

THE GENDERED DYNAMICS OF URBAN ETHNOGRAPHY: WHAT THE RESEARCHER'S "LOCATION" MEANS FOR THE PRODUCTION OF ETHNOGRAPHIC KNOWLEDGE

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ABSTRACT

In this chapter, I analyze how the intersection of geographic and social locations shapes ethnographic relationships in urban areas. While early urban ethnographers were acutely aware of the importance of geographic location, I argue that researchers' social locations were ignored, obscuring how their bodies and social identities lead to different forms of knowledge about the metropolis. I use data from a two-year ethnographic research project conducted in Caracas, Venezuela as well as interviews conducted with women qualitative researchers to consider gendered dynamics of fieldwork experiences and data collection. Using a framework of embodied ethnography, which posits that all ethnographic knowledge is shaped by researchers' bodies, I argue that men and women confront similar but distinct challenges while conducting fieldwork, and discuss what this means for data collection in cities. Specifically, I focus on how social control mechanisms, the gendered meanings attached to researchers' bodies, and geographic barriers in urban areas can facilitate and restrict fieldwork. Critiquing hegemonic standards within ethnography that encourage researchers to leave their bodies out of their tales of the field, I advocate for the incorporation of gendered research

experiences in our ethnographic writing with the aim of producing more complete narratives, but also to better prepare future ethnographers for fieldwork.

Keywords: Ethnography; feminist methodology; urban sociology; Chicago School; embodiment; danger

Miguel was a state police officer who had taken time off from the force to work for the *Consejo General de Policía* (CGP), one of the state institutions supervising the implementation of the police reform that was the topic of my dissertation. Miguel was a “promoter” with the CGP, a job that entailed organizing and training citizen police oversight committees throughout a number of states in Venezuela. Though he maintained the closely cropped haircut associated with police and military officers, his soft, almost shy, demeanor made it difficult for me to imagine him in uniform. Miguel was in his late thirties and was a passionate supporter of police reform. During his time on the force, he had witnessed concrete examples of why reform was so sorely needed. With some of the other committee promoters, getting information about meetings and activities was like pulling teeth. In comparison, Miguel kept me up-to-date on activities of all the committees he was working with, eagerly put me in contact with other promoters, and often invited me to accompany him around Caracas or out of town, both for committee and non-committee business. His passion for reform and my interest in studying it provided common ground and, I believe, was part of why Miguel was so helpful during my time in the field. However, Miguel had other motivations, which became clear as we spent more time together.

A few months into our work relationship, we spent three days traveling throughout western Venezuela of Venezuela, visiting police oversight committees in different cities and towns. Before we parted ways – Miguel was heading to his home a few hours outside of Caracas, the city where I lived and conducted most of my research – we had lunch in a food plaza in a large mall. After lunch, Miguel insisted on buying me something from one of the stores before we left. We walked around the store for what felt like an eternity – but was probably closer to 20 minutes – as I looked over items asking myself what it would mean to him if I bought a piece of clothing or cheap jewelry [...] definitely not perfume.

Less than 10 minutes after getting on the bus to head back to Caracas, I received the text message I was, unfortunately, anticipating. Miguel told me that he was hoping at some point we could have something more than a work relationship. He clarified that he hoped he wasn’t “making me uncomfortable but these feelings are real and I cannot hold them back.” Eventually, I would become somewhat accustomed to Miguel’s advances, which were usually very polite, even timid. At the time, however, I did not know how Miguel would respond to rejection; my immediate response to the initial text message was to panic, and then to feel relief that I wouldn’t have to see him until the next time he came to the city. One question tumbled round and round in my head: “How can I respond without pushing him away?” I had already lost so many contacts after refusing to date research participants, and Miguel had quickly become a key contact for my work with the citizen police oversight

committees. I ended up responding to him that I liked him very much as well but that “unfortunately for now I have a boyfriend.” I used these kinds of qualifiers to try to soften rejection, but hated that I felt the need to deploy them.

During another trip accompanying Miguel to committee meetings outside of Caracas, we arrived at a hotel in the middle of the night, where the manager explained that there was only one room left. It had been such a long day. I was exhausted, and I felt reluctant to go searching for another hotel alone at night in an unfamiliar city. After the manager said that the room had two beds, I decided to stay.

After awkwardly swapping trips to the bathroom to change, Miguel and I climbed into our beds. No more than two minutes later Miguel was standing beside my bed, crouching over me. He touched my shoulder as if he was trying to gently wake me and whispered into my ear:

I just can't control myself around you, if I don't leave right now. I am not going to be able to stop myself, so I am going to have to go somewhere else. [...] Either we have sex right now or I have to go somewhere else.

Panicking internally, concerned both about my access as well as my safety, I choked out: “Well I guess you are just going to have to find somewhere else to go!” He did so without contestation, and we slept in separate hotel rooms for the rest of the trip.

After almost two years of working together, multiple text messages about his feelings for me, and carefully planning the amount of time we spent together — how many invitations of his I accepted, and how many texts I responded to — Miguel texted me one day and told me that our relationship would have to end if he could not “have me.” At this point, I had already lost enough participants and was close enough to ending my dissertation research that his message did not bring me to the edge of a panic attack. I was able to tell him, in a more respectful, kind, and restrained fashion than I would have in a similar situation outside of the field, that I would not date him and that I wished him all the best.

I wrote parts of this vignette between 2012 and 2014, while conducting a two-year ethnography of police reform in Caracas, Venezuela. Initially, I included these interactions in a “venting journal,” a notebook or document ethnographers are sometimes advised to keep while in the field to jot down emotions, stresses, and challenges. These are kept in a separate document because, though they tell us something about what field research is like, they ostensibly are not part of the data we collect. Researchers might even be told, as I had heard and read while in a graduate school, that by excising these feelings through writing, we are better prepared to collect unbiased (or at least less biased) data.

I open with this extended vignette because my relationship with Miguel well illustrates how the research setting and the researcher's body are constitutive of the access we gain, the data we gather, and, thus, the knowledge we produce. My research placed me in a hyper-masculine field of relations in a city in Venezuela; within this field my gender, skin color, age, and academic status (as

a graduate student) became both resources and barriers. My position as a white woman from the US undoubtedly created advantages for me in this field. My blue eyes and white skin drew people's (mostly men's) attention to me, which opened up avenues of access. Miguel, for example, was willing to act as a gatekeeper to certain social worlds because of his romantic interest in me. However, gaining and maintaining this access often meant acquiescing to sexualization and sexual harassment. And, as a graduate student, I felt that changing my research topic or modifying my project to reduce these kinds of interactions was both financially prohibitive and would discredit me as an ethnographer. In order to maintain my access under these circumstances, I eventually learned to create buffers between myself and some of my gatekeepers. And, this is where working in an urban setting became something of a tool for me. The dynamics of my urban research relationships imposed boundaries on when and how I spent time with some research participants, which allowed me to maintain these relationships over a longer period of time and with fewer panic attacks than I might have had if conducting a similar project in a rural setting.

Ethnographic tales of the field (not only those from urban places) often reproduce the myth of the neutral researcher, that is, a researcher whose body does not disrupt, unsettle, or "contaminate" the research setting. When ethnographers do write about embodiment and research, they often include these reflections in the introduction or an appendix, separate from the "real" data. Although researchers' bodies are an integral part of the research process, how and why they matter is frequently obscured in ethnographic reports as well as methods courses and textbooks as ethnographers strive to meet standards that emerged from the fieldwork experiences of white (often elite) cis men. This separation of our emotions and our embodied experiences from our data – including experiences with sexual harassment that I and many other researchers have dealt with – is one of many hegemonic expectations that continue to structure how we write and teach ethnography. As Patricia Richards and I have argued elsewhere, standards in sociological ethnography encourage researchers to adhere to a homogenized narrative, a disembodied presentation of the research process and data collection – a narrative structured by androcentric, colonialist, and racist norms (Hanson & Richards, 2017).

From its inception, urban ethnography in the US has been heavily influenced by the concept of "locatedness" (Abbott, 1997). Writing specifically about the Chicago School, Abbott observes that, though searching for relatively predictable patterns, urban ethnographers recognized that these patterns were reciprocally determined by the interplay of temporal processes and social contexts. While the founding fathers of the Chicago School studied cities to understand "human behavior and human nature generally," (Park, 1915), searching for laws that would provide the basis of a science of society, they also recognized that theories must be about constellations of forces that intersect in particular spaces and temporalities.

What early urban ethnographers from the Chicago School did not account for within these constellations of forces was the researcher's body; in other words, how the ethnographer's own social location within structural systems of

power – race, class, gender, sexuality, and so on – shaped their access to different fields and actors and the stories they would tell when they came back. Urban ethnography grew up with an acute awareness of how urban dynamics – residential mobility, changing patterns of division of labor, population density, immigration – created new opportunities for research. However, the homogeneity of urban ethnographers in the early twentieth century allowed white men to elide how their bodies, identities, and self-presentation shaped these opportunities also. This is not to ignore the important work conducted by W. E. B. Du Bois and a handful of women who studied with the founding fathers of the Chicago School (such as Frances Donovan and Ruth Shonle Cavan). Nevertheless, both the objects of study (i.e., who and what was considered worthy of research) and how research was conducted was overwhelmingly dominated by the opinions and practices of elite white men. Indeed, as Rudwick (1969) and Morris (2015) have written, the groundbreaking research conducted by Du Bois was largely ignored by leading scholars, including those in the Chicago School. This marginalization had material implications, limiting the ability of certain scholars to mold future generations of sociologists (see Morris & Ghaziani, 2005).

Using the concept of participant objectivation (2003), Bourdieu has called for a reflexivity that aims our analytic lens on the social conditions that produce the social agents themselves, including those engaged in knowledge construction. This perspective demands an awareness of how our disciplines and positions within academia are constitutive of the research we conduct. While Bourdieu is primarily concerned with the researcher as academic and the scholarly gaze that may bias our research (see Wacquant, 1989), he acknowledges what feminist theorists have long understood: that experiences and biography are the product of intersecting social structures, which then structure how we interact with and study the social. Indeed, feminist and queer theory (among others) highlights how our positionalities in the geography of intersecting fields of power constitute how we know (e.g., Adjepong, 2017; Cho, Crenshaw, & McCall, 2013; Fotopoulou, 2012; Hill Collins, 1999; Scott, 1991; Sutton, 2010). If a principle tenet of Chicago School thought is that “one cannot understand social life without understanding the arrangements of particular social actors in particular social times and places” (Abbott, 1997, p. 1152), I would argue that the researcher must be included in this arrangement. If social facts are “located,” so are those people who produce them.

This requires that we acknowledge that researchers hold different positions within the same geographic location, depending on their bodies, identities, and self-presentation. Our fieldwork – the access we gain, the relationships we build, the experiences that we document – is constituted by local context as well as how our bodies are perceived within and “fit into” that context. The socio-spatial contexts in which we embed ourselves attach certain meanings to our bodies – how our bodies are read, the meanings they carry and convey, are different depending on our social and geographic locations. In other words, it is not just about our individual characteristics, self-presentation, or social identities, but also how these are activated, concealed, highlighted, or obscured by geographic and socio-spatial environments.

This is not to say that knowledge produced by researchers with different social positions will always be different, or that some researchers' bodies will always produce certain challenges in the field that others do not confront. Rather, my goal here is to consider how researchers' bodies contribute to the process of knowledge production. What might at first glance appear to be idiosyncratic experiences can, when subjected to sociological examination, become methodological and analytic resources. As Naples (1998, p. 48) has argued, by considering our standpoint within a given community context, ethnographers "can better reveal the multiplicity of perspectives along with the dynamic structural dimensions of the social, political, and economic environments." I argue that when researchers obscure how social location in a specific geographic context shapes the knowledge we produce about that place, we ignore these as resources, producing research that is less transparent, valid, and rigorous. It is essential, then, that researchers pay attention to not only the "locatedness" of their field but also their social locations in those places to tell more complete ethnographic tales.

In this chapter, I focus on the metropolis to consider how "subject positionings affect knowledge construction" (Doucet & Mauthner, 2006, p. 42) in urban locations. I use my own experiences conducting urban ethnography as well as data collected with women urban ethnographers to discuss how bodies and space (in this case, urban spaces) intersect to produce opportunities and constraints for researchers. Patricia Richards and I conducted the interviews cited in this chapter between 2014 and 2015 for our project on sexual harassment and the construction of ethnographic knowledge. I draw from the theoretical insights from this project to consider ethnographic work conducted in urban settings. In our recent book (Hanson & Richards, 2019), we argue that there are three "ethnographic fixations" that structure how we teach, write, and evaluate ethnography: solitude, danger, and intimacy. Perhaps particularly true in sociology, these fixations constrain how researchers can write about their bodies in relation to the research process and maintain academic credibility. Here, I focus on one of these fixations – danger – and how risk and opportunities in the field are gendered.

It is important to note here that the embodied ethnography we advocate for encompasses more than gender. This is an intersectional approach that calls for awareness of how social categories and social divisions shape knowledge production. Though I focus here on gender and urban ethnography, gender never operates in isolation.

EARLY ETHNOGRAPHIC KNOWLEDGE ABOUT THE CITY

Early urban ethnographies capture the vibrancy and excitement of city life. Much emphasis was placed on how city life unsettles and disrupts more "traditional" forms of living, producing opportunities and experiences that were previously closed off. Writing about the city in 1915, Robert E. Park, one of the founding fathers of urban sociology, described the attraction of the metropolis in part due to:

the fact that in the long run every individual finds somewhere among the varied manifestations of city life the sort of environment in which he expands and feels at ease [...] It is, I suspect, motives of this kind which have their basis [...] in something more fundamental and primitive which draw many, if not most, of the young men and women from the security of their homes in the country into the big, booming confusion and excitement of the city. (p. 608)

The significance of the city for early ethnographers, as Gubrium has written, was that “as a rapidly developing social form, it represented a world of opportunity for those who had rarely been offered it in such large numbers and on so many fronts” (2007, p. 239).

Of course, the majority of urban ethnographies from the early twentieth century were written about men, from the perspective of straight, (often elite) white, men who made up the Chicago School. The few ethnographies of women’s lives that were conducted in the early twentieth century also focus on the freedom that women experienced upon moving to the city. In *The Woman who Waits* (1920), Francis Donovan – one of three women who published sociological ethnographies in the early twentieth century – wrote about women in Chicago:

How many thousands are there whose stories we never learn? Why do they come? Because life is dull in the small town or on the farm and because there is excitement and adventure, in the city. The lure of the stage, of the movie, of the shop, and of the office make of it the definite El Dorado of the women. It is her frontier and in it she is the pioneer. (p. 9)

Perhaps, the most impassioned rallying cry to study these dynamics comes from an unpublished manuscript written by Park, counseling researchers to put aside the “musty stacks of routine records” found in the archives, he encourages students:

Go and sit in the lounges of the luxury hotels and on the doorsteps of flophouses; sit on the Gold Coast settees and the slum shakedown; sit in the Orchestra Hall and in the Star and Garter burlesque. In short, gentlemen, go get the seat of your pants dirty in *real* research. (Prus, 1996)

Ethnographers were not to get “too close,” however, in practicing “real” research. Michael Burawoy (1998) reminds us that Robert Park and the Chicago School championed the objective and detached character of ethnography. As with other methodological approaches, the goal of ethnography was to understand human conduct through systematic scientific investigation, to reflect “without distortion the way the world is [...] corresponding to a reality that is ‘out there’ and unchanged by the human study of it” (Harding, 1992, p. 585). In 1921, Park distinguished sociology from other social sciences because sociologists sought verifiable knowledge, while the discipline of history seeks, in his words, to “reproduce and interpret concrete events as they actually occur in time and space,” sociology aims to:

arrive at natural laws and generalizations in regard to human nature and society [...] to explain, on the basis of a study of other instances, the nature of the process involved. (p. 411)

Here we see how ethnography’s emergence from the natural sciences in the nineteenth century (see Kuklick, 1997; Wedeen, 2009) has continued to influence how sociologists think about conducting and evaluating the validity of

ethnographic research. Scheper-Hughes (1983, p. 115) has referred to the ethnographer mythologized within anthropology during this period as courageous and hardy, “always present and keenly observant, but invisible in his ministrations and empty of personal affect and passion,” an observation that could also be easily applied to early urban ethnography. This concern with scientific objectivity was used to keep women out of the social sciences, and marginalize the few who were able to conduct research.

Kristin Luker (2010, p. 23) has written that Jane Addams was marginalized by the Chicago School because “sociology came to think of itself as tough-minded and scientific [and] women seemed too [...] *feminine* for the tough-minded science” (emphasis in the original). Additionally, she argues, the social reform that Addams championed was at odds with the scientific objectivity that was triumphed by the Chicago School. Thus, to some degree early urban ethnographers did acknowledge that researchers’ bodies shaped the knowledge produced by researchers, but used this as a justification to exclude women (and people of color) from conducting research. Even those women who were mentored by founders of the Chicago School were labelled as producing ethnographies that were “impressionistic and descriptive rather than systematic and formal,” an observation made by Park in the introduction he wrote to Donovan’s second book (1929, viii).

The scientific goals of ethnography have been challenged by critical race, feminist, poststructural, and queer methodology. However, they continue to shape how ethnographers write themselves into, or leave themselves out of, their ethnographies. In my case, in early versions of my dissertation I left out any mention of sexual harassment, flirtation, and sexualization, worried that these experiences would delegitimize my project and me as a researcher. Indeed, hegemonic tales of the field encourage us to elide how embodiment shapes fieldwork, reproducing the notion that certain bodies “pollute” the research process. Silence around these issues keeps us from considering how gender structures urban ethnography. In the following sections, I contribute to breaking this silence by considering how researchers’ bodies within urban contexts contextualize data collection, creating advantages and disadvantages that shape the knowledge we produce about cities.

GENDER AND URBAN SPACES

In this section, I focus on danger as a social control mechanism associated with the inner city, a “spatial expression of patriarchy” (Valentine, 1989). Urban ethnographies lend themselves to narratives of danger and risk. If hardiness has long been an idealized characteristic of ethnographers, this takes on renewed meaning for those working in urban spaces. While Park is certainly not only referring to danger when he refers to getting “dirty” to conduct good research, urban ethnography in the US has certainly been preoccupied with poverty, deviance, and violence in ways that are reminiscent of anthropologists’ obsession with the exotic worlds and cultures of the “other” (see Ray & Tillman, 2018).

As mentioned above, early ethnographic research in the metropolis focused on the opportunities and horizons that had been opened for women during the early twentieth century. However, feminist urban geographers have placed particular emphasis on that ways that urban planning and land-use “created mobility barriers for women with young children, reinforced gendered inequities in access to employment, and, overall, helped to maintain traditional gender roles” (Bondi and Rose, 2003, p. 231). The “time-space compression” in cities, that is, the destruction of spatial boundaries and barriers (see Harvey, 1990), has been experienced unevenly by its inhabitants. Access to transportation, jobs, and services was limited for women from the city’s inception. Garmanikow (1978) has argued that the city’s built environment, spanning out into suburbs, was constructed around the idealized nuclear family, which depends on limiting women’s mobility. Indeed, feminist academics “view the ‘man-made’ environment as just that: the material manifestation of a patriarchal society” (Spain, 2014, p. 585). Similar to the associations made between femininity, masculinity, and research, Schwartz (1976, pp. 334–335) characterized the city as a “masculine province” where “serious work” takes place, contrasting it to the femininity of the “passive, intellectually void” suburb. Cities are further masculinized through their association with danger and the unknown (see McCarthy et al., 2004, p. 236). The danger and risk of city life is acknowledged and to some degree celebrated within ethnography, but there is little to no discussion of how these are distinct for men and women. When women do confront violence and harassment in the field, they are often blamed for not anticipating these risks (see Hanson & Richards, 2019).

Fear of crime has historically limited women’s movements and access to public spaces, with the “shadow of rape” looming over women’s interactions (Ferraro, 1996; Kilanski, 2015). Although men are more likely to be victims of violent crime, women are more afraid of crime, at least somewhat due to everyday violence that can saturate daily existence (Stanko, 1990; see also Madriz, 1997). Additionally, women are more likely to be victims of sexual assault and harassment (McLaughlin, Uggen, & Blackstone, 2017) and report rates of public incivility more often than men (Bastomski & Smith, 2017). These experiences exact a “gender-specific tax on women’s access to public places” (Bastomski & Smith, 2017, p. 73) and can have profound effects on mobility and lifestyle, ranging from avoiding certain leisure activities and employment opportunities to self-imposed “virtual curfews” (Kinsey, 1984) to “an assiduous state of vigilance” (Pain, 1997, p. 234). Fear is predicated on a discourse that not only genders vulnerability and risk, but also racializes it. Towns (2016, p. 123) points specifically to violence perpetuated by the police against black women in urban spaces as a mechanism of social control:

The murder of Black women by police – disproportionately while driving – speaks to a long-held White mastery over mobility maintained by controlling the movements of people of color of all genders, sexualities, and classes.

And as De Welde (2003) notes, white middle-class women have access to privileged “safety zones,” making it less likely that they will be victimized, but also

have a disproportionate fear of crime due to histories of entitlement, the places they live, and myths about black criminals that remain potent.

None of this is to suggest that women are, in fact, more vulnerable than men in the city. As Elisabeth Wilson (1991) has observed, some feminist scholarship has perpetuated anti-urbanism, describing cities as spaces of disadvantage and oppression for women when they actually can allow them to break free of normative expectations. Nevertheless, I am interested in taking the gendered dynamics of crime, danger, and risk into account for at least two reasons: (1) to consider how social control in urban spaces differentially affects researchers, which can hopefully allow us to improve training and risk assessments before fieldwork; and (2) to analyze how academic capital accrued by “confronting” danger is distributed differentially among ethnographers on the basis of gender.

We must be careful not to equate women with vulnerability. Doing so reproduces the urban myth that women are more “at risk” in the city and the sociological fixation on men’s bodies as neutral in the knowledge production process. Martin Sanchez-Jankowski’s (1991) writing on the masculine “tests” he was given while in the field researching gangs in US urban society provide a nice counterpoint to this myth. In order to maintain his research relationships, Sanchez-Jankowski writes about proving his masculinity by engaging in physical violence. During one of these tests, to determine whether or not he was an informant, he was confronted and physically attacked; another test was designed “to see how good a fighter I was and to see if I had ‘heart’ (courage)” (p. 12).

In contrast, being a woman living and working in violent neighborhoods, I believe, was an advantage. For example, one day toward the end of my research while talking with Alexis, a National Police officer and professor at the police academy who was one of my key informants, I mentioned how relieved I was to have made it through my project without having been involved in some sort of violent encounter. Alexis replied that I had been lucky, but that my gender had most likely protected me. The neighborhoods where I worked, he said, were much more dangerous for men than for women. While only one form of violence, homicide rates show that men are more “at risk” than women, at least at being victims of lethal violence. Ninety-five percent of homicide victims in Venezuela are men, 69% of whom die between the ages of 15–29 (Sanjuan, 1999). Being viewed as a young, naïve woman made it unlikely that anyone would perceive me to be a threat. My gender, age, and appearance all made me seem nonthreatening, friendly, and approachable.

Maritza, a Latina researcher Patricia Richards and I interviewed during our research on sexual harassment of ethnographers, talked about this issue as well, describing her time conducting urban ethnography this way:

It forced me to think, okay I do feel disadvantaged when I get these catcalls but there are advantages. I thought about this when I was like, “Okay: What does it mean for me to be a woman in the streets? It means that I get catcalled but it also means I wasn’t recruited into a gang when I was young, or that someone didn’t jump on me or when I went to public schools [in the city] I wasn’t criminalized.

Women may experience other forms of violence and aggression, however. As Maritza mentions, even walking down the street, women may face microaggressions on a daily basis, in the form of catcalls and stares. These everyday interactions can inform the researcher's sense of self, impacting their ability to conduct research as well as their psychological well-being long after a research project is over. In my case because I was perceived by many men to be innocent and naïve, I was consistently "put in my place" by men who infantilized me. I was in my late 20s when I conducted most of my fieldwork, but was consistently taken for 21 or 22. In fact, my research decisions, like living in lower-class sectors of town, were sometimes chalked up to naiveté by police officers, despite the fact that I explained to them that I had spent years working in these sectors. Thus, my social position lent itself to a discourse of vulnerability that men used to harass me; the "risk" of inhabiting the city alone was used as a pretext to sexualize and dominate. However, my location as a graduate student in a "professional universe" (Bourdieu, 2002) that valorizes persistence and "trial by fire" ethnography made adapting or modifying my project unthinkable at the time.

To some degree, ethnographers are expected to stare down danger to prove their mettle, to adhere to expectations based on "the risk-taking, intrepid, white and male ethnographer star" (Huang, 2016). Danger is one of three "ethnographic fixations" that structures the evaluation of ethnographic research (Hanson & Richards, 2017), referenced by several scholars who participated in our study, who described "good" ethnographers as those who would do anything to "get the data." Many of the women we interviewed talked about putting themselves in situations that made them question their safety (during or after the fact), situations they said they avoid when not in the field. However, the academic capital we may accrue for facing danger is distributed differently, depending on the type of danger we are exposed to and gendered expectations around how to deal with risk. Writing about the respect researchers can receive for working in dangerous contexts, Theidon observes:

In numerous conversations I have found myself listening to male colleagues engage in one-upmanship on the "horror index". Just who has seen the goriest scenes, the most battered bodies, dodged the heaviest rain of bullets? (2014, p. 5)

Here Theidon, a woman researcher, speaks to the prestige that ethnographers can gain when they engage in macho bravado.

In contrast, the dangers that women ethnographers are more likely to confront – sexual violence, for example – are not idolized or rewarded. While masculine bravado and boasting of brushes with physical violence can shore up academic capital, the same does not apply to experiences with sexual harassment and assault. In fact, such experiences may discredit women ethnographers, in the same way that men ethnographers who transgress expectations of hegemonic masculinity by not "bravely confronting" violence may be delegitimized as researchers (see Hanson & Richards, 2019). If something had happened that night in the hotel with Miguel, I would worry about sharing the experience, worry that some might blame anything that happened on my "naivety," for not anticipating Miguel's actions. Indeed, even though Miguel left that night, I still

tell this story with trepidation for these same reasons. On the one hand, as ethnographers we are encouraged to immerse ourselves in our research sites, to “get our pants dirty,” to “persevere in [...] efforts to gain access and deepen [...] embeddedness” (Wacquant, 2015, p. 7). On the other hand, we are not taught to consider the different forms of violence we open ourselves up to as we persist and persevere. When this immersive process opens women up to harassment or assault, we may be blamed for not “thinking things through,” for ignoring safety concerns that as women we are expected to heed and as ethnographers we are expected to push aside.

There is an allure and excitement that one experiences when first starting an ethnographic project. There is so much potential, so many opportunities to explore, and anxiety can start to build as we worry about taking advantage of these, as we consider ways to explore all avenues, to absorb all that we can in the time that we have. When in the field, we may feel that we have to do anything we can to “get the data,” that we should say yes to every potential opportunity. However, doing so can lead researchers to ignore instincts that can keep them safe. Though it may leave us with a feeling of dissatisfaction or disappointment, researchers should err on the side of caution (see Baird, 2009), for our own safety but also to reduce the likelihood that we will use research experiences to build academic cred by exoticizing poverty, exploitation, and marginalization.

URBAN RESEARCH RELATIONSHIPS

In this section, I highlight the tension between opportunities and constraints organized by gender for men and women in the city, which shape the access and restrictions that ethnographers confront when conducting ethnography in urban spaces. Although women can perhaps move more freely in urban spaces in comparison to prior eras, and lines between public and private spaces may be blurred, women’s mobility, even their mere presence, is still censored by the politics of decency. As Maritza said during her interview:

It’s not the same for me to be a woman in the streets as it has been for men who have written most of urban ethnography. And the women who have done urban ethnography, they are pushed to study women, interview women, all of these because these other places are not very friendly to us.

Additionally, mobility depends on other aspects of a researcher’s body, including but not limited to race, class, sexual identity, and able bodiedness.

Certain spaces are marked as appropriate for men but not for women (and vice versa), and there can be consequences for transgressing the boundaries that separate them. Marina, another Latina participant in our study who was conducting research in a city on day laborers in the US, spoke about bars as gendered spaces and how this affected her access:

[My colleague] tells me these stories about going to the bars with [the workers] and hanging out with them all night and the type of information that he was able to gather just from these like interactions with them. And I’m like there’s no way that I can hang out with them at a

cantina, you know, and not be thought of as a prostitute. Because you don't see women hanging out with them in these spaces.

Historically, the public sphere has been synonymous with men's space; women found there have been considered of suspicious "moral fabric," and thus "deserving" of what happens to them when they cross over into it. Sometimes, women's presence anywhere in the city may be challenged. On more than one occasion, I was told by men that I should not have been "left alone" in the city, "abandoned" by my then-partner who was in the US. They then used this as a pretext to assure me that they would protect me. (Of course, my race and nationality also factored into this "pick-up" approach.) This does not mean that women cannot conduct research in these spaces. But their presence can have repercussions that men do not face (such as being labeled a prostitute, as Marina talks about). Women who enter these male spaces alone (or even accompanied) may be perceived in ways that can damage their reputations.

Undoubtedly, the level of exclusivity in "male spaces" also depends on other factors, like social class, race, ethnicity, immigration status, etc. And, issues of identity and access are ubiquitous in ethnography; they are not unique to women's fieldwork. As Forrest Stuart writes about in his ethnography of Skid Row (2016), his body also presented barriers to gaining access. Informal street vendors worried that his lighter skin could call the attention of the police because most residents and vendors were black with a darker skin tone than Stuart. Others assumed he was a cop due to his physique and tightly cropped haircut. Stuart was eventually able to find people who vouched for his identity and intentions, but this depended on him spending a significant period of time in the field. When undertaking research that violates the politics of decency or where our bodies cause disruption, it is important that researchers consider whether they have the time to cultivate relationships that can ease the tension or suspicion their presence can generate.

Although, the issue of identity and access is universal, the "costs" researchers incur for breaking societal norms and expectations simply by being in a place they "do not belong" are gendered. While women researchers may be granted access in male preserves, they may have to acquiesce to sexual advances and harassment. Kimberly Kay Hoang's extensive research in bars in Ho Chi Minh City in Vietnam, working as a consultant, translator, hostess, and bartender, is a case in point. In order to maintain access, Hoang writes about standing in line-ups while men chose women to invite to their tables, and remaining "silent when men touched me inappropriately" (2014, p. 520). This may be true for men as well who research spaces wherein women are encouraged to objectify men (see Montemurro, Bloom, & Madell, 2003) and for men and women who work in hypersexualized spaces. Men too will face challenges when attempting to spend time in women's spaces, but it is less likely that they will confront the threat of sexual violence when they do.

Of course, this is not always the case. For instance, the intersection of gender and sexual identity can illicit similar performances of power. Clarke a white trans man Patricia and I interviewed, was attacked by a group of gay men who

tried to remove his clothes at a bar where he worked, when he refused to answer their questions regarding his sex at birth. During his interview, Clarke, who described himself as being “read as very gender normative,” reflected that he could go out in the city where he worked and not have to worry about being “grabbed or groped” like many of his women colleagues. However, Clarke had to deal with transphobic harassment and assault. Clarke told us:

The trans issues are so peripheral to mainstream gay and lesbian concerns, and a lot of the worst transphobia that I experienced was from gay men [...] I mean, I think this particular story is not so much about just a sort of an episodic thing, but rather, the guy who really sort of outed me, who sort of planted the seed of “this is not a real man,” was the head of this major LGBT organization.

While cities are often written about as more welcoming to queer populations than rural areas (Abraham, 2009; Weston, 1995; but see Kazyak, 2011), trans researchers are subject to forms of social control that dominant groups in queer communities exert.

None of this is to suggest that women cannot conduct ethnographic research with men. In fact, women researchers may be better positioned to broach particular topics with men, thus gaining access to certain aspects of men’s social worlds that may be more closed off to other men. For example, Baird (2018, p. 354) notes that while conducting research with gang members in Medellín, macho banter “opened doors to ‘manly’ gang activities such as violence, guns, partying, fighting, sex with women, and the like.” But this also “set a particular tone, meaning that gang members were less likely to discuss the emotional burden of quotidian violence, fear or loss” (Baird, 2018). In contrast, while conducting interviews with gang members in Caracas, Veronica Zubillaga (2003) found that being a woman facilitated these kinds of conversations. Zubillaga writes:

I was thought to be a reporter, a psychologist, or a college student, and this made it easier to talk with ease. I had the good luck with the youngest (between 17 and 19 years old) to be assigned a maternal image, which opened a path for very intimate and painful confessions. This dynamic was undoubtedly marked by the weight that the image of the mother has in the life of these young men. (p. 322, my translation)

Conducting research in an urban setting as a young woman created certain resources for me as well. Distance as well as time constraints created by urban life provided much-needed barriers and breaks from research participants. This is because, as was the case with Miguel, I lived in the metropolitan center of Venezuela – Caracas, the country’s capital – and Miguel lived a few hours away. Largely due to cost of living in the city, he commuted there regularly for work, like many Venezuelans – there is a difference in population size of almost four million between what is considered the *Distrito Capital* and what is considered *Gran Caracas*. Cities like Caracas remain the “headquarters” of many countries. This means that even people who reside outside of the city may often travel to and through them. Men’s consistent flow through the urban center from surrounding peri-urban areas both created opportunities for me to see them and imposed limitations on the time we could spend together. One of the reasons why I was able to maintain my relationship with Miguel, despite his

persistent come-ons, was because of the geographic distance, an externally imposed limitation (i.e., one I did not have to enforce) on the amount of time we spent together. In other words, I did not have to turn him down as consistently as other men because he was in the city less often. We remained in contact via text, traveled out of the city together for work, and met when he was in town. But, our geographical separation operated as a cushion for me, allowing me to draw out our relationship for much longer than I would have been able to had he lived in the city.

With research participants who lived in the city, the urban advantage was distinct. Many of my research relationships ended after a few months, when men discovered that I would not date or sleep with them. Thus, there was an expiration date – one that I became quite good at estimating – on these relationships. However, like many working-class and poor men who live in urban centers, some of the men I knew worked multiple jobs, which also limited the amount of time we could spend together. For example, after my first meeting with Ramón, a police officer who I worked with during my entire dissertation research, we texted back and forth for weeks, making multiple plans to meet up. But Ramón always had to cancel last minute, either because he was called into work or because he had picked up a moving job. (When off duty, Ramón spent much of his time moving pieces of furniture or appliances to supplement his income.) His busy schedule ended up working to my advantage. Since we only spent time together at his work for the first few months, flirtation was kept to a minimum and I did not have to consistently turn down offers to go dancing or drinking as I did with other officers.

Conducting research in populous urban spaces can also mean coming into contact with new people on a regular basis. Over the two-year period, during which I conducted my dissertation research, I worked with many of the same community residents. However, the police officers assigned to these communities, as well as their supervisors, changed quite often. This meant constructing new relationships – and navigating many of the same pitfalls – every time a new officer was assigned to a neighborhood where I was conducting research, and attempting to maintain a relationship once an officer was transferred. But, it also offered new research opportunities when others fell through. And, although I worked with many of the same residents, I conducted research in four different communities in three different parishes (the smallest territorial unit used in Venezuela). Working in different communities that were at least a short bus ride away (and in two cases multiple subway stops and one bus ride from each other) meant that my access was not completely dependent on one or two gatekeepers, nor did my “standing” in one community make or break my research project. I also worked with police officers and members of citizen oversight committees of the police spread across different parishes. In contrast to researchers who work in rural areas, or even those working in urban areas but who are focused on a single community, this “diversification” of research sites reduced my anxiety about losing access in one place. Although this was not an intentional strategy at the beginning of my research project, I did consciously

continue to cultivate and maintain relationships across the city once I realized how tenuous my research relationships with men could be.

CONCLUSION

In this chapter, I have argued that accounting for the social location of a researcher in the urban setting – and writing this into our ethnographies – is as pertinent to evaluating ethnographic knowledge as paying attention to the details of the geographic location where we work. Considering how these two conceptualizations of location intersect to facilitate and constrain research can tell us not only about our experiences as ethnographers, but can also actually provide insight into important structural dynamics in the field. Indeed, my interest in social location has less to do with attention to researchers' biographies and individual experiences in the field and more with what Bourdieu refers to as "pre-reflexive" social and academic experiences (2003) and what feminist researchers have referred to as standpoint epistemology (Collins, 1986, 2000; Harding, 1993; Naples, 1998). Though these concepts are by no means synonymous, they call our attention to how individual knowers are the creations of long-standing relations of power.

As Patricia Richards and I have argued, accounting for the body in our fieldnotes and publications strengthens validity by honestly and clearly describing the tools researchers use to gather data, as well as how these tools can be used against them in ways that also produce data. In other words, it increases validity by providing critical details and information about our inquiry and discovery process. In our work, we have advocated for "embodied ethnography," which not only recognizes bodies as tools to get closer to the worlds of research participants, but also takes seriously the presupposition that all data and knowledge emerge from experiences, conversations, and interactions shaped by the bodies that engage in them (Hanson & Richards, 2019).

Of course, how we as ethnographers present ourselves, how we are interpreted, and how we gain access and build and maintain relationships change not only depending on the researcher but also on their fieldsite. Social and geographic location overlap and intersect to shape the data we collect and how we go about collecting it. How my race, nationality, and gender were read allowed me to gain access to certain spaces and people, but also complicated maintaining that access. As a young woman traversing the city on an almost daily basis, I was subject to various forms of social control that have developed in urban centers, but urban living also facilitated relationship-building in multiple communities, allowing me to "diversify" risk, and created certain beneficial barriers I would not have had working in a small, rural area.

Ethnographic fixations dictate that we leave our bodies out of our research design and writing, when in fact, they can clue us into power dynamics within urban spaces. In other words, in order to present ourselves as successful ethnographers, we are encouraged to ignore that which can provide important insights into our fieldsites. Because, our embodied practices are both individually experienced and structured by underlying social relations of inequality (see Sutton,

2010), researchers' embodied experiences can inform our understandings of how gender (and race, nationality, class, etc.) is structured in cities. Our positionalities within the places we study, can tell us a lot about where we study, a key insight of feminist methodology. Although, urban ethnography is founded on the importance of geographic location, researchers' social locations and what their bodies communicate about these have received much less attention; indeed, they have been actively ignored.

Furthermore, when we take for granted our ability to enter a site and impose our presence on our research participants, we are more likely to lose sight of the power dynamics that structure urban spaces. *Real* research in the way that Park and others conceived of it meant invading spaces that had been constructed by the subaltern, "an invasion of people of color's personal space as part of an exotic interlude from normativity" (Adjepong, 2017, p. 2). In this way, the Chicago School continued the anthropological tradition of invasive ethnography. In fact, Chicago School ethnographers studied the poverty and marginalization that the University of Chicago itself was a part of producing, studying these spaces as "natural" outcomes of urban life rather than the outcomes of urban planning, elite investment and construction. Victor Rios (2011) has critiqued this tradition in urban ethnography, referring to our obsession with danger as a "jungle-book trope," a "colonial fairy-tale narrative in the Western imagination of the 'Other'" that encourages researchers to tell stories about surviving "the wild" and living to tell the civilized world about it (p. 14). The feminist urban ethnography that Ray and Tillman (2018) call for would make it more difficult to ignore "how systems of binary gender or White supremacist structures oppress those they write about" (p. 5).

In contrast, we can turn to such work as Adjepong's "invading ethnography," which calls attention to the method's colonial history. By:

reflecting on moments of discomfort and disorientation [...] it is possible to disabuse their reader of any ideas that the ethnographer can walk through walls or fits easily into the spaces they study. In other words, making transparent those moments in which the ethnographer does not fit has the potential to levy a decolonial critique of the ethnographic method by challenging a 'zero point epistemology' that makes whiteness, maleness, and normative gender invisible. (2017, p. 17)

Paying attention to how our bodies do or do not fit into the places may also call our attention to androcentric, racist, and colonialist assumptions that we take for granted as we enter our fieldsites, contributing to more ethical and complex portraits of life in the city. These considerations can also better prepare researchers to navigate challenges and opportunities that they encounter in the field, which often differ depending on who is conducting the research. Despite the sexual harassment and sexualization I endured during my dissertation research, I continue to conduct research on policing in Venezuela. However, because I was forced to consider how my body could be both a liability and a resource in the field, I now approach this research differently and am better prepared to deal with the costs of these experiences. But, researchers should not be

expected to learn this on their own as they withstand “trial by fire” ethnographic fieldwork.

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