

Title: Ethnographic Toolkit: Strategic Positionality and Researchers' Visible and Invisible Tools in Field Research

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Abstract

For many, reflexivity is a core tenet in qualitative research. Often, scholars focus on how one or two of their socio-demographic traits compare to their participants and how it may influence field dynamics. Research that incorporates an intersectionality perspective, which brings attention to how people's *multiple* identities are entwined, also has a long history. Yet, researchers tend to pay less attention to how we strategically draw on our multiple social positions in the course of field work. Drawing on data I have collected over the past several years and extending recent sociological work that goes beyond a reflexive accounting of one or two of researchers' demographic characteristics, I argue that each researcher has their own *ethnographic toolkit* from which they strategically draw. It consists of researchers' visible (e.g., race/ethnicity) and invisible tools (e.g., social capital) and ties qualitative methodologies to research on how culture is strategically and inconsistently used.

Keywords: toolkit, methods, culture, reflexivity, positionality

Introduction

For many, reflexivity—thinking about the ways in which our social positions, or positionality, shapes our entrance, interactions, and conversations within the field—is a core tenet in qualitative research and represents one model of science (Burawoy 1998). Often, scholars focus on how one or two of their socio-demographic traits compare to their participants and influence field dynamics (e.g., Venkatesh 2006). Research incorporating an intersectionality perspective, which pays attention to how people’s *multiple* identities are entwined, also has a long history (Cooper 1892; Collins 2015). However, researchers tend to pay less attention to how we can actively draw on our social positions and capital in different ways, in the same study, to facilitate access to a variety of people and places (see Flores 2016 and Duck 2015 for exceptions). They also tend to pay less attention to our unsuccessful attempts (see Lichterman 2017) and how these positions and capital shape our data analysis (see Wherry 2011 for an exception).

Drawing on data collected over the past several years and Swidler’s (1986) “cultural toolkit,” I argue that researchers have their own *ethnographic toolkit* from which they draw. This toolkit consists of researchers’ social capital, backgrounds, among other characteristics, and shapes field access, field dynamics, and data analysis. In demonstrating that researchers each have their own ethnographic toolkit from which they strategically draw, this paper contributes to discussions on reflexivity by emphasizing how researchers’ social positions change across space, in interactions with different people, and across interactions with the same people. Additionally, rather than simply observing how different situations and people bring out different facets of our personality and identity, it pays attention to how researchers *strategically* use their positionalities and capital in successful and unsuccessful ways. It highlights how every researcher, not just

those who are marginalized, draws on tools in their assorted toolkit. Finally, it emphasizes how reflexivity can shape methodological choices and data analysis. The ethnographic toolkit primes researchers to think about how our social positions and forms of capital are strategically, yet inconsistently used—tying qualitative methodologies to how culture works more generally (DiMaggio 1997; Swidler 1986)—and how they guide how and why we navigate and understand the field in both “settled” and “unsettled” times.

In the following sections, I first situate the paper in the reflexive literature, and then describe the contents of the ethnographic toolkit. Next, I describe how two sets of tools in my ethnographic toolkit shaped field access, field dynamics, and data analysis in three research projects. More specifically, I highlight my Filipina heritage, U.S. citizenship, researcher status and social capital because they juxtapose characteristics that are visible (Filipina heritage, U.S. nationality) with those that are invisible (social capital, researcher status). Invisible tools offer an opportunity to contrast the purposes of when and why to reveal characteristics: for ethical reasons, I reveal my researcher status immediately, while I selectively use my social capital. I end by discussing how the ethnographic toolkit builds and extends contemporary accounts of reflexivity.

Reflexivity in Qualitative Research

The question of how our social positions shape access to participants, data, and field sites is a central concern of many qualitative researchers; however, being reflexive does not erase potential conflicts that occur in the field (Wasserfall 1993). Although some scholars analyze how non-researcher characteristics influence field dynamics (e.g., Brown-Saracino 2014), interrogating our own positions as researchers remains an essential task of qualitative research,

because reflexivity “affects both writing up the data...and the data’s status, standing and authority” (Brewer 2000:127).

One continuing debate regards researchers’ statuses as “insiders”—those who study populations who are similar to themselves—or “outsiders”—those who study populations who differ from themselves influence their work (Young Jr. 2008, Innes 2009, Davis 1997, Aguilar 1981). Although we all retain both characteristics because we have multiple statuses (Merton 1972) and can experience a handful of “insider moments” with participants (May 2014) and/or being the “outsider within” (Collins 1986, Zempi and Awan 2017), social science researchers have a long history of focusing on how one or two of their traits compare to their participants (e.g., Meyerhoff 1980, Duneier 1999).

For example, two contemporary classical ethnographies, Venkatesh’s (2006) *Off the Books* and Duneier’s (1999) *Sidewalk*, follow this model, and many scholars follow their lead in comparing one or two of their traits with participants and/or providing brief descriptions that nod to how researchers’ social positions may influence field dynamics (e.g., Goffman 2014; Rivera 2015). Venkatesh (2006) describes how his South Asian identity facilitated access to the South Side Chicago, Black community he calls Maquis Park because his identity “gave [him] an indeterminate and unthreatening presence” (xviii) and since he “was neither white nor black...[he] was not immediately identified with the police (white) or as a resident of the community (black)” (xvii). Yet, he does not address how his South Asian identity may have differently influenced his interactions. For example, while some may have viewed him as non-threatening, he may have been seen as suspicious by others precisely because he did not occupy a known role. Venkatesh also lacks explicit attention to how his male identity may have shaped his interactions with men and women differently, and how his entry into the community through

his ties with Black Kings gang members influenced his interactions. Although ties among and between gang members, police officers, and non-gang members formed the “social fabric” of the community (5), that does not mean that they all similarly trusted him. Being reflexive about his strategic positionalities and how he may have been differently received could provide insight into his data collection and analysis by showing variation in access to people and their lives.

Duneier (1999) focuses on reflexivity—his white, Jewish identity—as it relates to the primary participants of his books: the people who live and work on the sidewalk, and how identities can unknowingly influence interactions. He tape-recorded a conversation of two participants after he left the vending table, and it revealed their thoughts about his Jewish identity. Previously, he was unaware of how this identity influenced interactions, and he provides an important insight: we may never really know what our participants think about us. However, although many urban ethnographies are based on a group of people related to a specific place, our fieldwork is not limited to our primary participants. For example, Duneier also interacted with Amtrak and legal professionals, customers of the vending tables and others, all of whom occupy different social positions. He also compared how residents differently interacted with the white Romps family, who lived in a camper from which they sold Christmas trees, and the mainly Black men who lived and worked on the sidewalk he studied, providing a racial critique to Jacob’s (1992) “eyes on the street” model.

Yet, he is not reflexive on how, for example, his white, Jewish identity nor his gender and status as a faculty member at two universities shaped his interactions with any of his non-primary participants. Reflexivity over these interactions is important because his traits may have allowed him access to people and/or information¹ that he may not have had otherwise. Being reflexive over our primary participants is a common practice. However, by not being reflexive

about more fleeting interactions, our *theoretical* understandings of the field, our data, and how we fit in remains underdeveloped because our data is not limited to primary participants.

Reflexivity Beyond One or Two Demographic Characteristics

Robertson (2002) calls for researchers to move away from comparing one or two of their demographic characteristics with participants, because this type of reflexivity can “become a form of self-stereotyping... By writing “as a [name the category],” ...[t]he implication is that people everywhere, regardless of every possible distinguishing variable, are susceptible to the whims of...ethnographers” (Robertson 2002:789-790). Instead, she urges researchers to discuss how assumptions about how our identities will operate in the field can hinder data collection and analysis and think about how identities and social positions are multiple, shifting, should be interrogated throughout fieldwork and woven into our writings.

In a similar vein, reflexivity over intersectionality, or how people’s *multiple* identities are entwined (e.g., Collins 2015), has a long history (e.g., Cooper 1892). Many scholars follow in this intellectual tradition of focusing on how researchers’ positionality and multiple identities shape field research (Young Jr 2004; Lacy 2007; Chege 2015; Flores 2016; Pattillo-McCoy 1999), and how reflexivity and “insider” and “outsider” statuses are relational and situational (Bolak 1996; Doucet 2008; Woodward 2008). Hoang (2015) discusses how even the ways in which her physical body, clothes, and adornment changed depending on her field site.

For example, Contreras (2013) grapples with his multiple identities and “triple representational dilemmas” (28) he faced as a scholar of color writing about violence and minority men in a world that is full of harmful stereotypes about them and discusses how his positionalities shaped his entrance and encounters in the field, and the theoretical perspectives on

which he drew. Similarly, Rios (2011), while acknowledging he grew up in and around the neighborhoods he studied, discussed how his social position as a graduate student afforded him privileges that marked him as different than the participants he studied. So, too, does Duck (2015) describe how he strategically revealed personal information to some participants over others, how his entry into the field shaped his access, and his purposeful decisions regarding, for example, where to live and how to interact with people so as to keep open lines of communication with different types of people within the community, while Wherry (2011) describes how his social capital, educational credentials, (perceived) race/ethnicity, language, and sexuality shaped his foray into his field site, interactions across people and organizations, data analysis and revisit.

Although scholars of color commonly include these kinds of concrete, reflexive thinking about the multiple ways their identities shape field dynamics, it is not limited to them. For example, Desmond (2016) recognizes how his whiteness privileged him both in terms of police interactions and by Black residents themselves and notes, “[e]verything about you – your race and gender, where and how you were raised, your temperament and disposition – can influence whom you meet, what is confided to you, what you are shown, and how you interpret what you see. My identity opened some doors and closed others” (Desmond 2016: 325-326).

I follow in the footsteps of these scholars who recognize researchers’ and participants’ multiple identities, and how we behave differently depending on our participants (Bolak 1996). Drawing on and extending their work, I shift attention to how researchers *actively* and *strategically* draw on their characteristics and resources, which I divide into two types: visible traits, such as our gender and race/ethnicity, and invisible traits, such as our social capital, or the resources available to us based on social networks (Portes 2010, chapter 3). In doing so, I use the

toolkit metaphor to provide the conceptual mechanisms that guide how and why we navigate and understand the field.

Ethnographic Toolkit

In 1986, Swidler described culture as a toolkit that consists of the “symbols, stories, rituals and world-views, which people use in varying configurations to solve different kinds of problems” (273), and from which people devise their strategies of action,” or the “persistent ways of ordering action through time,” (273). In providing a metaphor to explain culture as a causal, independent force that shapes social life, Swidler (1986) shifts focus to how people make sense of the “diverse, often conflicting symbols, rituals, stories, and guides to action” (277) they are faced with on a daily basis. She also accounts for social change, in that in periods of “settled” times, people depend on the familiar to make sense of their lives, while in “unsettled times,” people are explicitly and consciously articulating new ways of being and understanding the world. Scholars have extended this idea of culture as a toolkit to other arenas of social life. Lacy (2007), for example, shows how, why, and when middle-class Black Americans draw on their Black middle-class tool kit to “strategically” assimilate.

I use the term *ethnographic toolkit* to show how the toolkit metaphor can help scholars understand and use qualitative methodologies. First, it recognizes the wide array of tools ethnographers use in fieldwork. Similar to Swidler’s (1986) understanding of cultural tools as “symbols, rituals, and world-views” (273) and what Sánchez-Jankowski (2002) calls the “researcher’s library of cues that help him or her determine what is going on in a particular social environment or interaction” (146), the tools in the ethnographic toolkit include researchers’ own social capital, identities, and backgrounds, among other characteristics.² These are tools in the

sense that as qualitative scholars, our bodies, racial/ethnic identities, gender, sexuality, appearance, backgrounds, education, citizenship, and social networks, among others, all matter and are used to gain access and understand the field. The ethnographic toolkit highlights how these traits shape the ways we view the world and the ways in which the world views us. While some of these traits are relatively visible—the color of our skin or our presenting gender—others are relatively invisible, such as our social capital. In this way, it builds on Flores’ (2016) work on temporarily-given hidden privileges, or characteristics such as occupation and education that are verbally revealed and not visible, and how she emphasized her academic credentials depending on her audience. It extends her work by placing researchers’ characteristics, not just those who are racial/ethnic minorities, as tools in an assorted toolkit that all qualitative scholars draw on, though the specific tools available are dependent on individual researchers. It also connects researcher traits and their strategic use to scholarship on culture as dynamic and strategically, though inconsistently, used (e.g., DiMaggio 1997).

Second, the ethnographic toolkit highlights how fieldwork, analysis and writing are not passive. Instead, they are shaped by the strategic and conscious decisions we make (Duck 2015; Flores 2016). That is, it emphasizes how researchers strategically draw on their visible and invisible tools during fieldwork precisely because it is an “unsettled time,” one in which scholars are constantly and consciously attempting to understand their participants, their behavior, their understanding of social life, and how these intersect with broader social structures. As such, it answers Lichterman’s (2017) call for “interpretive reflexivity” by emphasizing reflexivity in all stages of research and provides theoretical and methodological language to understand how researchers experience fieldwork as a settled or unsettled time may have an effect on their data collection, analysis, and the theoretical and substantial claims they make.

For example, when researchers fail to reflect on how their own social positions shape their research, it may be because they have entered into a “settled time” in field research by uncritically claiming to understand participants’ “habits, skills, and styles” (Swidler 1986) and becoming complacent in crafting their stories and linking them to broader social structures. For instance, Rios (2011) describes Venkatesh’s 2008 book as a stereotypical “jungle-book trope” which is rooted in a “very familiar colonial fairy-tale narrative in the Western imagination of the “Other” [which] goes something like this: ‘I got lost in the world, the wild people took me in and helped me, made me their king, and I lived to tell civilization about it!’” (14) because Venkatesh romanticized what it means to witness violence and be a part of a gang and did not highlight people’s complexities nor their daily lives. More recently, Rios (2015), among others, critiqued Goffman’s (2014) work for comparable reasons. Contreras (2013) calls this approach ‘cowboy ethnography’ which refers to ‘researchers who are thought to glorify themselves at the expense of the study participants . . . They project themselves as bravely risking life and limb, as tight-ropeing dangerous race and class lines – and making it back to tell the tale’ (pp. 26–27) (see also Hoang 2015).

By centering qualitative work on our methodological choices, and how these relate to reflexivity, our multiple positionalities, and researchers’ active and strategic use of the tools at their disposal, the ethnographic toolkit builds on previous scholars who write about the importance of interrogating our social positions in fieldwork and writing. It takes seriously the call to move toward what Rios, Carney and Kelekay (2017) call a “sociological double-consciousness” and be “rooted in a deeply reflexive methodological practice that guides researchers in their quest to understand power, inequality, and justice” (494), and extends this call to all qualitative research, not just the subject of their piece—criminality and crime—while

also providing methodological tools on how to think about and practice reflexivity on-the-ground and in writing.

Third, it provides a common set of tools and language to understanding field dynamics (such as changes in demeanor, explanation, or access to particular topics) within the same group of people, precisely because researchers may share certain commonalities with participants, but not others, and these commonalities and ways to build rapport depend on people and circumstances. As such, and similar to Rios' (2011) discussion of not being a "true" insider and Flores' (2016) discussion of being an insider with Latinas but an outsider with non-Latinos at the same field site, the ethnographic toolkit highlights how there are never true "insiders" precisely because we have *multiple* characteristics we draw on and we do not share all of our participants' characteristics.

Fourth, some scholars already note how they strategically reveal what I call visible and invisible traits (see Flores 2016), and the ethnographic toolkit provides theoretical and methodological language that helps researchers understand how and why reflexive accounts should consider both types of traits, how, why, and when researchers reveal them, and when our attempts to open doors and collect data succeed and fail (see Lichterman 2017). Fifth, it moves beyond researcher narratives explaining their experience in the field and how that relates to their identities to understanding the mechanisms underlying under which circumstances researchers use certain tools over others.

Sixth, it allows us to understand ethnographic comparisons more generally, and what Burawoy (2003) calls focused revisits. For example, differences in revisits may be due to changes across time, space and social structures, but may also be because each researcher enters and navigates the field with different sets of tools, develops relationships with participants, and

“fits” in the field in ways that cannot be replicated.³ Finally, analyses involving the ethnographic toolkit allow us to go beyond what Small (2015) describes as two problematic portrayals of researchers in their work which represent “a kind of rhetorical exploitation” (353): sympathetic observer and courageous immersive, by presenting a methodological language for researchers to thoughtfully discuss their positionalities across time, space, people, and interactions.

In my fieldwork, and despite my familial connections to the place, I was in a constant “unsettled” time in part because my focus was on a place that is by definition transient—Subic Bay Freeport Zone, Philippines. Instead of the traditional approach to focus on a group of people (e.g., Anderson 1990, Liebow 2003 [1967]), I focused on how different types of people navigate a space that is transient and semi-autonomous – what elsewhere is called a global borderland (author citation). As such, I was continually moving between different groups of people and observing different spaces and places that people occupied within the Zone. This methodological choice, along with my multiracial background and ties to the U.S. and the Philippines, primed me to continually think about my multiple statuses and how to strategically gain access and build rapport with multiple types of people, both those whom I would be “studying up” and those whom I would be “studying down.”

In the following sections, I discuss my background to situate my research, then demonstrate the toolkit’s usefulness by highlighting how I strategically used my invisible and visible tools to shape my research plans, gain access to people and places, how my ability to use these tools changed over time, context, and reception, and how they shaped my data analysis.

Background

Our backgrounds often shape the cases and theories to which we gravitate (Contreras 2013). Yet, if this true, it is not obvious *which* part of our backgrounds do so. For example, my family background could have led to research interests and theories in abuse, crime, or upward mobility, among others. However, it is my grandmother's migration story that sparked my sociological imagination (Mills 2000 [1959]) and shaped my research.

She grew up in the Philippines and in 1967, at 19 years old, she migrated to the U.S. through marriage to a U.S. naval seaman. After more than a decade of marriage they divorced, and I was raised by my grandmother because my mother had me in 1983, when she was fifteen-years-old. My grandmother raised myself and her five children while working three jobs and was ostracized from Filipino American communities because of the stigma of sex work accompanying marriage migrants. When I was 20 years old, I interviewed her for a class project, and in our conversation, described her experience growing up near the former U.S. Subic Bay Naval Base, Philippines, her delight in seeing the ships, and how having an American boyfriend allowed her access inside.

Her story spurred me to question whether her positive descriptions of the former base was because I am her American-born granddaughter, or whether it reflected broader experiences of women who had relationships with U.S. servicemembers. Over the past ten years, I've conducted research that ties back to this puzzle of how and why people understand the U.S. military overseas and its servicemen, including studies of Filipina marriage migrants and Amerasian mothers—women whose children have U.S. servicemembers as fathers, and in my reflections of past research, I do not rely on memory. Instead, I rely on my writings: drafts and completed versions of papers and field notes

For graduate fieldwork, I returned to Subic Bay, Philippines—home of the former U.S. Subic Bay Naval Base and current site of the Subic Bay Freeport Zone (SBFZ) to understand how foreignness and foreigners are received in communities. I conducted interviews and observations with foreigners, SBFZ workers and SBFZ visitors, and archival research from U.S. and Philippine sources. Furthermore, I gravitated toward a relational economic sociology approach (Zelizer 2005) and theories on boundary-making (Lamont and Molnar 2002) that emphasize how people “match” relationships with media and transactions and draw boundaries among groups of people that may be either different or similar to themselves. These theories rang as particularly useful because they explained how and why seemingly similar groups of people could think about people and places so differently. Understanding my personal background as tools in my ethnographic toolkit shifts attention to how these factors did not *passively* shape my research topic, methods, or theories but rather actively shaped the choices I made as I purposefully pursued research related to my grandmother’s migration and avoided certain topics such as crime.

Visible Tools

Two visible tools in my ethnographic toolkit include my race/ethnicity and my U.S. nationality, though when and how these characteristics are visible depends on with whom I am interacting and whether they have the knowledge to interpret these cues. For example, my Filipina heritage is obvious to other Filipinos, as is my last name. However, to others, particularly non-Filipino/as in the U.S., I am often racially/ethnically miscategorized as Latina.

How did my visible tools shape my research? First, they opened doors to my participants, which may not have been opened otherwise. For example, when I approached marriage migrants

whom were middle-class, and Amerasian mothers in the Philippines who were working poor, they agreed to be interviewed after seeing a recruitment flyer or through direct referrals through Filipino social networks. As such, they knew—or suspected—my shared Filipina heritage and upon meeting, almost always confirmed it.

Similarly, in the Philippines, Filipinos often recognize both my Filipina heritage vis-à-vis my skin color and my U.S. nationality through my demeanor, appearance, and accent. As with my previous work, my Filipina heritage may have provided an entry with Filipino workers and visitors to the SBFZ that could not have been as easily opened otherwise. However, it was only a small, and not the most important, connection, as I'll discuss later.

In interactions with different types of foreigners in Subic Bay, I used my American nationality, which is visible due to my height and mannerisms, to establish rapport. For example, I first approached Rebecca, a white Peace Corps Volunteer, at a coffee shop. As we chatted, she was excited to talk to another American. For example, she told me how I am “the only other white girl—you blend in here better than I do. You're clearly not full....You're Fil-Am [Filipino American]. Well, there are so many Fil-Ams here, you still blend. You're the only other ... American female under 50 who's here for any other reason than...religious perpetuation, whatever you want to call it. You're it.” For Rebecca, being American meant being white, and I—no matter my skin color—was white, someone similar to her in nationality and education. It was this visibility as an American that sparked our initial conversation and opened up doors.

Invisible Tools

If my visible tools initially may have helped open doors, it was my *invisible* tools that kept them open. In every interview, there is a power difference between participant and

researcher, because the researcher uses participants' stories to publish and build their career. To mitigate these differences and build rapport, I shared my grandmother basic story after the U.S. marriage migrants and the Amerasian mothers in the Philippines asked me who I was and why I wanted to speak with them—something that happened upon introduction. Whether I elaborated depended on the reception I received, further prodding by participants, and whether they shared something similar to my background.

It was only after I shared my family background as a means of using my social capital that women divulged their own stories. For example, in Seattle, when talking with four marriage migrants who were middle-class, I was asked about my grandmother's life. I disclosed how she felt alienated when she visited a Filipino American community center because of her military marriage. Soon after, one woman revealed that gossip about her third marriage, which was to an American who was almost 30 years older than she, was prevalent throughout the Filipino/a American community:

[When I first migrated] I have a lot of friends...but you gonna have to be careful in dealing with the questions of the people around. Cause, I have been misunderstood so many times, I was hurt so much, but I just ignore it until...it took me a long long time to remove that hurt from me...this gossip about my life, Americans or whatever, you know.

She felt comfortable addressing a presumably taboo topic in part because, she told me, I disclosed my grandmother's story, and she continually referenced it in her explanation.

The stories I shared included not only my grandmother's migration story, but in the case of Amerasian mothers, a family history of abuse, which I did not share with the marriage migrants. I selectively shared stories from my family depending on what the women said, their demeanor and reactions. Afterward, women elaborated on their stories, often referring to grandmother's experience throughout our conversations, and comparing her to their own.

In Subic, my Olongapo family ties, or social capital, and ability to speak Tagalog became critical. For example, Harbor Point mall workers would often initially speak English. I would reply in Tagalog to build rapport. Afterward, the majority of the workers switched to Tagalog since more than one person told me that it was easier to speak in Tagalog than in English and visibly changed their demeanor. Other times, however, this strategy did not work. For example, I spoke with Tomas, a wealthy Filipino SBFZ visitor, in Tagalog to build rapport, but switched to English at his behest. The tools I draw upon do not always “match” the ones in which my participants want to engage.

My social capital also helped me navigate Philippine bureaucracy. My *Tita*’s (aunt) assistance, for example, was critical in re-establishing ties that I inadvertently damaged with the Subic Bay Metropolitan Authority (SBMA), the SBFZ’s government. Upon my arrival, I scheduled a meeting with the Chairman to gain permission to conduct interviews with SBMA officials. Impressed with my educational credentials as a Princeton PhD candidate (another tool in my ethnographic toolkit), he wrote a letter extending his permission. I then showed the letter to SBMA officials as I explained my work and sought interviews. One of the places in which I gained access was the legal department. After being cleared to examine lease contracts, I set out what I thought I gained permission to do—scan the files so I could later analyze them more in-depth. Unfortunately, my understanding of what the Chairman gave me permission to do was not the same as legal department officials. Days after I started, my main contact asked me to delete all my scans. I didn’t understand the extent of harm I caused until my *Tita* took me to go see the mayor of the *barangay* (neighborhood) in which my family lived, so he could write a letter on my behalf. Afterward, she counseled me on how to frame my apologies to the SBMA Chairman and lawyers in the legal department. It was only through my family-based social capital that I

understood the degree in which I breeched expectations and that I was able to somewhat rectify my mistakes. Otherwise, I would have not known what I did wrong. Yet, while in this case I actively drew on my familial social capital, I did not always do so; for example, when I conducted fieldwork in nearby Clark or in non-governmental interview settings.

I also used my invisible tool of social capital to navigate local courts. I wanted to supplement my collection of Philippine Supreme Court and Court of Appeals cases with local court cases involving the SBFZ and former U.S. Subic Bay Naval Base. Because of my previous experience with the SBMA, I brought along my *Tita* to the Olongapo City courts. This court system has multiple Regional Trial Courts (RTC), each with its own jurisdiction, and I visited the RTCs multiple times. Each time I returned, I was told I needed another form signed by another official.

In one of these visits, my *Tita* told me that a security guard informed her that the reason I was encountering so much difficulty was because the workers were expecting a bribe. I suspect it is precisely because I am American that he did not share this information with me, which I would have not otherwise gotten without my invisible tools.⁴ I shared my reservations with her about paying a bribe, and she likened it to the meal incentives I offered interviewees. Still, I could not in good conscious bribe anyone for documents. However, I did feel comfortable giving a thank you gift, in the form of a meal, to the people who helped me, but only after I received their assistance. My *Tita* helped me navigate these dynamics, acting as a cultural translator and facilitator.

Yet, my *Tita* was not always helpful and my status as a researcher also was a barrier at times, even in the Court system. For example, I spoke with a judge at one of the RTC branches, and in my field notes I record my unsuccessful attempts at developing rapport:

[I] [i]ntroduced myself in Tagalog but [the judge] interrupted me and said to speak in English...Tita Linda came in behind me and when she sat down he asked who she was, I said ang tita ko [my aunt] and he asked if she also had business with him and she said no and would wait outside. He then explained that this was a family court and he had to protect the integrity of his Court. He couldn't give "every Tom, Dick and Harry" the files "just because they were doing a thesis." (Field note 9/18/2012)

I tried to use my Tagalog skills and my social capital to build rapport but was unsuccessful. Thus, my use of invisible tools does not always facilitate access. In the words of Zelizer (2005) and Mears (2015), these would be a "bad" match or a "mismatch" between myself, the ethnographic tools I draw on, and the access, relationship, and response of the recipient.

How did my invisible tools relate to access and field dynamics with non-Filipinos? During my fieldwork, a U.S. military ship docked in the SBFZ. After witnessing how the influx of mainly servicemen changed the SBFZ's demography and knowing the long history of the U.S. military in Subic Bay, I wanted to interview as many associated people as possible. However, I also knew that the most well-known social science research related to the U.S. military overseas is primarily negative, portraying servicemembers as villains (e.g., Enloe 2000 [1989]). I suspected that my role as an American researcher might be an obstacle.

My fears heightened after I revealed to Rebecca, the Peace Corps Volunteer, that I was a researcher examining Subic Bay. This is because she then told me of how she confronts sex tourists and servicemembers about their presumed sexual activity because of their exploitation of Filipinas. For example, she described how she and a group of her foreign friends visited Puerto Galera. There, they played a "game" where they would walk up to people who they assumed were sex tourists to talk about sexually transmitted infections, providing them with statistics that would presumably make them feel uncomfortable for being there for sexual gratification. She does something similar when she sits next to a foreigner on a jeepney:

...and then what I do to white men who make the mistake of sitting down next to me. I tell them about my work, I focus on the human trafficking aspects and STD, HIV, and AIDS.... So I talked to them about that, and they're stuck next to me in the jeepney out to the Barrettos, and I always ask what they're doing before I launch into this, and I play the friendly card. Once I found out that they are tourists. 'Oh, why did you pick this area?' 'Oh, I've been here before and I think the people are nice.' That's the give-a-way. If they have kids with them, I won't do it... I do gauge it. With the military personnel, I launch into full statistics...there's this many of gonorrhea in the past month ... and I embarrass the hell out of them...[but I] never approach anyone who isn't a captive. In a jeepney, I sit next to them and they're my captive

Rebecca confronted U.S. servicemembers because the U.S. military created and helps maintain the Philippine sex industry, and she felt comfortable sharing this information with me after I revealed my status as a researcher, and because academics studying the U.S. military overseas often come from a critical perspective. Yet, no matter how correct her assumptions, I hoped the servicemembers to whom I passed out my recruitment flyers would not think I was trying to do what Rebecca did—be friendly before going on an attack.

Yet, as I feared, many of the servicemembers refused my flyers and were not interested in talking, despite my American accent and nationality. For those who would listen to me explain my project, I revealed my marriage to a Department of Defense (DoD) analyst to purposefully avoid being perceived as someone as anti-U.S. military. After my disclosure, many of them looked visibly relieved or more relaxed, and were more inclined to set up an interview. My goal is not to villainize U.S. servicemembers, rather, I strive to understand their points of view and juxtapose them with structural conditions. After hearing about my DoD connection, Sophia, a U.S. servicemember, talked with her superiors. After she confirmed that my PhD supervisor was who I said he was, she agreed to an interview. My researcher status and social capital—this time, my marriage to a DoD employee—facilitated access that may not have been granted otherwise. Yet, in other interviews, I did not share my invisible tool of marriage to a U.S. government employee.

In contrast to U.S. military personnel who were initially wary of my role as an American researcher, my researcher status facilitated access to other foreigners in the area. For example, Kevin, a middle-aged American male who is in the Philippines “for the beautiful women” agreed to speak with me because, according to him, I did not want anything from him, I just wanted to listen. This is in contrast to what he encounters on a daily basis “... people in lower class. They look at you as an ATM—the only reason they interact with you is to extract money from you. Not in every case, but in many cases.”

Speaking English also played a role in accessing missionaries of an evangelical missionary ship docked in the SBFZ and SBFZ business owners who were from English-speaking countries. Interviews, access, and the ability to use tools in my ethnographic toolkit are relational; they depend on the interviewees themselves and their own reasons for being interviewed. For example, each missionary I spoke with inquired about my religious beliefs and invited me to their services, while at least one of the Amerasian mothers asked for my assistance in finding the child’s father. She believed he belonged in the U.S. because of his American heritage and that I could help her son. She hoped that he would not face the discrimination in the U.S. that he faced in Angeles. Not only are my decisions to interview strategic, but participants also made particular requests or discussed particular events, based on their knowledge of my social position and likely because of their own strategic reasoning.

Analyzing Data

I also drew on my ethnographic toolkit to analyze the data (see Wherry 2011). For example, most scholars and activists view relationships between U.S. servicemen and Filipinas as inherently exploitative (e.g., Enloe 2000 [1989]). However, informed by my family

background and the ways in which my grandmother migrated to the U.S. and how she spoke about the base with nostalgia, I paid particular attention to the ways in which my participants discussed relationships with U.S. servicemen, and specific arguments made in government documents, like embassy notes.

By not presuming negative sentiments to the U.S. military, I found different patterns than other scholars did. For example, the Philippine non-profit organization, Women's Education, Development, Productivity and Research Organization conducted a survey in 1991 of 300 Filipina sex workers and used the results to critique the broader structural conditions brought on by the U.S. military. They dismiss some of their findings—where 26% of their participants “have gone into wishful thinking, fantasizing of marrying an American and having American kids” (Miralao, Carlos, and Santos 1990:47)—as ill-fated fantasies, because to them, the U.S. military necessarily exploits Filipinas.

What I find in these documents and my interviews are *additional* patterns—that Filipina women drew on the bourgeois and prosaic-realism myths described by Swidler (2001) to describe their relationships with U.S. servicemen, and what I call a heroic love myth where these relationships are interrelated to dreams of a better life (author citation). I also find that these women set distinct moral boundaries (Lamont and Molnar 2002) between themselves and others who are similarly involved with U.S. servicemen. The Amerasian mothers in the Philippines differentiated their own relationships from sex work, for example, by emphasizing whether and why their child's birth certificate named the father, while Filipina marriage migrants to the U.S. delineated their relationships from sex workers or others whom they saw as using marriages to migrate, rather than as acts of love.

Additionally, I show that while for many Subic Bay continues to be a sign of U.S. power and exploitation, for others, it signals a chance at “the good life.” These stories are important to understand why and how places like special economic zones and overseas military bases continue to exist: not only because of foreign or elite domestic power and influence, but also because they represent a chance for everyday people to partake in a global community. The tools in my ethnographic toolkit primed me to think about, and access, the multiple people, viewpoints, and meanings involved, which may have gone unrealized by other scholars.

Discussion and Conclusion

I’ve focused on the visible and invisible tools in my ethnographic toolkit and how they shaped my field access, field dynamics and data analysis. How did they come together? The importance of my visible tools come into play *relationally*, depending on whom I am speaking with, the context in which we meet, whether it is a shared trait (Flores 2016; Duck 2015), participants’ ability to interpret these visible characteristics, and my interpretation of interactions. While in the U.S., speaking to the Filipina marriage migrants, my citizenship was assumed and taken for granted since we were all U.S. citizens within the U.S. In contrast, in the Philippines, my nationality is a visible trait that marks me as foreign, as does my Filipina heritage, because it is obvious that I am both foreign and not *fully* Filipina. This facilitated access in some circumstances, while constraining access in others. This visible mark as a foreigner also allowed me to connect with other foreigners in the Philippines, because our foreignness was a commonality that marked us as different from the majority and became a means to build rapport. Yet, it also constrained access to other foreigners, such as U.S. military service personnel who

did not want to speak with me. That is, my visible tools both opened and closed doors (see also Desmond 2016).

My invisible tools were not only what Flores (2016) describes as “hidden privileges.” They were also key mechanisms in which to build rapport that *began* with shared traits. For example, although my Filipina heritage may have opened doors to my participants, it was a necessary but insufficient condition. It was my strategic use of invisible traits that *kept* the door open, as evidence by continual reference in our conversations to the traits I shared with them. In the case of the Filipina marriage migrants and Amerasian mothers, this took the form of personal disclosures. For others, it was my Tagalog skills, social capital, researcher status and/or academic credentials. In the case of U.S. servicemembers, it was my social capital in the form of marriage to a DoD analyst that opened the door when my nationality did not.

While much of reflexive work involves our visible traits and how it may shape our fieldwork, the ethnographic toolkit shows the importance of both visible and invisible traits. It shows how both can be strategically used in the field, in different ways, depending on our interactions, and provides a common set of language and tools to understand these methodological choices. Often, it is our invisible connections to our participants that deepens connections that may have first arisen out of visible traits. However, how we use our tools and our intentions do not always “match” what our participants expect or want from us.

What are the implications for qualitative researchers? We often begin our research asking questions, such as “What site should I choose? Whom should I approach and how? How do I present myself? Do I need to be an insider?” The conceptual ethnographic toolkit allows researchers to see how no one is completely an “insider,” because “being there” is premised on *researching* the community, and research will never share all of their invisible tools with our

participants. Furthermore, it allows our understanding of what is an insider/outsider to be more flexible and encourages researchers to think strategically about ways to develop rapport, even if the communities we study seem to be so different from ourselves, and, like Small (2015) suggests, empathize with our participants. Doing so allows us to see visible and invisible similarities with people across statuses.

Yet our socio-cultural capital and identities are not the only tools in our ethnographic toolkit. It also includes the theoretical traditions with which we enter the field—what Timmermans and Tavory (2012) call abductive analysis—and how that influences our understanding of data. For example, whether we are trained in particular “schools” of thought, such as the Chicago School of ethnography (Deegan 2001), matters. It shapes what we pay attention to and how we think. As someone trained at Princeton, I am grounded in a particular way of doing cultural, political, and spatial analyses. The ethnographic toolkit also allows us to see how our analysis and theoretical frameworks do not emerge in a vacuum but are tools we acquire in our training and backgrounds.

The ethnographic toolkit also includes the varied methodological approaches and decisions we make regarding data collection and analysis. For example, whether we choose to conduct formal or informal interviews, become a participant observer or an observer participant, name places or people and/or share data (author citation), and use a tape or video recorder matter. Future scholars should use the ethnographic toolkit to examine how both our theoretical traditions and methodological choices are strategically used throughout research.

I recognize the limitations of my research; my pre-graduate school data is limited, and my social position includes distinctive traits. For example, scholars may not have a multiracial background, or share similar racial/ethnic backgrounds and/or have ties to the communities they

study. Additionally, most ethnographers focus on a particular set of people, rather than a transient place. Yet, the insight that *all* scholars have visible and invisible tools that they can use to gain rapport, access the field, and shape our analysis is more broadly applicable. It follows in the footsteps of scholars who have noted how certain characteristics give them particular insights and advantages and suggests that the active choices we make in the field are methodologically important. Future research should continue to document the wide range of tools we have at our disposal, how they may both facilitate and constrain access, and how our impressions of our toolkit compare to participants' impressions of us. Researchers should also systematically investigate "settled" and "unsettled" times in fieldwork.

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¹ Duneier also wrote a detailed methodological appendix regarding his “extended place method” as a way to link the micro, the macro, and processes across spaces and places, and the importance of “checking stuff.” I do not go more in-depth on these important methodological advances because my focus is on reflexivity

² This is complementary to what Steinmetz (2008) calls ethnographic capital—colonizers’ knowledge of colonial subjects. Whereas Steinmetz focused on how the colonial rulers used ethnographic knowledge of natives to assert symbolic capital among one another, the ethnographic toolkit describes researchers’ own tools and how they use these tools to understand, navigate, and analyze the field.

³ Although he rejects differences in positionality as the primary reason that explains the differences he saw in his factory revisit, Burawoy (2003) ultimately calls for a “reflexive ethnography [that] recognizes two dilemmas: “(1) There is a world outside ourselves (realist moment), but ethnographers only know it through their relation to it (constructivist moment); and (2) ethnographers are part of that world (internal moment), but only part of it (external moment)” (668). I agree with his call to incorporate all four moments in ethnographies. This paper focuses on the ethnographer themselves.

⁴ As a reviewer helpfully pointed out, I cannot disentangle my family-based social capital from cultural knowledge in this circumstance or the mistake with the SBMA; that is, was this knowledge contingent on family ties or could someone else have relayed this information to me? It’s likely that someone else could have relayed this information to me, though I suspect that it would have to be someone who I was close enough for whom helping me and sharing this information was something they wanted to do. In my case, cultural knowledge and family ties are interwoven