Three Models of Transparency in Ethnographic Research: Naming Places, Naming People, and Sharing Data

Abstract

Ethnographic research consists of multiple methodological approaches, including short- and/or long-term participant observation, interviews, photographs, videos, and group field work, to name a few. Yet, it is commonly practiced as a solitary endeavor and primary data is not often subject to scholarly scrutiny. In this paper, I suggest a model in which to understand the different ways in which ethnographies can be transparent—naming places, naming people, and sharing data—and the varied decisions ethnographers have made with regard to them: whether to name a region, city or specific neighborhood, name primary participants or public officials, and to share interview guides, transcripts, or different kinds of field notes. In doing so, this paper highlights how decisions regarding transparency are part of an ethnographer's methodological toolkit, and should be made on a case-by-case basis depending on the *who*, *what*, *where*, *when* and *why* of our research.

Key Words: Methodology, toolkit, social science, transparency, ethnographic models

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Ethnographic research sheds light on the “definition of the situation” (Thomas 1923) as seen from the everyday lives of individuals. It consists of multiple methodological approaches (e.g., Gans 1999), including short- and/or long-term participant observation, observations more generally, interviews, photographs, videos, and group field work, to name a few. As such, we can think of ethnography as a set of tools in a “methodological toolkit.” What unites these methods is the aforementioned focus on everyday life, people, meanings, and practices. What they allow us to do is answer research questions related to comparing “what people say” to “what people do” (Jerolmack and Khan 2014)ⁱ and collect rich sources of data which shed light on meanings and life’s complexity. Yet, ethnographic research is commonly practiced as a solitary endeavor and primary data is not often subject to scholarly scrutiny, despite the very real consequences it has for our research participants, the broader communities in which they live, and policy recommendations that derive from our scholarship. It also has ramifications for our discipline when there are controversies around the validity and reliability of data, and the extent to which we should participate in the lives we study.ⁱⁱ

Can ethnographic data ever be valid or reliable? According to a positivist approach, yes. If we see ethnography as a social scientific method, then our findings represent observed phenomena based on data that is verifiable and replicable. In contrast, an interpretive approach focuses on meanings and sees “reality” and “data” as socially constructed. While these two approaches are often pitted against one another, they do not have to be—we draw our conclusions from data about meaning-making activities and people’s discourse and behavior. Meanings and interpretation can change over time, across space, and depending on the relationship among actors, yet what we know about meanings is rooted in the information we gather through our fieldwork and interviews. If one goal of ethnographic research is to contribute

to more generalized knowledge, then ethnographers need to be concerned with understanding how and why our methods are scientific, while taking into account the interpretive foundation of our work. One way to do this is through transparency in our data and methods. By transparency I refer to being precise about what we are counting as data or information and how and over what period of time we collected them. If research relies on data (e.g., observations, interviews) for the accumulation of knowledge, then making ethnographic data more transparent should be of scholarly concern. Jerolmack and Murphy (2017) and Murphy and Jerolmack (2015) have suggested that to increase transparency, scholars should name people and places in ethnographic work.

Transparency was also at the center of a recent debate in *Ethnography* vis-à-vis the question of whether “ethnographic data [should] be stored and made accessible for verification purposes?” (Pool 2017: 282), and the implications this has for what we mean by “ethnographic data” and “verification.” Despite different answers to this question—whether promoting guidelines on record keeping and presenting more data in publications (Pool 2017), storing data in a data architecture like ©ArchEthno (Weber 2017), or more thorough reflexivity (or “self-interrogation”) (Port 2017)—we see that all three authors share a common concern about transparency and accountability. As such, I think focusing on whether data should be stored (and where) and made accessible for verification (and to whom) are the wrong questions on which to focus. Instead, we should focus on how to make ethnographic data transparent.

In this paper, I suggest a model in which to understand the different ways in which ethnographies can be transparent—naming places, naming people, and sharing data—and the varied decisions ethnographers have made with regard to them: whether to name a region, city or specific neighborhood, name primary participants or public officials, and to share interview

guides, transcripts, or different kinds of field notes. In doing so, this paper makes several contributions. Theoretically, it switches attention on transparency from a “one size fits most” approach to highlighting how these decisions are part of an ethnographer’s methodological toolkit, and should be made on a case-by-case basis depending on the *who*, *what*, *where*, *when* and *why* of our research. Empirically, it synthesizes these varied approaches, and highlights their advantages and disadvantages. Finally, it contributes to ongoing debates regarding who ethnographers should be accountable to—our participants, other scholars, and/or ourselves.

NAMING PLACES

Although anonymizing places and people is a common default practice for qualitative research, many scholars focus on naming places in order to root their findings in a particular historical time and place. This type of ethnographic transparency has a long history in the social sciences (e.g., DuBois 1996 [1899]; Zorbaugh 1983 [1929]).

There are three trends to naming places, (see Figure 1). First, some scholars name the region in which the research takes place (see Jerolmack and Murphy 2017 for a critique of masking places). This is a common way for ethnographers and other qualitative scholars to contextualize the places they study while also maintaining interviewees’ anonymity (Lareau 2003; Winddance Twine 1998; Dreby 2010; Stack 1974). For example, Elijah Anderson, in his 1992 book *Streetwise*, uses the pseudonym of “Village-Northton” for a place that “encompasses two communities—one black and low income to very poor...the other racially mixed but becoming increasingly middle to upper income and white” (Anderson 1992: ix). He shows how socio-economic and cultural boundaries are created and maintained between the two communities through practices, discourse, and symbols and how these occur within the particular

socio-economic and political environment of Eastern City, where Village-Northton is located. His pseudonym suggests the city is located on the East Coast, and we can reasonably assume that he's studying a community in or around Philadelphia, given his employment at University of Pennsylvania, and his decision to study Village-Northton after he moved there. He arguably names the region, but not the city in this book. Although informed readers may guess that Village-Northton is somewhere in Philadelphia, and some may know the precise neighborhoods because of their knowledge on where Anderson lived, there is still *plausible deniability*. This plausible deniability allows for the people who are studied to disavow knowledge or participation in the research.

Broadly speaking, we know that not only are there are important differences between cities, suburbs, and rural areas, but that there are also important regional variations, for example, between cities located in the Great Lakes Midwest (Chicago, IL) and the South (Atlanta, GA) or on the East Coast (Philadelphia) and West Coast (Los Angeles) (e.g., Oakley 2015), and the advantage of naming the regions in which we locate our studies is that it contextualizes the findings vis-à-vis history and regional cultures. Knowing that Village-Northton, for example, is on the East Coast of the United States situates it within a particular history and context of deindustrialization, Black-White relations, and increase in immigrant populations. Highlighting these regional characteristics is an essential component of situating place-based research in a historical and cultural environment, while also maintaining broad anonymity and protection of participants. Yet, when we mask a place's identity, we lose important contextual information since places have their own identities and characteristics that influence social life (Gieryn 2000), and it can give way to what Jerolmack and Murphy (2017) call “pseudo-generalizability”—which “gloss the ‘messiness’ of particular cases by reducing them to ideal typical social

arrangements” (11-12) and “may also imply that the case is representative of other unobserved cases” (12).

Figure 1: Three Models of Ethnographic Transparency

Other researchers do name the city or a certain subsection of a city that they study, for similar reasons that scholars name regions—to situate the research in a particular historical context, while maintaining anonymity of individual interviewees. In contrast to naming regions, specifying a particular city allows for more accuracy and detail. For example, Liebow (1967) documents how the meanings and practices of low-income Black men interacted with the structural conditions in which they lived and the relationships they created and maintained. The New Deal Carry-out store, and its front corner sidewalk, where Liebow met Tally and the other men in his study, was located in Washington, D.C. “within walking distance of the White House, the Smithsonian Institution and other major public buildings of the nation’s capital, if anyone cared to walk there, but no one ever does” (Liebow’s 1967:10). Although “Tally’s Corner” has recently been identified as the intersection of 11th and M streets NW in Shaw, this was revealed posthumously by his wife, not by Liebow.ⁱⁱⁱ There was “nothing distinctive about these men or

this corner” (Liebow 1967:8) and the structural conditions were similar to other parts of the country. Yet its location in the heart of Washington, DC was crucial to know in order to understand these men’s stories, because at the time it was the only major city where Blacks outnumbered Whites and it served as a hub of Black migration from the South (Leibow 1967:10 [footnote 10])—central aspects that shaped the context of their lives.

More contemporary scholars also follow this practice (e.g., Lacy 2007, Newman 1999). For instance, Mary Pattillo-McCoy (1999) examines how the spatial and contextual traits of the Black middle-class neighborhood of “Groveland” shapes the relationships, lives, and mobility of the people who live there. Groveland is located in the South Side—or “Black Belt”—of Chicago. This allowed her to not only situate her study in the historical context of Chicago as a major port for Blacks migrating from the South to the North during the Great Migration, but also the rich and long-standing literature on Chicago, stemming from the Chicago School of sociology, among others. Furthermore, she not only uses Chicago-based research to situate Groveland in its particular context, but also uses empirical and theoretical insights to compare her own findings, for example, on the use of gangsta and ghetto styles and symbols, with that of Drake and Cayton’s 1993 [1970] findings on zootsuite styles (see chapter 6 in Pattillo-McCoy 1999).

Rather than relying on potential readers’ knowledge of regional identities to contextualize research, scholars who name a particular city or subsection of a city, situate their work in a particular historical and socio-cultural context. It allows scholars to build on the theoretical and empirical work of others to add to our collective knowledge about particular places and practices.

Yet, we also know that there are crucial differences between neighborhoods or sections within the same city (e.g., Massey and Denton 1993), and some researchers address these

differences within cities by naming the specific building, street, neighborhood or community they study (Drake and Cayton 1993 [1970]; Zorbaugh 1983; Molotch 1972; Gans 1962). The underlying focus on the importance of place unites urbanists from different disciplines, so for many, naming the specific place or neighborhood they studied is a necessity.^{iv} A main advantage of naming specific places we study is that it allows for scholars to revisit places and compare findings across time (see also Jerolmack and Murphy 2017). For example, DuBois (1996 [1899]) systematically and empirically examines the socio-economic lives of Black men and women living in Philadelphia, traces their history to slavery and discrimination, and roots his analysis in the Seventh Ward. Precisely because DuBois does so, Marcus Anthony Hunter (2013) traces the history of the Seventh Ward since the publication of *The Philadelphia Negro*, more than one hundred years later, and interweaves DuBois' vivid descriptions with his own work. By focusing on critical junctions in the Seventh Ward's history, he highlights Black Philadelphians' political agency vis-à-vis framing, voting, mobilization and secondary (within-city) migration and their hand in influencing the city.

Contemporary ethnographers also follow this pattern (Young Jr 2004; Small 2004; Venkatesh 2002; Wherry 2011; Anderson 2011; Khan 2010; Reyes 2015). For example, Jane Jacobs (1992 [1961]) names Greenwich Village as the location in which she argues that neighborhood safety can be reproduced, in part, by maintaining “eyes upon the street” (35) — that is, when those invested in the neighborhood keep watch of people's comings and goings. As such, Duneier (1999) provides a powerful racial critique and follow up of Jacobs' “eyes on the street” model in his book *Sidewalk* by showing how, in the same area, informal Black book vendors and the Romps, a white family from Vermont, were differently received by local community members. For ten years, the Romps traveled to the West Village during the holiday

season to sell Christmas trees, and lived in a camper on the streets. In contrast to the informal book vendors of his study, he found that the Romps were welcomed by local businesses to use their public bathrooms, given keys for places to stay, allowed to use electricity from a local business, and community members delighted in being customers to their three-year-old son Henry and his side business selling branches. Duneier is able to contrast the Romps, the informal book vendors, and the people who populated Greenwich Village during Jacobs' work precisely because Jacobs named her research site. Similarly, Eric Klinenberg (2015 [2002]) argues that disparate mortality rates during a Chicago heat wave were the result of each community's "social infrastructure": the sidewalks, stores, public facilities and community organizations that bring people into contact with friends and neighbors" (xxiv) and names the adjacent neighborhoods in Chicago he studied: North Lawndale and South Lawndale. As such, Duneier (2006) was able to follow up on the data and argues that Klinenberg's work suffered from the "ecological fallacy" of using aggregated data to make claims about individuals. Klinenberg (2006) responded in the same issue of *American Sociological Review* and suggests that Duneier's critique is unfounded because it is a selective accounting of his work, Duneier made errors in his own reporting of Medical Examiner reports and death certificates for the people he highlighted in his critique, and that Duneier bases his claims on decades-old memories, often of people who were not with the deceased at the time of their deaths and who were publically blamed for said deaths by not checking in with their loved ones.^v

The practice of naming specific neighborhoods encourages those interested to visit the actual sites to refine our collective understanding of its current and historical forms and challenge and refine our analyses. It also allows for transparency and contextualization in our research, and for others to follow-up and debate our findings more precisely—all of which are

hallmarks of a *social scientific* endeavor. Yet there are limitations, particularly if we wish to maintain the relative anonymity of the people we study. This means that particular neighborhoods or buildings that are relatively small may not be named in order to protect participants, and researchers may find it difficult to gain access to organizations (see Jerolmack and Murphy 2017).^{vi}

NAMING PEOPLE

A default in much of qualitative research is to assign pseudonyms for the people we study in order to protect their identities and any unintended harm that they may be subject to as a result of their participation. However, in a move to further transparency in, and the rigor of, ethnography some scholars name the people that they study. For example, Colin Jerolmack and Alexandra Murphy (2017) advocate that this practice of naming people should be a default in social science research (see also Murphy and Jerolmack 2015). For example, they argue that researchers cannot guarantee anonymity, it may be more ethical to name people so as to allow them their own voice, and that “masking” information undermines ethnographic methods as a social scientific endeavor because people alter information to ensure confidentiality—thus not allowing for ethnographic revisits—and to make their work seem more generalizable than it is (Jerolmack and Murphy 2017).

They are not the only ones to name people in their work. In his 1999 book *Sidewalk*, Mitchell Duneier, after gaining informed consent during fieldwork and as he finished writing the book, both names the people he studied and provides their pictures. To ensure informed consent, he read aloud passages to his participants that pertained to them, and in the spirit of reciprocity Duneier continues to not only keep in touch and bring Hakeem—one of his key informants—to

conferences and class lectures,^{vii} but also provides the book vendors with royalties from his book (see methodological appendix, Duneier 1999).

Yet we also know that assigning pseudonyms does not always equate to our participants' anonymity or include the protections that we intend, particularly when people in the community from where the study was located can identify individuals because of the details included in published work (Jerolmack and Murphy 2017). For example, Nancy Scheper-Hughes (1979) examined mental illness in a small Irish village she referred to as "Ballybran." Upon her revisit to the site twenty-five years later, she was warned by the very people who she studied, that she was no longer welcome in the community. Not only did *Irish Times* reporter Michael Viney identify Ballybran as An Clochan by the information in her monograph, but the members of An Clochan read her book. While she (2000) says that many of the people she got to know would individually express to her that she "got it right" and that her book forced people to be reflective of their childrearing practices and were able to institute change, these same people shunned her because of the very real threat posed by the Irish Republican Army.^{viii}

Similarly, Alice Goffman (2015) assigned pseudonyms for her participants in her monograph *On the Run* and the associated 2009 *American Sociological Review* article. However, it was recently discovered that at least one of her participants' pseudonyms was a derivative or nick name of his actual name, because her descriptions of how he died included identifying information—that he was murdered outside of a Chinese restaurant in Philadelphia. If descriptions are linked with identifying information and the names we assign our participants derive from their own names, then they are not pseudonyms in practice, theory, or intent.^{ixx} Still others, such as Sudhir Venkatesh, give different pseudonyms for the same person, in different works, which raises questions about the precision in keeping track of qualitative data.^{xi} Similar

concerns regarding privacy, ethical considerations and protection of participants have been raised with regard to social network data, and the ability of researchers to identify original sources even without “access to the full dataset itself” (Zimmer 2010: 316). Ethical considerations are part and parcel of research methods, but as we see with Scheper-Hughes’, Goffman’s, and Venkatesh’s works, practices of maintaining anonymity for the places and people we study are not fool-proof, as Jerolmack and Murphy (2017) also point out.

However, there are ethical questions that we have to grapple with if naming people becomes a default in cases, even when there is not criminal activity, as Jerolmack and Murphy (2015, 2017) suggest. For example, what if interviewees say one thing to the original researcher, but something else to someone who revisits, because of the differing relationships they have or do not have with the researchers? Which account is more “correct”? Additionally, when we ask participants for their informed consent, whether in interviews, being welcomed in their lives, or to use their actual names, can they really ever give informed consent? Do they realize they are giving consent to possibly have additional researchers come into their lives thirty plus years after the initial research from which they were identified, and being asked not only about their current situation but about conversations, thoughts, and memories from years ago? How reliable and accurate would these memories be? How can we get participants to realize the extent to which they are being asked to identify themselves in academic research? What are the unintended consequences particularly as it relates to “studying down”—those who are less educated and have lower socio-economic status than we do as academics or “studying up”—those who have equal to greater education and socio-economic status than we as academics? One hypothesis is that this may unintentionally exacerbate the exploitation of poor, and perhaps minority and other underrepresented interviewees if those who have higher education and money refused to be

named because they are better able to comprehend some of the unintended consequences that come with being named in academic research. Although Jerolmack's (2013) study of the "global pigeon" suggest these concerns may not reflect reality since there was seemingly no difference in the race or class of people who chose to be named, and the unhoused people Duneier (1999) studied gave explicit consent, we must grapple with, and be thoughtful of, the possible unintended consequences of naming people when we choose this approach.

Some wealthy individuals have been named in ethnographic research—for example, Duneier (1999) names Richard Rubel, director of an Amtrak homeless outreach program, and Edward C Wallace, a lawyer, in his book. Yet, Duneier talks to these men—and other relatively wealthy individuals—in their official capacities, not as the primary participants of his book, since Duneier was following up with them regarding the transformation of Grand Central Station, the unhoused populations, and informal street vending. Interacting with someone in power and in their official capacity reflects a much different engagement than with those who have little to no power and/or who are the most vulnerable. Thus, we can separate naming people into two: naming people who speak to us in their official capacity and naming the primary participants of our research (who may or may not be vulnerable) (see Figure 1).

The movement toward naming people addresses some of the problems that arise when qualitative researchers default to masking identities vis-à-vis pseudonyms, and provides much needed rigorous fact-checking and transparency that is necessary in social scientific endeavors. Yet, there is a difference in naming people we come in contact with as part of their official capacity and those who are the participants of our research. Additionally, pseudonyms in research are able to provide *plausible deniability*. Even if people are able to guess the real identity of the interviewees, the participants are able to assert plausible deniability that it is them

precisely because they are not explicitly named. This is an important point that should not be underestimated. It is one thing to guess at someone's identity and another to know for certain whom those people are, though as we see in Scheper-Hughes' work, sometimes participants are unable to take advantage of such deniability.

SHARING DATA

In the spirit of making ethnographic and other forms of qualitative work more transparent, other scholars practice the sharing of data. Sharing ethnographic data takes many forms (see Figure 1). First, scholars share their interview questionnaires (e.g., Swidler 2003). While the data and methods sections of papers and books often detail the types of questions we ask and how we meet our participants, they generally do not include a comprehensive list of questions asked. Sharing interview questionnaires allows graduate students and junior faculty to see examples of successful work and finalized interview guides, and will also allow scholars to evaluate the wording of interview questions regarding any biases, see what kinds of questions were asked and how these compare to the claims detailed by the researcher from the answers. Yet, it also leaves readers with very little information with regard to what actually happened in the field, and what sorts of interactions researchers had with their participants.

A second way to share ethnographic data is to include detailed methodological notes as part of a book's appendices. This is something many ethnographers commonly write (e.g., Duneier 1999, Goffman 2015). These notes provide more context for what dynamics the field, and an opportunity for reflexivity, thinking about the ways in which our social positions shape not only our interactions in the field, but also what we see and hear. For example, Duneier (1999) discovered how his informants talked about him when he left his position near one of the book

tables for a short while and left his voice recorder on, revealing how his Jewish identity influenced his relationships with them—something he had previously be unaware. Yet, methodological notes in appendices are often *summations* of field work and are a selected accounting of what happened in the field.

A third way scholars have tried to increase accountability regarding ethnographic data is by making transcribed qualitative interviews accessible to researchers. For example, Doug Massey made available the qualitative interviews that he and Magaly Sanchez oversaw in their “Transnational Identities and Behavior: an Ethnographic Comparison of First and Second Generation Latino Immigrants” project.^{xii} This is a subproject from their two larger quantitative studies, and includes 159 interviews from first and second generation immigrants in northeastern U.S. These interviews are available for analysis and scrutiny in the original language in which they were conducted. This move toward making interviews accessible is one in which the discipline of history has been a part of for a long time. The Oral History Association (OHA) is an organization of historians that have established “goals, guidelines, and evaluation standards” in “the collection, preservation, dissemination and uses of oral testimony.”^{xiii} In preserving first person narrative accounts, scholars aim to have these narratives become a part of the historical record and as such, the default is to name the individuals telling their stories.^{xiv}

However, there is a key difference between the disciplines of history and sociology with regard to access to such accounts: compliance and approval from universities’ IRBs. As a discipline, history straddles the line between being a part of the social sciences or the humanities, which has implications for whether and how oral history research in particular relates to IRBs and the Common Rule standards. One reason oral histories face contestation regarding IRB accountability is because there is a debate on whether this research is “generalizable” and thus

falls under Common Rule guidelines. Research is defined by the Department of Health and Human Services as “a systematic investigation, including research development, testing and evaluation, designed to develop or contribute to generalizable knowledge.”^{xv} Recently, the U.S. Health and Human Services, along with 15 other federal agencies, proposed revisions to the current Common Rule to “[e]xclude from coverage under the Common Rule certain categories of activities that should be deemed not to be research, are inherently low risk, or where protections similar to those usually provided by IRB review are separately mandated.”^{xvi} The OHA, among other organizations, released a statement in support of this recommendation. Part of their argument is that because the practice of oral history focuses on particular individuals, it is not research that is generalizable, and thus should be excluded from review under these proposed guidelines.^{xvii} In contrast, qualitative social scientists study particular sets of people or places with the goal of contributing to more generalized knowledge about the social world, and as such, are subject to IRB approval.^{xviii} Making interviews subject to scholarly scrutiny and analysis is another step toward making these methods more transparent.

Another way to share ethnographic data is to share field notes. Field notes constitute the raw data of ethnography and other types of qualitative research, in the sense that they contain information and analyses of our observations regarding our surroundings and interactions within them while we are in the field. Whereas programs such as STATA and R are the tools used by quantitative researchers to analyze social life, for researchers who talk, interact, and spend time with people, we are the tools used to gather and analyze data.^{xix}

What constitutes a field note? Knowing what they are is not intuitive.^{xx} Field notes is a catch-all phrase that describes a wide array of ethnographic data (see table 1). For example, I

have a set of field notes stored on my phone that are pure count data on the number of foreigners I see within and outside the Subic Bay Freeport Zone, Philippines (see Figure 2).

Figure 2: An Example of How I Kept Track of the Number of Foreigners I saw within SBFZ

1wForeignbabae, 1wF, 1wF, 2, 3wForiegnbabae, 2, 1, 1, 2, 1, 2, 2, 5 (1 bl), 1, 1, 1wF, 1wF, 1babae, 1, 1, 1 babae, 1wF, 1,1, 1, 1wF, 1, 1, 1wF, 1babae, 1blwF, 1, 1, 2babae, 1babae, 1, 1,1, 1bl, 1, 1, 4(3babae), 1bl, 2, 1, 1, 1, 1bl, 1blbabae, 1wF, 1, 1wF, 1, 1wF, 2(1babae),1, 1bl, 1wF, 1wF, 1wF, 1, 1, 1wF, 1wF, 1, 3(1babae), 1, 1bl, 2babae, 1bl, 1, 2, 1, 2babae, 1, 2babae,, 2, 1, 3, 1wF, 2babae, 1, 1wF, 2, 1, 1, 3(1babae, 1bata), 1, 1wF, 1babae, 3(1babae), 1, 1, 2babae, 1wF, 3, 1, 1wF, 1wF, 2, 1wF, 1, 2bl(1babae), 1, 2, 2, 2, 5, 3(1babae), 4(1babae, 2bata), 1wF(and family), 1wF, 2wF, 1wF, 5(2bl babae), 1wF, 2(1babae), 2, 1wF, 1babae, 2, 1bl, 3(1bk), 2(1babae), 2babae, 1, 1wF, 1babae, 1wF, 1, 1babae, 1, 1wF

Source: Field notes stored on phone.

Note: These are not all of the counts I collected, only a snippet. *Babae* means “female” in Tagalog, and in my notetaking it stands for a foreign woman—you’ll see that I originally would write out “Foreignbabae, but for time considerations, I shortened it to “babae.” “wF” is shorthand for “with Filipina,” “bl” is shorthand for a Black foreigner, while “bata” means “child/children” in Tagalog and refers to a child, I did not keep track of whether the bata was foreign or Filipino, particularly since nationality could not be as inferred easily when accompanied by adults who are of different nationalities.

I also have other field notes that are detailed third-party interactions, such as when the US military was docked and how many military personnel behaved in a FZ coffee shop or a description of a blessing:

Sept 7 [2012] | 10:55am

In [coffee shop] collecting US court cases and a bunch of people came in. There a priest – identified by his white garb –walked through toward the employee back area, which is near where I am sitting. The others gather around in front of the cash register while priest throws holy water from his tray, in all directions. I assume it’s a store blessing? Then another person, right behind the priest threw 1 peso coins and handfuls of mentos (small candies). And everyone scattered to pick up both the coins and the candies. I’m able to get 3, 1 peso coins.

Source: Hand written field notes in journal

I also have field notes that were taken while interviewing, and others yet, that are my thoughts after interactions, and my musings in the field on what to do next:

Sept 4th 2012 | 5:42pm

- Private/public -> dichotomy. Eg: jeepneys not allowed but free shuttle and shuttle to Pure Gold

- Trash: pick up inside, outside not

- Ayala Harbor Point

- The busiest I've seen the SBFZ -> what I had been expecting the first time I came. There were tons of foreigners
- Seems to [skew] younger, perhaps different demographic
- Royal Subic/Pure Gold world attract a difficult kind of clientele
- One buys groceries -> so perhaps longer term visitors, not vacations for a week or two. [...]
- Makes me think about whether or not to change from Royal Subic to Harbor Point. Pros -> no shortage of employees, visitors and foreigners. Cons -> how could I whittle it down, who could I interview? I think it would provide very different insights. Which one to choose?

Source: Hand written field notes in journal

Table 1: A Selected List of My Varied Types of Field Notes

Types of field notes	Description
Phone	Observations, descriptions and counts of people as they occur Stored on my password protected phone
Photographs	Photographs of places I visited

	I do not include photographs of particular individuals unless they are in a public space
Handwritten in a journal	Observations, descriptions, analyses and counts <ul style="list-style-type: none"> - As they occur - Immediately after an interaction or observation - Summation at the end of the day or set period of time

There are also many ways to share our field notes (see Figure 3). One way to share field notes is with students as part of our pedagogical strategies to train them in qualitative methods. Since what constitutes field notes is not self-evident, hiring graduate and/or undergraduate students to transcribe field notes is an excellent opportunity to train and involve them in the research process. This is what I have done. I hired an undergraduate senior, Minh Tran, to transcribe my handwritten notes in a journal I kept while in the field. In order for her to participate in this project, she had to complete National Institute of Health human subjects training. She also had recently finished her senior thesis on peri-urban farmers that was based on ethnographic fieldwork in Vietnam, so this allowed her an opportunity to compare ways in which we are able to keep track of data and use it in our writing.

In her reflections on her work transcribing my field notes, Minh identified four main things she learned. First, project management. She writes, “I thought it was very helpful to see your notes on how you organized your work plan and coped with unexpected problems. I recognized that you also raised questions relevant to your study while dealing with bureaucracy/institutions to get access to documents -> how every single step in the process tells you something.” Second, was learning how to observe. For example, “after having done field research on my own, I was able to reflect as I transcribed your notes and learned how you paid attention to everything around you and interpreted it while avoiding assumptions/biases. I remembered some very interesting observations as you sat in [coffee shop] that I myself might

not have paid attention to.” Third, she learned how to tailor questions to interviewees, and finally, because she is also working with me on coding documents for another project, she notes “having transcribed your notes makes the coding work that I’m doing now easier since I have some sense of the place already!” In reviewing my own notes with her, and answering her questions, it allowed me to reflect on the quality and precision of my field notes and what I can and cannot say from it. We may also share our field notes in the classroom, for in- or out of class assignments, whether using ours as a model from which students should emulate in their own observations, or to read and reflect on positionality and data in ethnographic research.

Figure 3: Sharing Field Notes

* Being explicit about when we are sharing field notes versus summations or analyses stemming from our data

Second, we can incorporate field notes into our writings, including the main texts and methodological appendices, as well as in presentations of work—and being explicit about when we do so. As previously mentioned, many researchers include methodological appendices. Our

writings also include dialogue, observations, and analyses that are often taken from our field notes. Similar to how scholars differentiate between quotes from tape recorded conversations, and those from field notes (e.g., Jerolmack 2013), scholars should be explicit regarding our use of field notes in our writings. Anderson (1992) does this by italicizing both conversations and/or interview quotes and descriptions from his field notes. This allows the reader to parse out the difference between when Anderson is describing events or places as he sees them, or when he is describing scenes at a much later time, during his analysis.

Third, we share them with other colleagues (who have gone through requisite human subjects training). This can be informally or formally, by housing them in institutional repositories to be available to other researchers. For example, George Murdock and Mark May started a project that would develop into the Human Relations Area Files (HRAF), which continues to be housed at Yale and is currently disseminated to almost 400 educational facilities in the U.S. and other countries.^{xxi} The HRAF is one of many similar projects that were created in the pursuit of cataloging data to enhance general knowledge about cultures around the world (e.g., Lemov 2009, 2011), and follows in the footsteps of two major movements—the first is toward digitizing humanities. Here, documents, oral histories, and other types of raw data are being put online for transparency, to share data, and increase research accessibility.^{xxii} The second is the movement toward open access and/or open source data and publications. While digitizing humanities is firmly rooted in the humanities fields, digital places like the Center for Open Science (<https://cos.io/>) extend the sharing of data to the social sciences.

Sharing notes makes our logical processes and inference transparent, as seen in the aforementioned small excerpts from my own field notes. For example, in one of the above field notes, you can see how my logic of case selection evolved while in the field. I initially intended

to focus on one of two duty-free stores that had items ranging from groceries, makeup, and jewelry to electronics and home goods. However, in the course of observations, I observed many more foreigners and the type of interactions and behaviors I was looking for in the recently built Harbor Point mall when compared to either Royal Subic or Pure Gold—two of the most popular duty-free stores in the SBFZ. Above, I think about the pros and cons of changing my case selection and my initial musings on what that means for the project. Yet, in my writings to date, I haven't yet discussed this decision to focus on Harbor Point rather than Royal Subic. When I included this field note as an example, a reviewer helpfully pointed out that this was an important point that I probably addressed elsewhere. Because I shared this information, I will likely include this information in my current writings about Subic Bay as I reflect on the methodological and theoretical importance of changing field sites. As such, others can hypothesize and/or investigate how and why dynamics between these two differ and what that means for foreign-local encounters in global borderlands (Reyes 2015).

By sharing field notes, we can compare which types of notes sheds light on types of field dynamics and collectively discuss how, why and when we can improve our data collection. It also allows us to reflect and collectively discuss our reasoning regarding which data to share, what to exclude and why, since what we think is important to include in our writings does not necessarily correspond to what others might think is important. It allows for others to critique and extend our writings by pointing out crucial but missing information that readers would not have known about without the sharing of field notes.

Making field notes available to other researchers allows for data mining, and can provide primary sources of data for ethnomethodologists and researchers studying the history of science. It also allows other ethnographers access to additional data that can complement their own work.

Ethnomethodology is concerned with the construction of social order at the micro-interactional level, with conversation analysis being only one approach that focuses on utterances and sequence of dialogue (Maynard and Clayman 1991; Meier 1982). Others have applied ethnomethodological approaches to written sources (Atkinson 1988). Meanwhile, researchers interested in the sociology or history of science focus on the development, construction, and maintenance of science itself (e.g., Gieryn 1983). As such, sharing field notes provides primary data that would be of interest to both ethnomethodologists and sociologists of science regarding how ethnographers create social order in their field notes and how the field of ethnography is constructed. Similarly, while rare, field notes provide rich sources data for ethnographic reanalyses—“the interrogation of an already existing ethnography *without* any further field work” (Burawoy 2003: 646), ethnographic updates (“which brings an earlier study up to the present but does not reengage it” (646)) and ethnographic revisits (see Jerolmack and Murphy 2017). They also may provide nuggets from which future studies may be constructed. For example, while my observations regarding the blessing for the opening of the store is not necessarily of interest to me, reading this account may stir an interest for another researcher to follow up on in the course of their own research.

Finally, depending on whether a scholar opts to also name a place or name people, making field notes available provides an opportunity for others to revisit the same sites and/or allow for a comparison between their notes and analyses with those of others. For example, linking field notes with named places allows researchers to compare interactions across time and space (whether the place grows or shrinks), thus preserving a record of how the place works, which future researchers can use and extend in research on the place in the future. When field notes correspond to the practice of naming people, this also allows for a comparison. Participants

themselves can comment on particular interpretations of the researcher, and scholars can follow up with the interviewees to get their point of view on what occurred—getting a more comprehensive view on the setting and actions as described by the researcher.

What about researchers who choose confidentiality and do not name people or places—how do making field notes fit into this type of work? Field notes can include non-identifiable personal and place-specific notes. By non-identifiable personal data, I refer to observations that cannot be linked to specific *people*. This can be done by sharing notes that include observations and interactions of unnamed third parties—like people in a coffee shop (Oldenburg 1999 [1989]), public restroom (Humphrey 2009 [1970]) or public square (Whyte 2009 [1988]; Low 2000)—which sheds light on dynamics of social life within these places. What can be useful about field notes that have identifiable place details removed? They can be useful in describing the setting, contextualizing the research, and in situating the researcher in a particular place and time since the researcher is the social scientific instrument in qualitative research. Since reflexivity “affects both writing up the data...and the data’s status, standing and authority” (Brewer 2000:127), knowing researchers’ thoughts and real-time analyses helps readers better understand the data and argument they present, and the sharing field notes can enable readers to point out flaws in researchers’ reasoning and points of view.

However, sharing field notes, in any form, contains many complicating factors, faces much criticism, and raises important questions on the unintended consequences of this practice. For example, HRAF has been criticized for its aim to universalize and compartmentalize data which needs to be contextualized in order to be meaningful, the data quality, the emphasis on evolutionary processes in societies, and how these things justify and reproduce forms of colonialism (e.g., Clarke and Henige 1985, Cohn 1996). There needs to be a careful balance

between perpetuating our own biases around the world and place societies in moral or evolutionary categories versus being reflexive about the claims we make and allowing for more varied view points and analyses of the same site, topic and/or people.

Additionally, transcribing field notes requires an extensive time commitment. When we hire students to transcribe them as part of our pedagogical tools, it requires a substantial financial commitment and may rely on the ability to obtain grants. If we share our field notes, we also need to comb through them to assign consistent pseudonyms to people (if we promise anonymity) and to redact particular notes that may cause intentional or unintentional harm to participants. This stands in stark contrast to sharing interview transcripts, which are relatively easy to de-identify and are not as long or extensive to transcript. Field notes also contain specific phrases or information that others may not be able to interpret. Thus, if we are to share our field notes, we need to also practice annotating them, similar to the notes that accompany the field notes I share in this paper, which requires an intensive time and financial investment.

Ethnographic field notes also include our personal, private thoughts on our interactions with participants. Yet, these personal, private thoughts are part of the data—they are the tools researchers use to make claims. If we made personally identifying information available, this may shape what it is we write down, how our own respondents then view their interactions with us, and limit access for future researchers. This may also inadvertently cause field notes to become standardized to the field's detriment, curtailing creativity.

It also raises questions about who, when, and whether to ask permission regarding field notes that we intend to share. For example, should we gain permission to take field notes in public spaces? What about private spaces, such as malls, where you do not necessarily need permission to enter and are able spend a long amount of time there? Malls are privately-owned. I

gained permission from mall officials to conduct my interviews within the Harbor Point mall in the SBFZ—should this also include permission on field notes? What about places, like coffee shops, that have official (no shirts, no shoes, no entrance) and unofficial rules regarding who can enter, but are comparatively unregulated. Do we ask permission when we are passive observers?

Finally, does sharing field notes necessary entail only those projects that are relatively non-controversial in order to protect our participants? If so, are there unintended consequences regarding the sharing of certain field notes over others, including a move toward encouraging either more mundane or more sensational field work or certain forms of field work being considered more scientific than others? These are important questions with which to grapple if and whether we decide to share our field notes, in any form.

CONCLUSION

As a method, ethnography is concerned with the taken-for-granted aspects of life and its relationship with greater social structures. Although concerned with the meanings and moralities that people create, a defining element of ethnography is the juxtaposition between “what they say” and “what they do” (e.g., Jerolmack and Khan 2014). Every relationship, interaction, and conversation is shaped by power distributions, cultural understandings and historical contexts. Since the *context* of conversations shapes its *content*, the process of “being there” – participating and interacting with individuals over the course of events and through time – allows ethnographers to grasp a rich portrait of the social world they are studying.

Yet, precisely because “being there” is a cornerstone of ethnographic and other types of qualitative research, ethnographic data is dependent on personal interactions, behaviors, relationships, and conversations. How can we ensure that ethnography is taken seriously as a

social scientific enterprise? In this paper, I draw on a wide array of ethnographic research to show how ethnographers have attempted to be transparent in their work. In highlighting three common models of ethnographic transparency, I combine a positivist tradition that focuses on data, with interpretative and post-colonial approaches that highlight meanings and reflexivity. These models include what Jerolmack and Murphy (2017) suggest as naming places—something that has a long history in the social sciences—and naming people. They also include another model that was the subject of a recent debate in *Ethnography*: that of sharing ethnographic data, which may include sharing interview questionnaires, writing methodological appendices in books, storing qualitative interviews in institutional repositories, and sharing field notes with students, in our writings, and/or with other researchers.

In describing three common models of ethnographic transparency, this paper makes several contributions. Theoretically, I situate the decisions ethnographers make in naming places (regions, cities, neighborhoods), naming people (primary participants, public officials) and/or sharing data (questionnaires, transcripts, field notes) as part of their methodological toolkits. These decisions are not dichotomous. For example, Anderson (1992) both names the region in which *Streetwise* is located and is explicit in his use of field notes and quotes. They are also strategic and can evolve over time, depending on the project and the people and places we study. For example, returning to Anderson's work, we see that while *Streetwise's* location was anonymous, albeit on the East Coast, his 2011 book *Cosmopolitan Canopy*, was based on fieldwork in the Reading Terminal Market in Philadelphia. The latter project necessitated revealing the location of his field site. Empirically, I synthesize these varied approaches and their benefits, disadvantages, and related questions that arise in their use. Although one key limitation is that I do not address the use of composite characters in ethnographies, this paper will help

researchers grapple with the social scientific decisions—and the related consequences, intended or otherwise—we continually have to make in the course of our research.

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ENDNOTES

ⁱ Jerolmack and Khan (2014) argue that this is the foundation of ethnography and warn against the dangers of what they call the attitudinal fallacy—“the error of inferring situated behavior from verbal accounts” (179). However, they are not the first to propose that foundation of ethnographic methods. The difference between “what they say” and “what they do” was at the center of my ethnographic training. Their discussion of the different strengths of methods and how different methods answer different questions was also a core element of my own graduate training more generally.

ⁱⁱ See for example, recent controversies regarding Goffman (2015), including her ethnographic survey (Cohen 2015) and ethical considerations (Lubet’s critique (<https://newrepublic.com/article/121909/did-sociologist-alice-goffman-drive-getaway-car-murder-plot>, last accessed 9/5/16), Goffman’s reply (<http://www.ssc.wisc.edu/soc/faculty/docs/goffman/A%20Reply%20to%20Professor%20Lubet.pdf>, last accessed 9/5/16), Lubet’s reply (<https://newrepublic.com/article/121958/sociologist-alice-goffman-denies-murder-conspiracy-run>, last accessed 9/5/16); New York Times summary of the debate between Goffman and Lubet: <http://www.nytimes.com/2015/06/06/books/alice-goffmans-heralded-book-on-crime-disputed.html>, last accessed 9/5/16. Also, see Sudhir Venkatesh’s (2008) participation in the beating of someone and other ethical concerns (see New York Times summary of some of the controversies of his work: <http://www.nytimes.com/2012/12/02/nyregion/sudhir-venkatesh-columbias-gang-scholar-lives-on-the-edge.html>, last accessed 9/5/16)

ⁱⁱⁱ In 2011, journalist John Kelly revealed the location: 11th and M streets NW in Shaw, which he uncovered through Liebow’s wife, Harriet, see: <http://www.washingtonpost.com/wp-dyn/content/article/2011/02/26/AR2011022603483.html>, last accessed December 17, 2015

^{iv} It is important to note for those who name regions, they also provide as much detail as they can about the place they study, without giving away too much identifying information

^v See Jerolmack and Murphy 2017 for how quantitative scholars tested and compared Klinenberg’s work to others

^{vi} For a discussion of organizational ethnography, see the following blog post: <https://orgtheory.wordpress.com/2015/06/09/organizational-ethnography-and-pseudonyms-at-what-cost/>, last accessed September 5, 2016

^{vii} Personal observations, from 2009 onward

^{viii} See Jerolmack and Murphy 2017 for another discussion of Scheper-Hughes’ work. Also see an exchange between Eileen Kane and Scheper-Hughes (Kane 1982a, Scheper-Hughes 1982, and Kane 1982b) for this and other controversies over her work

^{ix} For example, “Chuck” was easily identifiable for Charles Tunstall, see: http://articles.philly.com/2007-07-23/news/25239972_1_shooting-deaths-shooting-rampage-bet, last accessed December 14, 2015. Additionally, reporters, like Jesse Singal, were able to identify others in her work, see: <http://nymag.com/scienceofus/2015/06/i-fact-checked-alice-goffman-with-her-subjects.html#>, last accessed December 14, 2015.

^x See Jerolmack and Murphy 2017 for a brief discussion of Goffman’s work

^{xi} See Jeremy Freese’s critiques of the same story told differently in Venkatesh (2008) and Levitt and Dubner (2009) here: <https://scatter.wordpress.com/2008/02/01/the-strange-case-of-dr-booty-and-mr-t-bone/>, last accessed December 14, 2015

^{xii} See: <http://opr.princeton.edu/archive/iip/>, last accessed December 17, 2015

^{xiii} See: <http://www.oralhistory.org/about/>, last accessed December 23, 2015

^{xiv} See information on best practices here: <http://www.oralhistory.org/about/principles-and-practices/>, last accessed December 23, 2015

^{xv} See: <http://www.hhs.gov/ohrp/humansubjects/guidance/45cfr46.html#46.102>, last accessed December 23, 2015

^{xvi} See: <http://www.hhs.gov/ohrp/humansubjects/regulations/nprm2015summary.html>, last accessed December 23, 2015

^{xvii} See: <http://historycoalition.org/wp-content/uploads/2015/10/Issue-Brief-Proposed-HHS-Rule-on-Protection-of-Human-Subjects-in-Research.pdf>, last accessed December 23, 2015

^{xviii} The debate on whether social scientists should be subject to IRB approval is not the purpose of this paper.

^{xix} Researchers also use other tools to analyze collected data, such as ATLAS.ti, Excel etc. Here, my focus is on how data is collected by people and as such, situating our own experiences and knowledge in the field is important. This is why reflexivity is important to include in our analysis. I suggest one step further, in that our field notes contains important information regarding our data collection and processing.

^{xx} Although in my ethnographic training, we shared analytic memos, we did not share varied types of field notes. In the conference that formed the basis of this special issue, other panelists expressed surprise and delight to find out that I took different types of field notes and shared them, because it confirmed similarities in their own note taking.

^{xxi} See: <http://hraf.yale.edu/about/history-and-development/>, last accessed March 5, 2016

^{xxii} See for example the National Endowment for the Humanities' Office of Digital Humanities (<http://www.neh.gov/divisions/odh>) and <http://dhcommons.org/about>, both last accessed December 29, 2015.