quantitative analysis of large data sets in the United States are also “backyard researchers” (and native anthropologists if they are doing research among U.S. populations) since their assumptions about U.S. society might frame their survey tools or choice of subjects. But somehow the labels are reserved about certain kinds of researchers and these terminologies tend to take us back to a time when “natives” like me were thought to be “subjects” and not producers of knowledge. My propositions in this paragraph are to refrain from making any kind of moral claim to innocence, authenticity for “native” anthropology, as I am deeply aware of my class and caste privilege in India as much as I am aware of my marginal status in academe as a nonresident alien woman from the developing world.

The depth of what I describe is not new, but it is not talked about enough outside feminist circles. Someone like me may find it tough to balance a position between that of an “expert/authentic insider” (sort of as culture broker for all things India) or a “native anthropologist” without the objectivity of an outsider. Therefore, Narayan proposed that we embrace and enact hybridity (1993, 679) not only in our understanding of culture (and our subject's lives) but in the scrutiny of ourselves as researchers. I belabor this point about terminological choice since, as most feminist scholars know, language and power are inextricably linked. It shapes how one frames oneself, one's methods, one's choice of subject because people make decisions about what is “data” based on such framings. I teach graduate students in an interdisciplinary field and more so than in anthropology classrooms I feel the need to teach and think about the complexity of research methods and politics of labeling within it. Feminist scholars and feminist anthropologists have for years discussed and written about positionality, bias in research, and ethics without these labels, but they are never cited in standard social science research design books and the de-contextualized caution about “backyard research” continues unchecked.

I see myself as an interlocutor and not an activist speaking for poor women in Darjeeling's rural areas. I cannot find solutions to all their problems, I know they have a good sense of what's best for them (in a non-essentialist way); my task is to produce knowledge that leads to a better understanding of their resilience, entrepreneurialism, creativity, and intelligence through which they *sustain* themselves everyday as development fads ebb and flow through their communities, sometimes as missionary help, sometimes as a gesture of plantation management, sometimes in the form of student volunteers, local NGO projects, Fair Trade certifiers, political party projects. That is what I am best trained to do: write about reality in their terms, knowing that the process is fraught with power, privilege, anger, disgust, but also mutual coexistence and respect. Having an Indian passport makes Darjeeling no less familiar and strange. We all just work with different biases and first principles and operate on hybrid playing fields, both researcher and subjects. Researchers also play "serious games" (Ortner 2005).

This book is based on ethnographic research completed over eleven years, beginning in 2004. Being fluent in multiple languages spoken in this district in India's eastern Himalayas, I was able to collect long-term, grounded data on women's past and present justice imaginaries and related everyday practices. The core of my methodology is a combination of life-history and semi-structured interviews with long-term participant observation. I also conducted content analysis of Fair Trade policy documents and social media stories of women's empowerment in Darjeeling and beyond. I did fieldwork in two Fair Trade-certified plantations (Sonākhēti and Phulbāri)² and the largest Fair Trade-certified tea cooperative in Darjeeling (Sānu Krishak Sansthā). Among the 147 respondents I interviewed in the plantation sector were ordinary workers, managers, plantation owners, and Fair Trade bureaucrats. In the cooperative area I interviewed about 97 people. Thereby I identified a core sample of 40 women plantation workers and 30 women smallholder tea farmers, whose narratives form the cornerstone of this ethnography.