This book is based upon years of engagement with Lebanon and with Palestinian refugee communities. My time there included the summers of 2007 and 2008; a year from October 2009 to 2010; May–June 2011, May–June 2012, and January and May–June 2014; and two weeks in 2018. Over the course of these trips, I visited eight of the twelve UNRWA-recognized refugee camps, spent time in multiple Palestinian gatherings (small settlements that are not recognized as camps but that receive some UNRWA services), and lived in Lebanese-Palestinian neighborhoods in Beirut and Saida as well as in a refugee camp in South Beirut.

From 2009 to 2010, I lived in three predominantly Lebanese neighborhoods in West Beirut (Raouche, Caracas, and Sanayeh) where Palestinian associational and political officials also lived and where my Palestinian friends were comfortable visiting me.¹ When I returned in 2011 and 2012, I lived with Palestinian friends in Tariq al-Jdideh, a Lebanese-Palestinian neighborhood in South Beirut near the Beirut Arab University, just north of Sabra. Starting part time in 2012, continuing full time in 2014, and returning in 2018, I lived with one of my interlocutors and her family in Burj al-Barajneh, a refugee camp adjoining the Beirut suburb of Haret Hreik. I also spent several weeks staying with my interlocutors in and around Saida. I visited family homes and political offices regularly, shared meals, helped children with homework, participated in household chores, attended weddings, and exchanged gossip, jokes, and news. Living in the camp allowed for participation in some of the most informative and frankest conversations about my research, as I was around when friends dropped by, when news flashed
across the TV screen, when storylines came to a dramatic climax on soap operas, or when people stayed up late relaxing after a long day.

Aware of existing resentment toward researchers in some of the refugee camps, as well as of the extractive dynamics that characterize many research plans, I endeavored to productively contribute to the communities where I worked. During the summer of 2008 and from October 2009 through October 2010, I volunteered with three civil society organizations, each of which was loosely associated with a different Palestinian political current. In the summer of 2008, I taught English for a well-known social association focused on educational and social support for women and girls in the camps. From fall 2009 to summer 2010, I worked for a small civil society organization that focused on training Palestinian journalists, which was run by a member/former member of various leftist parties. Over spring and summer of 2010, I spent increasing time at this educational association’s office, helping my supervisor with translations, grant applications, and research projects. Like other associational employees, I participated in human rights workshops in Beirut and South Lebanon, led a workshop of my own regarding the role of the international media in the camps, and spent hours discussing the educational situation for Palestinian refugees with parents, political officers, and United Nations officials. My work came to the attention of the camp’s Popular Committee’s leader, who asked if I might be willing to tutor primary-school students in a locally managed, United Nations Children’s Fund–supported community center, an offer that I accepted. Throughout my time in Lebanon, I tutored children; helped youth navigate their foreign university, scholarship, and job applications; and clarified questions about immigration lottery procedures.

As a US citizen, I could not feasibly work with some civil society groups—specifically, groups linked to certain Islamist and Salafi factions, as well as those linked to the PFLP. This was because of legal restrictions stemming from these organizations’ association with factions the US government has declared foreign terrorist organizations (FTOs). My volunteering was consequently limited to organizations historically linked to specific secular-nationalist and specific leftist parties. This constraint meant that my everyday interactions with secular-nationalist and leftist parties were much more extensive than my experiences with Islamist parties, though I made every effort to consult materials that represented the latter’s ideological standpoints.

Given the social and legal precarity that shapes Palestinian refugee communities in Lebanon, I wanted to be as open as possible with my interlocutors so that people could make decisions not only about whether to engage with me, but also to what extent. Volunteering for months before I began more intensive ethnographic work allowed people a chance to get to know me, ask me questions,
and form opinions about my character. In my consent procedures, I extensively discussed my funding sources, including the distinctions between the fellowships I was awarded through the National Science Foundation (which supported several years of my graduate education), and through organizations such as the Social Science Research Council on one hand and fellowships that I did not have that are administered through the US Department of Defense and the US Department of Education (e.g., Fulbright) on the other. For example, many interlocutors were especially wary of Fulbright grant holders; they associated them with espionage. However, many of the people with whom I was working were also well-versed in the idea of organizations such as foundations supporting students; many Palestinian students earn grants to study abroad, in view of the limited access to higher education for them in Lebanon. I also extensively discussed participant observation and my interviewing approach. Many people were enthusiastic, pushing me to “live the reality”; others repeatedly asked if I wanted to record or film them “like a journalist.” Assuring them that I would request neither audio nor video recording was often the reason that someone agreed to speak with me. Some people also declined to be interviewed, including several cadres to whom I was close and whose personal histories I knew; even if I know those stories, they have been omitted from this volume.

The project received ethical approval under Institutional Review Board protocols H10075 and H07177 at the University of Chicago and protocol 1312S46161 at the University of Minnesota. All research occurred under conditions of confidentiality, and all names provided herein are pseudonyms. Where essential to preserving confidentiality, identifying details have been masked or omitted. Research was conducted predominantly in Arabic, with some interviews in English.

**Organizational Ethnography**

My growing familiarity with the camp’s political parties and social associations allowed me to observe the relationships that undergirded organizational politics in sharp relief and in multiple contexts, whether in parties’ offices, in schools, or in private homes. To understand what kinds of roles, relationships, and social network ties exist within Palestinian political factions, I conducted participant observation in Palestinian communities, including ten months of organizational ethnography among members of Fatah’s Women’s Office. I observed meetings (at the camp, regional, and national level, open-invite as well as invite-only), visited party offices, collected and studied the party’s publications, watched its television channels, socialized in members’ and affiliates’ homes, attended events such as poetry readings and demonstrations, and
gathered materials such as party-produced yearly planners. I also studied sites of political discourse such as an email listserv for publicizing party events and public Facebook pages. Pursuing long-term, intensive research with Fatah members afforded me an opportunity to study the interface between members’ formal organizational networks and their quotidian ties, which include kinship, marriage, and friendship. Research with “public” organizations led to sustained participant observation in more “private” spaces. At least partially owing to my status as a young, unmarried, female researcher, I was increasingly invited into family homes and to social gatherings. Developing and maintaining long-term ties within Fatah afforded me the opportunity not only to observe contemporary organizational practices and hierarchies, but also to introduce comparative questions about the past to my interlocutors. Conversations about veterans’ health-care and salaries, for example, presented the opportunity to ask about party funding and how it had operated in the 1980s.

Over the years, I developed close relationships with seven families linked to Fatah. By “close” I mean that I communicated regularly with more than one family member (e.g., via text or WhatsApp), visited family members frequently at home or work, regularly ate meals at and slept over in family homes, helped with household chores, and attended party events with family members. I came to know members of multiple generations. For example, in one family, I repeatedly interacted with twenty-nine members whose birth dates fell between the late 1940s and the present—a widow born in the late 1940s, her children born between the mid-1960s and the mid-1980s, her children’s spouses and their mothers, and her grandchildren (born between the 1980s and the present). Half of the children born between the 1960s and 1980s currently serve or have served in a formal office in Fatah; several others would identify as being “with” Fatah. Another family with whom I was similarly close invited me to stay with them in their house in one of the camps; treated largely like a visiting cousin, I held myself to many of the same standards as other young women in the same family (e.g., observing a curfew when staying in the camp and contributing to housework). This approach allowed me to follow and participate in discursive networks that spanned multiple organizational and quotidian domains, operated across genders, and bridged generations.

**Participant-Driven Ethnohistory**

As described in chapter 1, I came into the Fatah fold in late April 2010 after a friend casually asked me to copyedit her weekly report of the office’s activities. I then participated in the office’s woman-centric projects and events surrounding Nakba Day, sharing in conversations related to the design of a poster and
pamphlet that featured notable Palestinian female fighters and politicians. Discussing this poster’s design and attending the subsequent exhibit at the Nakba Day events facilitated many of my introductions to women who had worked in the clandestine support network described in chapter 6.

Two weeks later, the Mavi Marmara incident occurred; I participated in the subsequent demonstration in Beirut with members of the Women’s Office. Women who were present repeatedly cited my presence at this event when they later introduced me to other members. Around the same time, a longtime US White House Press Corps member, Helen Thomas, resigned after being criticized for making controversial remarks about Israel; I shared in a meeting where the Women’s Office members drafted a formal statement of support not of Thomas’s comment (though they did not oppose it), but, in their words, of a fellow woman’s right to freedom of political expression. This in-depth conversation about women, politics, and journalism prompted one Women’s Office member to unearth a binder of media clippings from the early 1980s, which she had assembled as an employee in a joint PLO-LNM media office and preserved throughout the following decades. The officer gave me the binder, telling me that she didn’t want the “dusty thing” taking up space in her house anymore and that she hoped it could help in my work. She did this immediately following the meeting about the Helen Thomas statement, in front of other members of the office. Since the officer was known as a figure of authority and someone who was not easy to please, her decision to do me a clear favor served as an informal approval of the project, paving the way for my more in-depth, prolonged engagement with the Women’s Office.

Members of the Women’s Office often facilitated my relationships with other camp-level and regional offices within Fatah. I began visiting some offices several times a week and developed working relationships with several local officers, which deepened over the course of that year. I began spending extended periods of time with Fatah members: stopping by their offices a few days a week, tagging along on house visits, attending festivals and commemorations, sitting in on scout meetings. I spent many evenings with Fatah members, former members, and those close to them, smoking arguileh, watching soap operas, slicing potatoes or coring squash before dinner. In addition to observing members’ behavior, I participated in a number of formal events by doing anything from engaging in political discussions during rallies to singing and dancing dabke (a traditional line dance) at scout meetings. My presence at these types of events also allowed me to collect and analyze what Schatz (2009, 6) terms “human artifacts” such as custom-printed kaffiyehs that parties gave to members on formal occasions, t-shirts from scouting clubs, jewelry that youth activists wore, key chains that members of the Woman’s Office traded, and commemorative
plaques that officers exchanged. My engagement further intensified during my return trips when I continued to regularly visit party offices—by 2012, across three camps—began living with current and former cadres, and was allowed to observe a camp-level women’s Central Committee meeting. In 2012, largely thanks to these long-term connections and my own shifting positionality as someone who was increasingly known by members of Fatah and around South Beirut, the Fatah representative for Beirut granted me permission to conduct formal historical interviews with military leaders. With official sanction, I could ask officers directly about their strategies and tactical decisions during the 1980s and their aftermath, which enabled me to assemble critical evidence related to meso-level changes in organizational practices and routines over time.

Though my ethnographic engagement with Women’s Office members gave me special access to Fatah, I built strong working relationships with members from other political factions and currents as well. I socialized with members of other factions and recognized over time that people tracked how close I was to members of each one. Before I left my volunteer position at the association that trained journalists, I spent hours every week talking politics with my supervisor, a long-time leftist who had retired from his organization, and his friends, who belonged to multiple left-wing factions. There were days when I would go from mid-morning coffee with a former member of the PFLP, to a Fatah-affiliated family’s house for lunch, and to the apartment of a former member of the DFLP for dinner. As noted in chapter 1, I also spent extended periods of time with UNRWA employees in offices, schools, and clinics, discussing everything from healthcare policy to garbage disposal to factional interference in school activities.

An ethnographic approach also allowed me to progressively build working relationships (Fujii 2017) and trust with my interlocutors, gaining access to insider perspectives, experiences, and meaning-making practices while situating them in local and historical context, rather than taking external categories of participation or behavior for granted (Burawoy 1998, 2003; Bayard de Volo and Schatz 2004; Pachirat 2009, 143–44; Wedeen 2009, 2010; Yanow 2012). Gathering knowledge of changing funding, communications, and supply structures over time made it possible to ask about and consequently map changing social network flows. Ethnographic immersion also granted me the perspective necessary to asking the right people informed questions about factional histories; for example, I learned that despite their ranks, many national officers knew little of critical clandestine operations during the 1980s (see chapter 1). Trust and analytic insight gained through this kind of long-term engagement are crucial to accessing and situating restricted information that is unlikely to be gathered
in a one-shot interaction and that is not captured in documents or other accessible historical material.

**Interview and Life History Approach**

My access to research participants depended heavily on the reputation that I developed through my ethnographic work, volunteering, and “off-hours” socializing in various Palestinian social and political circles. For example, one day, a group of former military cadres from Shatila drew me a detailed map of the camp’s 1985 defensive front and described the specific network of fighters from Burj al-Shamali who designed it; they had originally stopped into a party office to see if their new ID cards had been completed. They recognized me from several previous interactions and started to ask about my research as they waited. Almost all my interviewees, including this group, participated in the project after knowing me for several weeks, if not months, in a more casual, passing context; many initiated conversations on the topic of more sensitive events themselves, in the context of a more general interview or conversation. Several interviewees put me in contact with friends, relatives, or colleagues who had also participated in political activities over the years; I approached these individuals to schedule interviews only after ensuring that my initial contact had already explained who I was, where I was from, the nature of my project, and the types of questions that I would ask to the potential interviewee. Only after the potential interviewee had granted permission for me to establish contact did I suggest an interview.

While I interviewed each participant for at least an hour, I spoke to the majority of them on at least two occasions and was able to interview many people for up to twelve hours across multiple sessions. This count excludes interactions with the families with whom I stayed for days or weeks at a time, the informal interviews that I conducted, and the conversations in which I participated on an almost daily basis as part of my participant observation in militant organizations’ offices, social associations, UNRWA installations, and private homes.

Central to this effort, I conducted twenty-four extended life history interviews with members of emergent—and largely underground—organizations that are described and analyzed in this book. During life history interviews, I recorded each interviewee’s demographic information (e.g., age), family attributes, their home village in Palestine, their birthplace and other places of residence, their current and former affiliations and organizational roles, whether they were imprisoned or deported, and, critically, their position in relational flows (specifically their handling of information, finance, and material goods such as weapons
and publications). I cataloged much of the same information for their family members; one of the early questions that I asked was “can you tell me about your family?” Later in the interview, I often learned that family members had been wounded, beaten, imprisoned, or even killed; many had also emigrated for safety. At other times, my interviewee had already invited family members to join our conversation. Additionally, I queried interviewees about general trends, behaviors, and decision-making processes within their respective organizations or social organizations.

I strove to conduct interviews in a way that afforded my interviewees maximum agency. While my questions focused on organizational dynamics, I was still cognizant of the potential that engaging with them could surface upsetting memories. I always started interviews with open-ended, broad questions. I did not ask for details about people’s involvement in specific violent events, avoided requesting details of my interlocutors’ own victimization, and checked in with my interviewees throughout our conversations by asking how they were doing, if they wanted to take a break, or if they wanted to move to a different topic. However, I also did not stop people from discussing issues related to their participation in resistance or their victim/survivor status, which some people did choose to share. During difficult moments, we often took smoke breaks or stopped to share coffee or a meal before continuing. I always emphasized that I could return if they wanted to continue a conversation another time, or that we could simply end our exchanges on a certain topic at that moment. Several interviewees asked to revisit conversations when I encountered them months or even years later.

Though I had the most expansive access to members of Fatah, I was able to approach historical interviews in a fashion that more evenly represented both the PLO loyalist organizations and former Palestinian National Salvation Front (PNSF) members. Organizations’ members often articulated that I needed “get the real story,” and seemed almost universally pleased to discuss the 1980s. Though I spoke to members of over a dozen different Palestinian militant organizations, ranging in ideology from communists to Salafis, I was never able to formally interview long-term, current members of Fatah al-Intifada or al-Sa’iqa. These groups, along with others whose members I was able to interview such as the PFLP-GC and a wing of the Palestine Liberation Front (PLF), are the most strongly aligned with Syria. I had hoped to approach members of these organizations when I returned to Lebanon in 2011 and 2012. However, these trips, as well as later ones, occurred in the midst of the Syrian revolution. The heightened presence of Syrian intelligence agents and a shifting political environment made establishing new contacts ethically questionable for me because of the contentious nature of the Syrian government’s past and current activities in Lebanon. In some cases, I knew
secondhand that people had relatives suffering in besieged communities in Syria and felt it would consequently be insensitive to propose an interview about the War of the Camps.

**Talking about the Past**

Researchers, as Elisabeth Jean Wood (2003, 33) explains, face at least three challenges to evidentiary quality when conducting historical interviews: “the accuracy and intensity of the respondent’s initial memories, the subsequent shaping of those memories through social and cultural processes, and the respondents’ objectives in the ethnographic setting of the interview itself.” With the first, factors such as temporal distance from events may make people likely to forget details or to misreport how events transpired. However, time may also afford people the opportunity to process, evaluate, reconstruct, and analyze. Both social scientists such as Wood (2003, 33–34) and psychologists such as Mark Freeman (2010, chaps. 2, 4–6) emphasize that intense memories can also become clearer and more vibrant over time, and therefore are often more likely to be subject to recall. Indeed, many of my interlocutors had trouble identifying the exact sequencing of certain events or, for example, the months in which events occurred. In other moments, though, their memories of specific occurrences or practices—e.g., the rumors that swirled in the aftermath of the Sabra and Shatila massacre; the changing ways soldiers frisked women at checkpoints; the way a high-ranking officer slept on the floor with his men—were frequently sharp, focused, detailed, visceral, and vivid. These contrasts helped me to assess which events might have been relatively more important in my interlocutors’ personal trajectories and enabled me to search for commonalities in important themes across interviews and field notes.12

People also interpret their memories through the lens of contemporary contexts, politics, and relationships (Zerubavel 1996; Auyero 1999; E. J. Wood 2003, 34–35) and often relay them in the context of intersubjective encounters with a researcher. In my research, what initially seemed like romanticized memories surfaced, often seemingly paradoxically, about deeply trying times.13 Methodological techniques exist to help researchers to generate and leverage insights to be gained from encounters with incomplete or “rose-tinted” memories. For instance, Jocelyn Viterna (2006, 13) notes that she “structured [her] questionnaire around past events rather than past attitudes because memories of events are more reliable.” I emulated this technique in my life history and other in-depth interviews, especially as I sought evidence of actions taken resulting from collective interpretations of violence. Additionally, I structured my questions according to levels of detail. If, for example, I was assembling data to reconstruct
an interviewee’s social network over time, I would ask them about their family and close friends early in the interview, then return to family as we discussed their different roles over time (e.g., “Could you tell me what your sisters were doing when you were smuggling cigarettes?”). This technique flagged my interest in family and friends early in the interview, priming respondents to mention others’ roles as they discussed their own. However, I also became interested in the role that comparatively “rosy” memories of difficult periods played in contemporary organizational politics, treating those discourses as contemporary organizational data, specifically evidence of intraorganizational cultures, in their own right (Parkinson 2016).

Javier Auyero notes that memories of particular salience in the present day—in one interlocutor’s case, her memories of all the gifts the Peronist Party had given her—might be clearer and more accessible to them because of contemporary contextual factors (Auyero 1999, 332). In my own work, the changing present-day salience of events in the 1980s—initially heavily focused on issues related to factional funding and access to social services such as healthcare—provided a pathway to discussing the more “concrete” issues of roles and relations within 1980s guerrilla organizations. Many party cadres also saw the 1970s and 1980s as a golden age for the factions (Parkinson 2016). However, the project also gained new resonance during the Arab Uprisings, as people were excited to talk about their own past activism in light of contemporary movements in places such as Egypt, Syria, and Yemen.

**Political Change and Issue Sensitivity**

Following 2011, the Syrian Civil War’s escalation dramatically shifted the political context in which I was conducting research. Hundreds of thousands of Syrian refugees as well as Palestinian refugees from Syria came to live in the Palestinian camps where I conducted much of my work, often renting apartments from my interlocutors and socializing with them. A wave of foreign journalists and researchers followed, as well as Syrian and Lebanese intelligence services, quickly contributing to people’s existing feelings of research fatigue, surveillance, and exploitation (Nayel 2013; al-Hardan 2017; Sukarieh and Tannock 2019).

I grew increasingly concerned that attempting new formal interviews and reaching out to participants I had never met before would endanger both my interlocutors and myself, in view of the Syrian government’s past role in training and supporting both Amal and dissident Palestinian factions and the increasingly noticeable presence of Syrian agents in the camps themselves. After 2012, I stopped actively seeking to conduct new interviews about the War of the Camps and the Syrian occupation of Lebanon. By 2014, I had stopped talking about the
War of the Camps with all but my most trusted interlocutors. In a few cases, my interlocutors brought up my research on the War of the Camps with Palestinian friends from Syria, specifically in discussions of the siege of the Yarmouk Palestinian camp in Damascus with people who had fled. Once, when my interlocutors explicitly mentioned one of my publications and its contents, the conversation became an opportunity to talk about the skill sets and knowledge base that Palestinians in Lebanon had prior to the War of the Camps, whereas Syrians living in Yarmouk “didn’t even have Kalashnikovs” (author’s field notes, January 2014). While I engaged in these conversations and occasionally offered historical observations, I was extraordinarily careful of what I said, in light of my positionality as a US-based researcher and the potential for anything I said to be misinterpreted by both Palestinians from Syria and intelligence services.

**Moving across Political Lines: Ethical Considerations**

Countless ethical considerations emerge as researchers embark on intensive research in fragile and violence-affected settings, many of which have been discussed extensively in other works. In her work on the Rwandan genocide, for example, Lee Ann Fujii discusses what she terms the “insider-outsider” distinction. She describes how as a non-Rwandan, being an “obvious outsider allowed [her] to ask questions that might have seemed too obvious, and thus suspicious, if posed by a Rwandan” (Fujii 2010, 34). Yet in ethnographic and other participatory research, sources of “outsider” status are almost inevitably interpolated by relationally based proximity to specific interlocutors. Researchers’ ethical commitments necessarily shift as a result. While “outsider” researchers (one of which I was, as a white woman who held a US passport) may be able to ask the “too obvious” questions initially, interlocutors’ expectations change as relationships deepen. For example, over the months and then years, people with whom I had repeated interactions anticipated that I would change the type of questions that I asked. My not knowing someone in the camp’s political affiliation was acceptable to my interlocutors for a few months; it later became a mark of either willful ignorance or dishonesty.

In the contexts where I conducted research, people tended to conceive of their position in society and their safety in terms of those immediately around them, like their families, as well as their formal political affiliations (and thus the protection they could mobilize). The inverse also applied; distance from other sorts of actors granted security. It was common to hear people refer to each other in terms of their organizational affiliations, their occupation (especially if it was associated
with one of the parties), and their home camp. For example, in response to the question “Which Abir?” someone might answer: “Abir—she’s married to the guy who owns the cell phone shop near Karaj Darwish, they live in Sabra, they’re with the PFLP, but his mother is PCP. She works for a NGO.” Most people to whom I spoke instinctively ordered their lives in a similar way; in social network terms, who people considered close to them was primarily a function of social ties based on kinship, factional affiliation, friendship, or co-membership in a social club such as a dance team. They also could articulate closeness in near-textbook social network measures.

Political neutrality was not a particularly legible position in these settings. People still read individuals who were unaffiliated with any faction or who professed political apathy—as many of my friends and interlocutors did—as being “close” to certain factions and hostile to or opposed to others. For example, a friend in his late twenties who had left al-Sa’iqa was still widely considered to be “close” to the Syrian-allied parties because he worked in a community organization run by former members of the PFLP. Other friends, despite their refusal to join the parties, or their departure from them, were associated with them via their parents’ or children’s affiliations. I met only one person to whom no one assigned a factional affiliation or proximity; he worked for UNRWA and sent his children to private school.

In this sort of relational setting, a researcher’s attempt to perform neutrality or impartiality presents ethical questions because of the emotional and social stress those network positions can cause their interlocutors. For example, when one of my primary interlocutors, Sabah, became upset that I frequently visited Muna, the daughter of a former officer in a dissident faction, she initially told me I visited Muna too much given who my other friends were. Sabah might speak to Muna once a month in a public place, to be polite. But when it came to Sabah’s best friend in Fatah, she might visit her every day; a friend who was in another faction, like a former classmate, might get a visit once or twice a week. One of the ways that Sabah both measured and signaled closeness was in terms of the frequency of her social visits; Sabah and I saw each other almost every day. For me to visit Muna as frequently as Sabah was to imply that I had a dangerous degree of closeness with a radical, dissident faction (even if Muna wasn’t aware of her father’s politics and even if he was no longer an active cadre).

My interlocutors expected, not unreasonably given how their social circles operated, that I would adjust my behavior outside of my research with them according to the knowledge that I gained and the emotional bonds our interactions generated. As I became closer to individuals in several parties, they became comfortable scolding me as they would a family member or childhood friend, rather than keeping me at a professional distance. Occupying my specific net-
work position after beginning research with the Women’s Office increasingly came with a set of intense expectations. For example, my interlocutors in the Women’s Office began using the title of “Sister” with me, indicating to others a particular level of trust and social expectation; it was also a discursive move to claim me as an affiliate (that is, someone whom they considered to be “with them” if not a formal party member). Several pressured me to transfer my teaching position to a space under Fatah’s control. On top of representing an attempt at network closure, their move also reflected a desire to capitalize on the prestige that having a foreign scholar teaching in a faction-sponsored educational program would bring, a dynamic that surfaces across research contexts (Malejacq and Mukhopadhyay 2016, 1014).

I increasingly thought about how a “network sensibility” could inform my perspective on these social dynamics. Specifically, I considered how social proximity—understood, for example, in terms of repeated interactions, increasingly complex relationships, incorporation into multiplex social networks, escalating emotional obligations, and growing trust—could either close or open potential research trajectories. Scholars have long contemplated the challenges and fallacies of maintaining neutrality, objectivity, and emotional distance, especially in conflict research (Robben 1995; Sluka 2007; Moser 2008). Romain Malejacq and Dipali Mukhopadhyay (2016, 1012) emphasize that regardless of scholars’ epistemological or ontological commitments, fieldwork in contexts affected by violence is characterized by “unavoidable partiality” and shaped by the “social micro-systems” that researchers construct in order to collect data. However, rather than partiality’s being portrayed as a negative, I want to emphasize that chosen proximity to one group—party, NGO, or sports team—is often essential to understanding aspects of organizational life such as cliques, boundaries, rivalries, and memory politics (see, e.g., chapter 1 as well as Parkinson 2016 and 2021a). Certain organizational behaviors, such as socialization into community norms or sharing organizational history, may not occur when a researcher divides her time and emotional investment among multiple groups—which political factions, clans, or religious communities—or if they try to avoid becoming “too” embedded to start. In terms of my own inclusion in these networks, my personal biography was less important than the way that I fit into a biographically defined role, especially in Fatah’s organizational structure. I was in my mid-to-late twenties, I identify as a woman, I was unmarried, and I was clearly politically active, so members often classified me much as they did potential recruits.

My friendship with Sabah became stronger and subtly politicized through my research activities with Fatah. As a result, the context in which Sabah saw my relationships with others evolved as well. In a continuation of the vignette above, she grew increasingly desperate to keep me from visiting Muna’s father, Abu
Ghassan, who had been affiliated with Fatah-Revolutionary Council. It became clear that my continued interactions with him were upsetting her. But for months, she used gossip to communicate her concern, rather than telling me her underlying motivation. Sabah was initially trying to shield a new, outsider friend (me) while simultaneously protecting her community’s image. After several months, during which our relationship deepened significantly, she eventually revealed that Abu Ghassan had tortured and killed people in Fatah—information that she would likely not share with an outsider (even a Lebanese friend, much less a foreigner), since it might have reinforced the camps’ reputation among many Lebanese and foreigners as violent spaces.

But, when she shared this information, Sabah was telling me, in no uncertain terms, that my relationship with Abu Ghassan was unacceptable to her and her colleagues. The obligations accompanying my status in a web of social and political networks meant that once I knew the “real story,” my professional and personal judgment would come into question if I continued treating Abu Ghassan as equivalent to other members of Fatah. Although I regularly interacted with people from multiple parties without Fatah (or any other organization) taking issue, interacting socially with someone who was as stigmatized as Abu Ghassan was unacceptable.

These interactions and similar ones increasingly revealed intimate and often emotional aspects of organizational membership and affiliation. While the head of the journalism NGO where I volunteered offered that Americans could never understand the exact mentality of factional affiliation, he still tried to explain it: “It’s as if everyone is part of a family. If someone hurts someone from a family, the whole family will avenge them . . . people stand up for each other.” A friend my age who had quit one of the factions later relayed similar sentiments when he explained the difficulty of leaving the group. He emphasized that without rule of law, Palestinians and Lebanese only had their relatives and their parties to protect them. People, he emphasized, needed to know where others stood. Indeed, when a Lebanese woman who worked in the camp where I volunteered attempted to falsely inform on me, members of the Women’s Office stepped in to defend me, potentially saving me from interrogation, or worse. The types of protection that factions offered—physical, psychological, economic, social, emotional, reputational—seeped into the practices of everyday life, expanding the domain of “political” work to the balcony, living room, and kitchen. Yet they also demonstrated how repeated micro-interactions—a snub there, a scowl there, a perceived threat there—worked to either positively or negatively reinforce social relationships, thus structuring broad social worlds (Parkinson 2018, 2021a).

As a researcher, I initially felt that I had an ethical obligation not to adjudicate claims between parties such as Sabah and her colleagues on one side and
those such as Abu Ghassan and his family on the other. Mostly out of fear, I did eventually stop visiting Abu Ghassan’s family. I made this choice after Abu Ghassan figured out that I knew his backstory—specifically about the torture and killings—and subtly threatened me.23 When he called to inquire as to my whereabouts after several weeks, I told him that I was ill and couldn’t come to the camp. I later started visiting his daughter, Muna, again, though with some trepidation. At first, I attempted to avoid Abu Ghassan by timing my visits against his work and prayer schedule, but the tactic started to become obvious. Instead, I started imitating the daughters of senior Fatah officers who I learned were neighborhood friends with Muna; I had never before realized that they strictly confined their social interactions to the highly public and observable space of the sandwich shop that Muna’s family owned. Their behavior was certainly intentional—only one girl from a Fatah family ever visited Muna in her home, and her family was not from the camp—but the availability of discounted food and drinks in the restaurant conveniently masked their motivations. Sharing the girls’ predicament revealed their subtle way of bridging a bitter and emotionally loaded divide and sensitized me to the ways that members of different factions negotiated historical animosities.

Research Assistance and Interpreters

I employed two Palestinian research assistants (RAs) in Lebanon as well as several US-based students for the archival element of my work (See Appendix B.) They did not engage in any of my participant observation or interviews. I deliberately chose RAs from different political backgrounds who lived in different cities. I paid them at an hourly rate commensurate with a graduate research assistant’s pay at the American University of Beirut. Both came highly recommended, and in both instances the recommender made a point of telling me the individual’s political loyalties. These were later confirmed—unprompted—by the RAs themselves.

With early exceptions in 2007 and 2008, I chose not to use an interpreter or fixer during my research. On the two occasions when someone accompanied me to interviews, one was in the summer of 2007 when I was not yet proficient in colloquial Arabic and accompanied a journalist friend to conduct interviews among militia leaders in Ain al-Hilweh (see chapter 2). Entry to the camp in South Lebanon required a military permit, which the fixer obtained; he then facilitated and translated a series of interviews with Palestinian organizational elites whom he knew from prior work with journalists. The following summer, I again used a fixer to obtain the necessary military permit to visit Ain al-Hilweh, but largely conducted my own interviews.
Given my advanced competence in Arabic, I decided to conduct my subsequent interviews without research assistance. On the one hand, Palestinian RAs might have allowed me to access a broader spectrum of people or helped to transcribe my unrecorded interviews more thoroughly in the moment (see, e.g., Fujii 2009). However, conducting my research without an interpreter or fixer carried several advantages that generalize to similar research sites. First, I felt that bringing an interpreter would have conveyed that I was a particular type of foreigner: the type that needs one (in the parlance of the camps, probably a journalist or someone doing government-sponsored research on violent extremism or refugee dignity, two much-maligned but commonly proposed projects in the spaces where I worked). I didn’t want to obtain the organizations’ carefully choreographed policy statements or their bluster about weapons that they provided as sound bites to many outsiders (which I witnessed firsthand during the 2007 interviews in Ain al-Hilweh). I recognized that bringing a RA would likely trigger this specific type of performance because I would likely be read, by many, as a journalist doing “one-shot” interviews, and provided with a practiced script.

Second, from an ethical perspective, I knew that people in many Palestinian communities felt that outsiders who used interpreters often bought access, rather than earning it. This system produced economic incentives for introducing any paying outsider to a camp community without that community’s consent. I was frequently present when Palestinian friends who were not my research participants scornfully joked about foreigners visiting the camps looking to meet “terrorists” or discussed their feeling that many foreigners treated Shatila, the site of an internationally known massacre, like a “zoo.” I did not want to employ the subtle coercion that can accompany a family member’s or friend’s introduction of a foreigner who is paying them into their milieu; I wanted to be confident that people freely chose to talk to me, rather than their feeling obligated to do so out of fear of risking a loved one’s or neighbor’s job.

Third, I was concerned about both my interlocutors’ and any potential RAs’ security. My interlocutors often felt that “insiders” probably posed a greater threat to them than I did. On both early occasions when I used a fixer, my interviewees’ vetting of them took a substantial amount of time; people obviously worried about what a fixer could do with any off-the-record or background information that they shared. In some settings, community leaders or politicians may question research brokers about the work they do with scholars or about who has been interviewed, placing the brokers in a vulnerable position and potentially compromising interlocutors’ anonymity (Malejacq and Mukhopadhyay 2016, 1019). Indeed, Roman Malajacq has commented that in some contexts, the people whom scholars and journalists hire as interpreters often later go into politics themselves, typically with inside knowledge of their rivals gained from their prior work with
media and researchers. In the Lebanese context, Palestinian factions and various divisions of the Lebanese security apparatus (controlled, in turn, by various Lebanese parties) frequently approach Palestinian youth to act as informers. Because of Palestinians’ precarious civil rights situation in Lebanon, refusal to serve in such a capacity could potentially place any RA in an uncomfortable position vis-à-vis their personal and family security.

**Practical Notes on Confidentiality and Security**

I took several additional precautions in order to ensure my interviewees’ security and comfort. These efforts included the following:

1. I never recorded interviews; I felt that doing so posed an obvious security risk (having digital files with identifiable voices) as well as potentially making interviewees uncomfortable and consequently hesitant to speak freely.

2. I used verbal consent procedures both (a) to avoid a paper trail, thus helping to ensure confidentiality and (b) to avoid the discomfort, fear, and suspicion that can accompany the request for someone who is either legally marginalized or a member or former member of a semilegal armed organization to sign any kind of document.

3. With interviewees’ permission, I took written notes both in colloquial Arabic (phonetically or using the alphanumeric system that people employed to write text messages, online instant messages, and posts on social networking sites) and in English. If I was confused by or didn’t know a term in Arabic, I asked people about it. When it was impossible or impractical to take notes, I tried to jot down coded keywords in notebooks, on paper menus, or on old receipts and to reconstruct events later in my field notes. I transcribed these materials electronically, encrypted them, and destroyed the paper notes to ensure confidentiality/anonymity.

4. I kept my computer passworded and encrypted files in a vault on the hard drive and backed up on encrypted external drives. Most of my research took place before I could reliably upload from Lebanese internet via VPN to a secure cloud.

5. I took several precautions to ensure that the technology that I used could provide as little information as possible if inspected or confiscated. During the main period of research in 2009–2010 and again in 2011, I
deliberately chose not to use a smartphone because, first, the Global Positioning System (GPS) feature could have been used by government agencies to place or locate me in certain areas as opposed to proximate to certain cell phone towers, and second, because if my phone were lost or confiscated, someone could potentially gain access to my emails as well as my phone numbers. There was extreme suspicion surrounding mobile phones during the main period of my research because of their increasing use for Lebanese government and foreign state surveillance, so I often visibly shut down my phone and removed the battery during many interviews. However, given the increasing prevalence of messaging through apps such as WhatsApp, Facebook Messenger, and Viber (often because these services operate using data or WiFi rather than phone credit), this approach became practically infeasible by 2012, when I began using a smartphone.

6. I tried to schedule as many meetings as possible in person so that meeting information did not travel through and was not stored on my phone. When I was visiting militant organizations’ or social associations’ offices, I sat out of view of their doorways so that passerby could not easily note my presence. Interviewees always selected the time and location of our meetings; we frequently met at outdoor cafés or in private homes. Depending on the interview and the interviewee’s preference, I met people both alone and in groups (usually families or organizational cohorts). I varied the pathways that I took through the camps to avoid passing particular offices and shops, and I took cabs from different intersections around my various homes so that I would not always encounter the same taxi drivers.

7. Interlocutors’ names have all been changed. I invited all of them to contribute their own pseudonym. In a limited number of cases where I cite from my interlocutors in both the contemporary era and regarding their past experiences, I use separate names for each time period to further protect their identities. In some cases, I have also obscured potentially identifying details such as their precise professional position or exact migration journey. In consultation with several interlocutors who felt that a particular experience could be linked to them personally, I have worked with them to substitute particular details in a way that still preserves the story’s core content.