RESEARCH ARTICLE

Making Better Numbers through Bioethnographic Collaboration

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ABSTRACT In this article, I describe my ongoing bioethnographic collaboration with a multidisciplinary team of exposure scientists in environmental engineering and health. First, I explain how and why integrating ethnography and number-based disciplines is such a complex, time-consuming, and worthwhile process, when ethnography produces a kind of excessive "big data" that is not easily enumerated. Then I describe three of our current bioethnographic projects that seek to make better numbers about how neighborhoods, water distribution, and employment and chemical exposures shape bodily processes in a highly unequal world. To conclude, I reflect on how we might harness ethnographic excess for making better numbers and thus better knowledge, and also how bioethnographic collaboration inevitably transforms ethnography even as we insist on its excess. [*collaboration, methodology, ethnography, big data, biomedical science*]

RESUMEN En este artículo, describo mi colaboración bioetnográfica en curso con un equipo multidisciplinario de científicos sobre exposición en ingeniera ambiental y salud. Primero, explico cómo y por qué el integrar etnografía y disciplinas basadas en números es un proceso tan complejo, consumidor de tiempo y útil, cuando la etnografía produce una clase de "big data" excesiva que no es enumerada fácilmente. Luego describo tres de nuestros proyectos bioetnográficos actuales que buscan hacer mejores números acerca de cómo nuestros vecindarios, distribución de agua, y empleo y exposiciones químicas impactan los procesos corporales en un mundo altamente desigual. Para concluir, reflexiono sobre cómo podríamos aprovechar el exceso etnográfico para hacer mejores números y así un mejor conocimiento, y también cómo la colaboración bioetnográfica transforma

inevitablemente la etnografía aun cuando insistimos en su exceso. [*colaboración, metodología, etnografía, big data, ciencia biomédica*]

"You know too much! Ethnography is big data!" Brisa, the biostatistician, buried her face in her hands, partly in jest and partly in frustration. She and I are two of many researchers working together as part of a longitudinal birth-cohort study in Mexico City called Early Life Exposures in Mexico to ENvironmental Toxicants (ELEMENT). Brisa's comment crystallized the challenges of understanding and navigating the relationship between ethnography and research methods that generate numerical data.

Before I arrived in Brisa's office that afternoon, I had been at a meeting with other ELEMENT team members discussing a hypothesis about the effects of chemical exposure on menopause among study participants. In my mind the hypothesis was flawed because it presumed that the experience of menopause was everywhere the same. To prove my point to Brisa, I rattled off detail after detail about how radically different the participants' lives are from those of the researchers.

As a biostatistician, Brisa routinely works with data sets of millions, but my exhaustive ethnographic knowledge about the lives of a small subset of participants was just "too much." She voiced a concern I had heard from other team members: ethnography is excessive and unruly in its comprehensiveness, especially because its findings are not easily quantified or standardized. Over time, though, these colleagues have come to tell me they value the ethnographic excess I bring to our collaboration. Initially they expected that I would offer ethnographic perspectives on numbers they had already made. Now we were trying to make numbers together, which means we were fundamentally reworking the ways we all make knowledge.

This collaboration is an example of what I call *bioethnography*, a method my team and I have been developing since 2012 in order to make knowledge about dynamic and situated bodily processes in a highly unequal world (Roberts and Sanz 2017). Bioethnography seeks to generate integrated biomedical and ethnographic knowledge without separating natural from cultural processes. It insists that intensive ethnographic engagement drive questions from the start, instead of being retrospectively applied to numerical data already collected. Although ethnography is not well suited for making numbers on its own, it can be understood as a kind of "big data" that, in the long run, can be used to ask questions that produce better numbers and better knowledge.

On its own can, ethnography can bring valuable insights to bear on complex problems, like the relationship of inequality to health, but numbers have specific authoritative power for making reality by driving policy and interventions. Thus, bioethnography harnesses ethnographic excess to make *better* numbers, numbers better situated within the reality they purport to represent. My point, of course, is that ethnographic "excess" is not excessive at all. Knowledge that combines numerical data with ethnographic insight has the potential to identify and dislodge conditions that perpetuate inequality and ill health.

In this article, I divide my discussion into two parts. In part 1, "Bioethnographic Collaboration," I narrate how I came to work with ELEMENT and lay out the principles of bioethnography as we have developed it so far. In part 2, "Making Better Numbers," I describe three areas of collaborative investigation—neighborhoods, water quality, and aging—to illustrate the complexities and advantages of integrating critical ethnographic approaches and numbers-based disciplines. In each case, I describe numbers we have made, or could make together, that require ethnographic big data to know the world better.

PART 1—BIOETHNOGRAPHIC COLLABORATION

1.1 Bioethnography

In l993, a team of Mexican and US environmental health researchers established the ELEMENT project with the objective of understanding the effects of early-life nutrition and exposure to toxicants (such as lead and phenols) (Perng and al 2020). Since 1993, ELEMENT project researchers have collected and analyzed biological samples from about 1,600 participants (800 mother-child pairs) recruited through publicly funded clinics in Mexico City (Afeiche et al. 2011; Hu et al. 2006). ELEMENT's epidemiological findings have been detailed in over one hundred publications and have influenced policy decisions about lead and fluoride exposure, especially in the United States. Collection of additional biomarkers of toxicant exposure and molecular and other outcomes is scheduled to occur over the next years.

Like many longitudinal birth-cohort studies, ELEMENT maintains a bank of participants' biological samples (which include blood, urine, hair, toenails, breast milk, and teeth) and questionnaire data, which allows reanalysis of samples in response to newly arising questions.[1] The project has expanded to include study of new toxins (e.g., BPAs, mercury, and fluoride) and new health concerns (e.g., obesity, menopause, sleep) that can be studied through both newly collected data and the retrospective analysis of stored samples (Bashash 2017), using new frameworks, such as epigenetics, by which researchers link toxicant exposure to changes in gene expression that might affect health outcomes.

In 2012, I began conversations with the ELEMENT project PI, Karen Peterson, about conducting ethnographic observations among study participants and ELEMENT team members.

[1] Biosampling among marginalized groups is an ethically complex issue that I cannot adequately address here (Benjamin 2016; Prussing 2018; Radin 2017; TallBear 2013). Through the examples below I make the case that making better numbers through bioethnographically analyzed biosamples can be one powerful tool for intervening on the conditions that produce inequality.

Collaborating with ELEMENT would offer me the opportunity to deploy insights from feminist and other science and technology studies (STS), as well as from Latin American studies, for understanding phenomena as *always* relational, contingent, constructed, and enacted through practice (Latour 2010; Mol 2002; Murphy 2013; Stepan 1991; Wade 1993; Weismantel 1995). Following Donna Haraway, I wanted to study dynamic bodily conditions as *situated* within specific ecological niches (Fuentes 2018) and acknowledge that scientific knowledge is always situated, as it is produced in relation to these conditions (Haraway 1991). I saw ethnography as crucial for documenting phenomena and also for making them.

In particular, I wanted to deploy approaches from feminist medical anthropology and STS like Margaret Lock's situated biologies framework (Lock and Nguyen 2010) and Anne Fausto-Sterling's call for dynamic and developmental systems approaches (Fausto-Sterling 2005) to make better knowledge about the relationship between health and inequality. Thus, instead of combining *objects* of inquiry (biology and culture), I conceived of bioethnography as combining two different methods for knowing the world (Mol 2002, 153)—ethnographic observation and biochemical sampling—in order to ask and answer research questions that could not be addressed through either method alone. This methodological focus involves exploring how our data collection and analysis might be shaped if we suspended the nature/culture binary. Thus, blood lead levels and household organization, body mass index (BMI), class hierarchy, hospitality, international trade agreements, and biostatistical and coded ethnographic data are designated as neither nature nor culture. Instead, in bioethnographic collaboration, researchers would ask questions about bodily and environmental conditions together, while continuing to use different methods to discover how these phenomena are produced. Asking better questions would allow us to produce better data, leading to better and more comprehensive knowledge. The life scientists would hopefully come to understand biological processes as always dynamic and situated, and I would have to become willing to stand by the numerical knowledge we produced together.

Initially these aims were mine, not ELEMENT's. ELEMENT researchers thought my work might provide social context for interpreting statistical findings after data collection. But in bioethnography, ethnography is not consultancy—it's a driver. Most epidemiological studies deploy standard data-analysis models, built and validated elsewhere, that attempt to isolate the unidirectional effect of one variable (e.g., a toxic exposure or DNA methylation pattern) at a time. And even if they take a more complex approach (e.g., examining the effects of combined chemical exposures), they pay relatively little attention to participants' bodies as situated in a specific time and space. I wanted to develop a means to carry out complex multivariate analysis (assuming that phenomena cause and are caused by more than one variable), which in theory would illuminate how phenomena, including geopolitical processes and the things that matter to individual participants, are created through intra-active looping (Barad 2007; Hacking 1995). The point, then, was not to critique science-as-usual (although I have done that as well), but to restructure how ELEMENT makes knowledge.

Bioethnography has thick connections to anthropological efforts to combine quantitative with qualitative data (Bledsoe and Cohen 1993; Colson 1971; Scrimshaw 1979). A rich vein of research has combined cultural anthropology and epidemiology throughout the twentieth and twenty-first centuries (Heggenhougen and Shore 1986; Inhorn 1995; Trostle 2005). Bioethnography, however, is perhaps more akin to the proto-epidemiological studies carried out in the mid-nineteenth century, before the rise of germ theory. In their investigations of the conditions that caused illness, these studies did not distinguish between what we now call society and biology (Fleck and Ianni 1958; Virchow 1985). Soon afterward, however, the bacteriological revolution set epidemiologists on a narrower hunt for biological pathogens. Although some researchers in the last century sought to recombine biological and cultural research (Inhorn and Janes 2007), epidemiology has tended to formulate nature and culture as different domains: *nature* as material entities like parasites, toxins, and genes, and *culture* as immaterial beliefs and behavior.

This powerful nature/culture binary has had long-term effects, especially among public health researchers. Despite medical anthropologists' protestations, public health and public policy researchers tend to regard culture as a variable (e.g., beliefs that shape behavior) that can be captured and quantified in surveys and transformed through health education. They also regard culture as divorced from political and economic processes, as seen in the ignominious deployment of the "culture of poverty" concept (Bourgois 2001) and in the devastating effects of the worldwide decimation and privatization of social welfare and health care (Breilh 2008; Robinson and Pfeiffer 2015). Maintaining this Euro-American nature/culture binary makes it easier to miss the intertwined processes that shape phenomena (Meloni 2014; Roberts 2016; Stepan 1991).

Nevertheless, in collaborative projects, anthropologists tend to bring culture, while the life scientists bring biology. This division of labor is similar to what US-based biological anthropologists have formulated as a "biocultural synthesis," a form of inquiry that explores the role of culture in shaping human biological evolution and adaptation. Biocultural anthropologists have made essential contributions to the understanding of complex phenomena such as disease transmission and dietary adaptation (Lindstrom et al. 2011; Wiley 2008), but they do not tend to engage in long-term ethnographic work. And for the most part, biological anthropologists who use a biocultural framework designate in advance which variables are cultural (e.g., poverty and food preferences) and which are biological (e.g., caloric expenditure and disease rates) (Dufour 2006; Goodman and Leatherman 1998). This approach limits our understanding of interactive processes. Within biocultural synthesis and cultural epidemiology, then, biology tends to remain the bedrock on which the *soft* ideational world of culture rests.

Because of this history, I do not use the word *culture* in my bioethnographic work, despite its importance for understanding how the meaning people attribute to their experience powerfully shapes everyday life. Researchers in STEM fields too easily oppose *culture* and *biology.* Understanding culture as only additive to biology, or designating certain processes as either cultural or biological, or pairing the two in dialectic (Goodman and Leatherman 1998), prevents us from investigating the highly complex relationships through which phenomena are made.

To examine how phenomena are made requires opening up and scrutinizing “black-boxed” methodologies (Latour 1987) for making numbers, a time-consuming process. Thus, in our bioethnographic collaboration with ELEMENT researchers, my team and I have critically examined how measurements and tools like participant socioeconomic status (SES), BMI, and food frequency questionnaires (FFQs) are constructed and how they reinforce preexisting inequality (Jansen and al 2020; Téllez-Rojo 2020). To do this we have used my own ethnographic work in Mexico City among both ELEMENT participants and *researchers* and the insights of other social scientists about how bodily distinctions reinforce hierarchy (Kulick and Meneley 2005). But as my team and I have become complicit in making numbers, we also complicity “bracket” some of the practicalities of making them (Mol 2002). For instance, in trying to understand the effects of neighborhood on health outcomes, we decided to treat participant addresses as stable and singular, bracketing our knowledge that people rarely remain at one address throughout their lifetimes and that some ELEMENT participants move between neighborhoods on a regular basis.

Bioethnography also differs from cultural epidemiology and biocultural anthropology in the way research questions are developed. Long-term ethnographic fieldwork fosters what can seem like an excessive initial vagueness to scientists who deductively make hypotheses in advance. In my collaboration with ELEMENT, I have been able to insist on long-term, open-ended ethnographic engagement, or "slow research," which does several things at once: it prevents the anticipatory narrowing of research questions that "ignores other types of data that do not fit the hypothesis-organized-design" (Adams et al. 2014), and it allows the ethnographer to avoid being positioned as a consultant after the fact on data designated "behavioral" or "local" (for excellent examples of multidisplinary projects driven by open-ended, long-term ethnography, see Bond 2021; Messac et al. 2013; Rosenblum et al. 2014; Straight et al. 2019). This also makes bioethnography different from rapid ethnographic assessment, which involves teams of anthropologists who assess a situation quickly by involving community members in answering a predetermined set of research questions (Taplin et al. 2002). Instead, as I describe in more detail below, open-ended, long-term ethnography is a key driver for iteratively producing research questions, collecting data, and interpreting results.

1.2 Building Bioethnography

In 2012, I began observing and participating in ELEMENT staff meetings and spending time in ELEMENT laboratories at the University of Michigan and observing interactions between project staff and project participants in Mexico City. In 2014, I obtained funding from the National Science Foundation and the Wenner-Gren Foundation for a three-year project titled "Mexican Exposures (MEXPOS)," involving twelve months of ethnographic research in Mexico City and two years of follow-up work. The title of the project reflected my interest in expanding the concept of exposure ethnographically by examining whether and how both participants and project scientists (including myself) theorize and experience exposure (Roberts 2020).

Over a twelve-month period in 2014–2015, I carried out ethnographic work with six ELEMENT participant families in two different working-class neighborhoods in Mexico City, with the goal of eventually combining my ethnographic work among these families with their biomarker data. I paid attention to what mattered within these households: economic conditions, built environment, hospitality, religious practice, beauty, gender, class divides, eating, and bodily states. My familiarity with these households and neighborhoods, developed through wide-ranging, long-term interactions, has allowed me to collect deeply contextualized data about new areas of interest, such as sleep, without much lead time.

My slow exposure to these participants' life-worlds has enabled me to guide ELEMENT researchers who lack this experience. For instance, when researchers were designing a new sleep survey, they were not aware that most participants did not have their own bedrooms. Even though I never directly or explicitly studied sleeping arrangements, I knew that in most participants' homes, bedrooms accommodate up to eight people at once. Although my excessive knowledge of these households slowed down the design of this survey, it made for a better survey. ELEMENT researchers included a survey question about sleep arrangements, and when they analyzed the data, they found that bedroom sharing was associated with lower levels of mental/emotional sleep disturbances (MESD) than adolescents that did not share a bedroom. This collaborative experience also helped us design a new bioethnographic study (described below) characterizing sleep and menopause that does not assume we know in advance what either of those phenomena are for the participants.

Constructing a bioethnographic framework has been slow. Eight years into this collaboration, the scaffolding is only now beginning to take shape. This feels akin to what the chemist and philosopher Isabelle Stengers (2010) calls "slow science," a process that brings together disparate practitioners and practices, whose obligations and hesitancies, methods and politics, all contribute to the making of knowledge. Because my colleagues in environmental health and I are situated in radically different research ecologies with different obligations, combining our methods, data, and analytic strategies is laborious. Combining quantitative and ethnographic data is a task different and more challenging than combining numerical data from different quantitative fields, such as toxicology and neuropsychology. This makes bioethnography especially challenging for STEM researchers, whose funding mechanisms require them to deliver results more quickly than anthropologists.

Cultural anthropology's research ecology, which has allowed me to engage in slow, open-ended ethnographic work, has given me the time to develop three key principles that I bring to the collaboration with ELEMENT, which I illustrate through the examples in part 2: (1) that individuals are not necessarily the most meaningful unit of analysis when households, neighborhoods, nation-states, and political and economic processes like NAFTA shape bodily processes like endocrine response to toxicants; (2) that generating good research questions requires an open-ended, ethnographically inductive stage before narrowing the aperture to a limited hypothesis; and (3) that biological processes are as dynamic and historically shaped as any other process. It's difficult, though, for ELEMENT researchers to absorb these principles because their funding agencies, primarily the NIH, require hypotheses in advance and assume that bodies are basically the same for research purposes and that knowledge should be produced through and about individuals. To carry out our work together, we have had to find other funding sources, like the NSF, that accommodate more open-ended research. But this also creates other challenges because the NSF does not support basic research on human health. Thus, our proposals for new projects are designed to focus on the environment or economic processes rather than health outcomes.

When I returned in 2015 from my intensive fieldwork year with ELEMENT families, I began to gather a team to both theorize bioethnography and put it into practice. It includes a full-time postdoctoral fellow. MEXPOS projects also include undergraduates in our qualitative coding lab and graduate students and early-career researchers from biological anthropology, environmental engineering, nutrition, public health, and political science. Unfortunately, the structure of graduate education in cultural anthropology, unlike PhD work in environmental health and engineering, makes it difficult to include graduate students in team-based multidisciplinary work. If my environmental health colleagues have noted the irony that I insist that they “give up” individuals as meaningful units of statistical analysis while my discipline continues to insist on “rugged” methodological individuality for marketability, they, very politely, haven’t mentioned it. My STEM colleagues have also had to grapple with some of cultural anthropology’s techniques, which are strange to them. For instance, my ongoing participation in fieldwork as a means of maintaining and deepening my inductive analytical capacities complicates how we manage the project. In their world, it tends to be that only graduate students and staff collect field data, not principal investigators. And the fact that I take copious notes about our own interactions in order to iteratively loop our research process into our analytics of knowledge production does not always sit comfortably.[2]

Many key ELEMENT researchers were initially wary of my proposed collaboration. At first, Martha M. Téllez Rojo (Mara), the PI who directs the project in Mexico, regarded my study of ELEMENT participant families as overly vague and small-scale. Later, however, Mara came to call ethnography a *semillero*, a seedbed, for generating new questions for ELEMENT. This compliment allowed me to reflect on how seedbeds are containers, like the bank of biological samples. But while

[2] See (Leighton and Roberts 2020) for an ethnographic analysis of how the different disciplines involved in this collaborative approach phenomena like trust and the contingencies of fieldwork.

the contents of the biobank can easily be analyzed numerically, seedbeds need different kinds of tending to produce knowledge.

Similarly, Brisa Sánchez, the biostatistician, was initially unimpressed with my proposal to study only six ELEMENT families in two neighborhoods. To her, that was no data at all. Later though, it was Brisa who was overwhelmed by my excess of ethnographic big data. Her remark has become a seedbed of its own, compelling me to use this article to develop my thinking about what ethnography brings to our collaboration.

My reflections on Brisa's comment about ethnography as big data are formed through the robust critical literature in anthropology, medical anthropology, and STS on the practices and logics that produce numbers and data within finance (Zaloom 2009), health (Ruckenstein and Shull 2017), and global health (Adams 2016). Scholars tracing the power of the global health assemblage have delineated how numerical data and statistical evidence are virtually the only authoritative coin of the realm. As these scholars point out, however, all of this data is "cooked" (Biruk 2018), just as all facts are fabricated (Latour 2010).

For critical theorists of science and biomedicine, then, it's extremely easy to be skeptical of numbers when we fully examine how these numbers are made. A case in point: global health has developed in relation to the constriction of state-funded health efforts and an agenda increasingly set by foundations funded by corporations. Thus, data collection tends to reinforce the market logics of private rather than public interventions (Birn 2014; Vasquez 2020). This funding is increasingly predicated on privately funded finance instruments, like pandemic bonds that trigger payouts only when a certain case count is reached. As Susan Erikson (2019) has documented through her ethnographic investigations of the workings of global health finance, the thresholds for pandemic bond payouts are based on complex algorithms that incorporate data collected "on the ground." But those numbers are provided by underpaid workers, like those in Sierra Leone who frequently hand their cell phones over to truck drivers to give the impression that they are in the field documenting disease counts while in fact they fill in disease counts from home. Both these numbers and the algorithms they feed are shaped by the colonial inequities baked into global health efforts, including priorities set by the powerful, lack of electricity, bad roads, and in-country professionals who need to hustle to make a living. But even if the data were "better," it would not overcome the problems of a funding mechanism by which return on investment, not health, is the metric of success (Erikson 2020).

In response, bioethnography asks, what if we created numbers otherwise, upending the cooked data that reinforces inequality? In fact, bioethnography can enable us to identify structural forces, such as NAFTA and the global health apparatus itself, that are part of the bodily processes that make ill health. In other words, while we know that all data is cooked, it matters *how* it's cooked.

And what then about the relationship of big data to ethnography? *Big data* usually refers to very large numerical data sets that can be analyzed computationally to discern patterns "especially relating to human behavior and interactions" (Pink and Lanzeni 2018) and used for policy, diagnosis, marketing, and surveillance (Zuboff 2019). Some critics of big data call it "dumb data" because it's not accompanied by "big judgement" (Jain 2017, 51), which would situate the data within the social, historical, and political processes that make it. For instance, as Erikson demonstrated, the Harvard researchers who were initially lauded for using mobility data derived from cell phone usage to "see" an Ebola outbreak before anyone else lacked "big judgement." The modelers assumed that each cell phone represented an individual. But in resource-poor sites like Sierra Leone, cell phones are shared widely (Erikson 2018). Ethnographers could have told them this.

Ethnography does have some capacities also attributed to big data. Proponents of big data claim that its all-encompassing nature makes it unnecessary to establish research questions in advance: "No one has to decide ahead of time what constitutes the exact data that will answer a question, they just need massive volumes of collected data from which to 'source' the answer" (Erikson 2018, 319). While this claim doesn't hold up well for big data, ethnography's power to make knowledge comes precisely from the fact that it does not establish a narrow research question first. Instead, ethnography provides a seedbed from which to ask smart questions. It's a different kind of big data.

Brisa has come to think of ethnography's big data as useful for understanding causality, which has been elusive in biostatistics, owing to the many assumptions about directionality that statistical models must make in order to make inference possible (Hubbard et al. 2019; Kreiger and George 2016). Fortunately, when I met her, Brisa had already been working to develop multivariate statistical methods for measuring variables at spatially relevant scales, and she has come to think that the unruliness of ethnographic big data might help with her goals (Sánchez et al. 2017). She now considers knowing "too much" about ELEMENT families and these neighborhoods as a means to build more realistic assumptions into statistical models, enabling more reliable constructs, valid measurements, and big judgment. But, as she points out, while my long-term ethnographic work is a seedbed of good hunches for identifying generalizable patterns that can be harnessed to collect data on a subset of variables, it cannot demonstrate that those patterns have statistical validity. In order for Brisa's research to inform health policy, it is vital to demonstrate through numerical data that these patterns exist. And I agree

Now, across several related projects (three of which I describe below), Mara, Brisa, myself, and our collaborators are experimenting with harnessing our seedbed of ethnographic big data to produce numbers suitable for deposit in the ELEMENT biobank and to make testable hypotheses about associations between health and inequality. Our projects now begin with an open-ended ethnographic stage. The information we gather forms the basis for generating hypotheses, questionnaires, and instruments that narrow the focus of our inquiries and make the numbers. As we analyze the results, we continuously "loop in" our ethnographic big data (Fortun 2012). We call this "making better numbers."

PART II—MAKING BETTER NUMBERS

2.1 Neighborhoods, Not Individuals

From its inception, ELEMENT researchers brought participants into a clinical space for interviews, tests, and biosample collection that measured them as individuals. Researchers aggregate this data to make universalized knowledge about the developmental effects of chemical exposure. When I secured funding for an ethnography of ELEMENT participants, I wanted to expand the focus to a different unit of analysis, neighborhood, that allowed for a more spatial and historical understanding of the effects of chemical exposure.

Mexico City is enormous and geographically diverse. *Colonias* (neighborhoods) designate well-known municipal administrative boundaries and play a robust role in infrastructure allocation and the organization of daily life. Before I began fieldwork, I speculated that neighborhood characteristics might influence exposure levels. And then, after a year of living and working in two working-class neighborhoods with a density of ELEMENT participants and spending time in many others, I returned to ELEMENT researchers with extensive ethnographic knowledge for making the case that neighborhoods could be taken into account in assessing toxic exposures and other health outcomes, and that neighborhoods may increase certain harms while protecting from others, often in unpredictable ways. This knowledge has allowed us to develop bioethnographic methods to test my observations with numerical indicators.

One of the two neighborhoods where I lived, which I refer to as Colonia Periférico, had a bad reputation. It was surrounded by a sewage-filled dam, a freeway, and cement factories, and there was visible public drug use, graffiti, and garbage. The standard neighborhood-effects literature categorizes these attributes as markers of “neighborhood disorder” (Diez Roux and Mair 2010; Sampson 2011). Yet my long-term ethnographic observations came to show that children played on the streets, and residents cared for drug users. Moreover, it seemed that neighborhood “disorder” might in fact protect residents by preventing the entry of violent outsiders, including the police, and also by preventing the post-NAFTA gentrification so prevalent throughout Mexico City (Roberts 2017). The other neighborhood where I lived, which I call Buena Vista, boasted fresh air, beautiful views, and bustling commerce and transportation routes, but land speculation, evictions, water shortages, petty crime, and frequent police violence were common, making everyday life far less secure than in Colonia Periférico.

Before I could incorporate my observations about neighborhoods into ELEMENT projects, however, I needed to demonstrate a connection between neighborhood and bodily conditions. Eventually, Mara and I settled on a potentially answerable question: do blood lead levels vary by neighborhood? If the answer was “yes,” it might be worthwhile for ELEMENT to include neighborhood as a variable when examining health outcomes.

Answering this question proved complicated. First, to correlate blood lead levels with neighborhoods, we needed a standardized and searchable database of study-participant addresses.

Developing this took over a year, because although participant addresses (including neighborhood) had been recorded, they were primarily used to enable project drivers to pick up participants for visits at the clinic and were not standardized. We were able to use geocoded coordinates, gathered by the drivers, to arrive at a more accurate neighborhood designation. In addition, some participants were associated with multiple addresses. We decided to use only one address per participant. Although this simplification involved "bracketing" some potentially useful information about a participant's life trajectory, it led us to make an important new fact: ELEMENT participants live in 338 neighborhoods (more or less). This new knowledge could be used to ask future bioethnographic questions.

With ELEMENT participants sortable by neighborhood, we could pose a question about lead levels, which also took time to develop. Eventually, Mara's preliminary statistical correlations showed that the average blood-lead levels of children in Colonia Periférico were nearly a full microgram per deciliter higher than in Buena Vista and the ELEMENT cohort overall. Furthermore, my previous ethnographic observations proved crucial for interpreting this new information. High levels of toxicants like lead might not only indicate bodily damage but also correlate to the ability of neighborhoods to withstand other, larger dangers, like police violence. This finding is supported by literature in critical anthropology, geography, environmental justice, and urban studies that demonstrates how marginalized populations use toxicity and other boundaries to keep oppressive authorities out (Aguirre Beltrán and Walker 1979; King 2016). In other words, toxicity might prevent other kinds of damage. With neighborhood blood-lead-level data, I had now had a bioethnographic understanding of the kinds of bodily burdens that protective toxicity might entail.

This evidence spurred us to develop two new bioethnographic neighborhood-based projects, one focusing on water (next section) and the other examining the effect of neighborhood dynamics on health. Brisa, Mara, and I are working on proposals to investigate how neighborhood social density (the frequency of spontaneous social interactions on the street) might be integral to the health of working-class neighborhoods writ large. In Colonia Periférico and Buena Vista, I observed that residents were willing to walk farther, despite garbage and graffiti and lack of sidewalks, if they could interact frequently with others along the way. We plan to train and deploy a team of ethnographers to at least twenty neighborhoods to make intensive ethnographic observations about the effects of social density. We would use this data to develop epidemiological survey instruments for testing our social-density hypothesis. In this case, open-ended ethnography has allowed us to propose gathering a more narrow set of observations that will help us understand complex phenomena bioethnographically.

Our social-density hypothesis contradicts many standard assumptions in public health, global health, and urban planning. This literature usually identifies green space, sidewalks, and visual appeal as key drivers of physical activity (Gomez et al. 2015). Critical literature in social science and environmental justice, especially on green gentrification, however, demonstrates that this vision reflects the class position of most public health researchers (Gould and Lewis 2017; O'Guinn et al. 2015; Taylor 2014). The well-heeled tend to value open space and aesthetically harmonious surroundings and regard crowded spaces and graffiti as threatening, unattractive, and unhealthy. By contrast, working-class and poor people appreciate social density for facilitating collective social action and mobilization, especially when resources are scarce. Simply put, researchers may have identified attributes of neighborhood environments that matter more to them than to residents. By incorporating unruly ethnographic big judgement into our study, we will be able to make better numbers and knowledge about what matters for supporting vibrant urban, working-class neighborhoods. Additionally, not only individuals but also neighborhoods can now serve as a unit of analysis within ELEMENT.

2.2. Open-Ended Water Worlds

My year of intensive fieldwork also gave rise to another neighborhood-based investigation, focused on water. By spending extensive amounts of unstructured time in ELEMENT participant homes, I came to realize that despite public health campaigns that exhort Mexico City residents to drink water instead of soda, drinking soda made sense when the water supply is unreliable and soda is everywhere and is reliable, cheap, and pleasurable (Roberts 2015). According to water ecologists in Mexico City, most tap water is drinkable (Espinosa-Garcia et al. 2015), but I observed that most residents don't trust tap water and that in many neighborhoods water supply is intermittent, running only two or three days a week or only at certain times of day. I speculated that part of resident's distrust arose from their general distrust of government institutions.

My observations about the complex and fraught reality of water in working-class neighborhoods have become the basis for an ongoing bioethnographic study, "Neighborhood Environments as Socio-Techno-Bio Systems: Water Quality, Public Trust, and Health in Mexico City" (NESTSMX). NESTSMX combines ethnographic, environmental health, and environmental engineering methods to better understand the discrepancy between health messaging about the benefits of water when city residents don't trust it. So far, we have found that kinds of water intermittency matter in residents' experience of water as well as in preliminary numerical water-quality measures. Our complex bioethnographic understanding of intermittency has been made possible through our open-ended ethnographic aperture. When the project commenced, we did not know enough to ask good questions. A narrowed hypothesis would have foreclosed the possibility of making better numbers about important phenomena like intermittency.

In NESTSMX, which is funded through the NSF for four years, we collect data for understanding how neighborhood trust or distrust of the water supply is produced and in turn shapes household water management and consumption, how these socio-technological systems shape biological processes, and how these biological processes might in turn shape neighborhood environments. We plan to explore connections between the conditions of neighborhood and household water-supply management, molecular biomarkers (e.g., epigenetic DNA methylation), toxicant levels (e.g., blood lead), and anthropometric outcomes (e.g., BMI).

Slowed by the COVID-19 pandemic, the NESTSMX fieldwork team is conducting what we call "water audits," consisting of three visits in each of sixty ELEMENT households in forty-seven disparate neighborhoods throughout Mexico City. During visits to each household, fieldworkers collect water samples and participant biosamples (urine, saliva, hair), install water sensors for real-time chemical analysis and monitoring of water flow and pressure (Bartos et al. 2018), create household water maps of supply and usage, and facilitate discussions about water, health, neighborhoods, and household organization, with specific attention paid to residents' trust in the water supply.

Even though NESTSMX has a relatively narrow focus (water) and involves very brief household visits from an ethnographic standpoint, it is an extremely open-ended project from the perspective of our collaborators in environmental health and environmental engineering, who are used to creating a much more focused set of variables to test a predetermined hypothesis. But so far, we are generating hypotheses, not testing them. By any ethnographic standard, we simply don't know enough to test a meaningful hypothesis. Our first objective is to gather enough ethnographic, water-quality, and biomarker data to understand the realities of water supply and use in working-class neighborhoods and households in Mexico City. Generating big data in the field will enable us to ask the right questions later on.

In several household visits, the NESTSMX team was joined by a nutritional epidemiologist connected to ELEMENT, Jose Antonio de la Rosa, who supervised us in measuring residents' blood pressure and blood lead levels. Jose Antonio's first household visit with the team illustrated some of the differences between standard epidemiological methods and our water audits. He was accustomed to survey methods in which a single researcher leads a participant through a series of predetermined questions with the answers assigned numerical values. After the visit, Jose Antonio, seemed worried about the fact that other family members had been so involved with the visit. We explained to him that we wanted as many household members as possible to participate in these conversations. During that particular visit to an ELEMENT mother, the participant's sister-in-law had been cooking as we carried out the audit. She started teasing the participant about how she prophylactically medicates her husband and children every six months for parasites they might have ingested from food or water. Although the other families in the compound joked about this habit, the participant was not defensive, maintaining that it was what her own mother had always done. This open-ended conversation gave us knowledge about household water practices that we might never have discovered otherwise. Interactions like these will allow us to design more sensitive surveys that can ask and answer the right questions on a larger scale than most ethnographic investigations.

This open-ended style of interaction and observation has also suggested how an intermittent water supply influences water use. Some preliminary data indicate that distrust of tap water does not stem from distrust of government, as I had initially conjectured. Some residents do in fact trust the municipally provided water, assuming it's drinkable when it arrives to their household. Their distrust kicks in after they stored it themselves. Although storing water is necessary in many households because of the erratic supply, the longer it sits, the greater the likelihood of contamination.

These findings strengthen my earlier ethnographic observations about public health messaging. If the intermittent nature of the water supply leads to contamination during storage, public health encouragement of water consumption instead of soda, and the stigmatization of soda drinkers, might be a harmful intervention. The damage is not necessarily caused by bacteria or parasites but by public health campaigns. And, in fact, working-class people seem to know that their household water infrastructure makes drinking water a risky proposition, which is perhaps why some women regularly medicate their families for parasites.

Additionally, we are learning to ask and answer better questions by combining different kinds of data. During the audits, we found that whether a household has continuous or intermittent water can't be answered with a simple "yes" or "no." How household residents experience their water supply is complex and can differ from how they are designated as intermittent or continuous by the municipality. Through carefully examining our ethnographic data, we have determined that thirty-six of the sixty NESTSMX households have continuous water from the municipality. In some of these households, though, there are variations in water pressure at different times of day, so the supply feels intermittent. The other twenty-four households receive water intermittently, but *intermittency* varies there as well. Some households receive water only a few hours a day. Other households receive water only a few days a week, but the supply can feel continuous if they manage their water carefully.

Our use of water sensors in nineteen households has helped refine our understanding of intermittency. The environmental engineers on our team consider adequate and consistent chlorine levels to be the key index of water quality. Chlorine decays over time, so the longer water sits, the more susceptible it is to contamination. The engineer's preliminary read of the sensor data indicates that in neighborhoods with daily intermittency, household water tends to have a narrower chlorine variance, ranging from 1 to 2.5 milligrams per liter, than households with weekly intermittency, ranging from 0 to 2.5 milligrams per liter. In fact, in terms of consistent and adequate chlorine, daily intermittency looks similar to a continuous water supply.

This finding has implications for designing water distribution systems in the face of increasing scarcity. If interruptions in supply are necessary, daily intermittency may be preferable, in the sense that it results in more consistently chlorinated water. However, our ethnographic data demonstrates that some residents perceive heavily chlorinated water as "bad." Thus, even if we work with neighborhood residents to advocate for continuous water or daily water intermittency, we can't automatically assume they will be willing to drink it.

These numbers showing how different kinds of intermittency matter were made by combining inductive ethnographic methods attuned to the specificity of phenomena in time and place and environmental engineering methods for testing water quality. They were not formed through a predetermined hypothesis or through global health priorities that define what will be counted. When completed, our NESTSMX bioethnographic fieldwork and subsequent analysis will provide better knowledge for producing meaningful hypotheses for generating even better numbers about the complexity of working-class water worlds in Mexico City. We might also have created data that could aid in designing more equitable water-distribution systems in water-scarce megacities beyond Mexico City.

2.3 Aging, Employment, and Dynamic Bodily States, I am now in the early stages of yet another bioethnographic project with ELEMENT researchers, investigating potential relationships between aging, chemical exposures, sleep, and menopause in Mexico City. While this might seem like a departure from neighborhoods and water trust, it connects to ELEMENT's focus on the long-term effects of chemical exposure, illuminates how open-ended bioethnography first deploys ethnography to generate questions without narrowing topics in advance, and links to my own long-term interests in understanding reproductive processes like menopause as dynamic and situated bodily states linked to political-economic processes (Roberts 2012). We plan to conduct ethnographic research that establishes relevant questions about sleep, menopause, and chemical exposure in working-class households without assuming in advance that we know what these phenomena are or how they are connected. As I will describe below, this goal continues to be easier said than done, especially since biomedical researchers tend to assume that bodily phenomena are stable across time and space (Lock and Nguyen 2010)

By 2015, the pregnant women ELEMENT recruited between 1994 and 2004 ranged in age from their thirties to early sixties. This provided an opportunity for ELEMENT researchers to examine the relationship of chemical exposure to aging, with menopause framed as a critical and plastic period—just like infancy, adolescence, and pregnancy—during which chemical exposures might have greater effects than at other times of life. In 2017, ELEMENT researchers conducted a pilot study with one hundred participant mothers about menopause and chemical exposure. I accompanied some participants on study visits. I also began to stage conversations about aging and menopause during regular visits with the six MEXPOS participant families, sometimes in the company of Erica Jansen, an ELEMENT nutritional epidemiologist interested in sleep and aging. So far, we have had open-ended conversations with eighteen women (ages seventeen to seventy-three) in their homes, with many more people adding to the conversation. When COVID-19 subsides, we plan to work with larger sample of ELEMENT participants to understand aging, menopause, and sleep in extended-family households.

A conversation during an ELEMENT research meeting in 2018 reinforced, for me, the importance of foregrounding the situated dynamism of bodily processes. The meeting's goal was to reassess the initial pilot study questionnaire in preparation for expanding the aging study to the participant mothers in the larger cohort. At one point, the project neuropsychologist, Christine Till, put forth a speculative hypothesis about menopause.

> It might be pie-in-the-sky, but I'm quite interested in how bone fluoride is changing in menopausal women. I don't know if there's any way to measure bone fluoride or even urinary fluoride in these women. Maybe we could correlate urinary fluoride to memory loss.

Christine's inspiration came from a study of fifty-five adults in Ontario that found, among other things, that bone fluoride was lower in menopausal and post-menopausal women than in all men and in younger women (Mostafaei et al. 2015). Understanding her hypothesis, that high rates of urinary fluoride will correlate to memory loss in menopausal women requires an understanding of how bone absorbs and stores calcium and chemicals with similar affinities, like fluoride and lead, as well as the importance of calcium to metabolism. Without sufficient dietary calcium, calcium and these other chemicals leach from bone into the blood and then into the urine.

Christine was suggesting that the drop in estrogen levels starting at menopause might cause calcium and fluoride leaching from bone. She suspected that as a result, menopausal women's plasma fluoride levels would rise. Since fluoride is increasingly associated with lowered or impaired cognitive function (Bashash 2017), then lower bone fluoride, or higher urinary fluoride, might be related to memory problems in menopause. Christine's hypothesis was founded on her deep knowledge about specific biological processes and the relationship between fluoride and neurological outcomes (Green et al. 2019). Christine went on to raise alternatives to her hypothesis. Higher levels of fluoride in urine or plasma might result not from bone leaching but instead from "behavior," such as menopausal women drinking more black tea (which is high in fluoride) than other populations.

It seemed to me that Christine's hypothesis opened up new avenues for linking environmental chemical exposures to bodily effects, but it also assumed a link between memory loss and menopause in women everywhere. That assumption did not jibe with what I knew about ELEMENT participants. When I talked to menopausal women in ELEMENT and their family members, they described a consistent set of symptoms: hot flashes, crying, anxiety, depression, headaches, and insomnia. But not a single person mentioned memory loss. Likewise, I have found no mention of memory loss in the epidemiological literature on menopause in Latin America and Mexico (Leidy Sievert and Espinosa-Hernandez 2003; Malacara et al. 2002).

ELEMENT participants also expressed the view that women are entering menopause at a younger age. Older women told me they experienced menopause in their late forties and early fifties, some even in their sixties. The younger women I spoke with told me that menopause occurs in the late thirties and early forties. This potentially earlier onset made me wonder if there is a connection to the well-documented worldwide drop in age of first menarche (Biro et al. 2012). Additionally, we could speculate that if women are experiencing menopause earlier and don't experience memory issues, this might indicate that memory issues have more to do with age at menopause and not the menopause process.

Pharmaceutical company advertisements for menopause medications might also elicit awareness of earlier menopause symptoms. But to complicate things even more, I have found that

ELEMENT participants and their families do not medicate menopause symptoms. This strikes me as strange because, after spending years with these families, I know that they consume what to me seems like an astonishing quantity and variety of medicines: antibiotics, home remedies, tonics, and antidepressants for a multitude of ailments. So why is there no enthusiasm for medicating menopause? Perhaps, for these working-class women, hot flashes aren't considered worth medicating. In talking about menopause with Antonia, one of my long-term interlocutors who works cleaning houses, she laughed and pantomimed mopping her brow with her shirt during a hot flash while also mopping the floor of her employer's home. Maybe mopping up hot flashes while mopping a floor is expected and doesn't require treatment. And mopping a floor in house-cleaning clothes while having a hot flash is a different experience from sitting in an office and suddenly becoming drenched with sweat while wearing a dry-clean-only blouse. In a similar vein, because few ELEMENT women have professional occupations, the kind of mental fog some professional women experience might not be a problem for them. Cleaning houses might require different kinds of cognitive processes than working at a computer.

To bioethnographically understand the relationships between memory, fluoride, work, and menopause, then, we would need to map out differences like these while also avoiding classist assumptions that valorize "professional brain work" over domestic labor. We would also need to attend to the dynamism of chemical landscapes. What Christine labeled "biology" is made through government health policy: nearly 70 percent of Ontario's tap water is fluoridated, compared with 0 percent of Mexico City's. In Mexico, fluoride is added to table salt, toothpaste, and sometimes milk, but not water. And with respect to her speculation about "behavioral" diet patterns, Mexicans, even middle-aged women, don't drink much black tea, although compared to the United Kingdom and the United States, foods and beverages in Mexico have a higher fluoride content overall (Luna Villa et al.

2018). All of this means we would have a lot of work to do in trying to understand the relationship between menopause, chemical exposure, aging, sleep, and memory decline in Mexico City.

A bioethnographic approach illuminates why continuing to distinguish between nature and culture, and biology and behavior, is not useful. The paper about fluoride's effects among women in Canada did not address memory loss. It was Christine who made the hypothetical connection. Researchers, including me, make speculative leaps like this all the time. Speculation is extremely important for asking questions, and we all make assumptions as we speculate. This particular leap assumed that the exposure environments and bodily processes of ELEMENT participants are similar to those of ELEMENT researchers, so memory loss is part of menopause.

Christine's hypothesis prompted me to envision my own "pie-in-the-sky" vision for how we might design bioethnographic projects within ELEMENT. First, Christine reads a paper about measuring bone fluoride in Canada and wonders if bone fluoride might link menopause and memory loss, because she associates menopause with memory problems. She proposes testing this on the ELEMENT study population. At the same time, she might remember that menopause might be different for working-class women in Mexico than what she knows for Canada. Is memory and menopause connected for women in the ELEMENT study? Christine would then ask the ethnographer, who happens to be marinating in a luxuriant seedbed of ethnographic big data about ELEMENT participants, whether the ethnographer has observed any connection between menopause and memory. The ethnographer's complex but nonnumerical response might overwhelm Christine. But this response would not necessarily derail her from trying to establish whether there is a relationship between fluoride, menopause, and mental fog. Ultimately, this data would allow us to ask and answer better questions and make better numbers and knowledge together.

Admittedly, there is actually nothing simple about the process I envision. It would be a radical departure from standard practice if life scientists took time to de-universalize their own bodily experience and situate their subjects through ethnography before honing their hypotheses. That's why Brisa buried her face in her hands. She might have thought I wanted to shut down inquiry around menopause, cognition, and chemical exposure because the density of my knowledge about the participants made it impossible to ask narrow research questions. But I don't. We can and should study the adverse effects of chemical exposures bioethnographically.

And, indeed, when Christine read a draft of this paper, she quickly envisioned a numerical measure that could attend to women's dynamic chemical and economic landscapes and the relationship of class, gender, and memory to aging. Absorbing my description of ELEMENT participants' lives, she agreed that women would be less likely to report memory problems if these were not affecting their daily lives. Then she imagined memory tests among a statistically significant sample size of women who all work as domestics, comparing those with higher and lower burdens of chemicals like fluoride.

I deeply appreciated Christine's response. In her new scenario, she no longer centered menopause, instead making associations between chemical exposure, age, and gendered labor. To take up her response seriously, I might need to agree that we can place women in categories of high or low chemical exposure. Creating that threshold would be a complex process that would involve bracketing out some ethnographic big data that I would consider relevant. Nevertheless, Christine's reimagined approach, foregrounding the specificity of economic activities in working-class households in Mexico City in relation to ELEMENT data, demonstrates how ethnographic big data can help researchers pose different questions to make better knowledge about the effects of chemical exposures. And, crucially, making better numbers about chemical exposure that are ethnographically trustworthy makes better anthropological knowledge about everyday life, including bodily experience, in unequal environments.

CONCLUSION

In 2018, ELEMENT's general project manager, Laura Arboledo, an epidemiologist, visited Mexico City to understand more about data collection and biosample management. While she was there I took her to visit some ELEMENT neighborhoods and families. Back in Ann Arbor, Laura reported feeling overwhelmed.

> When I went to the neighborhoods, I started questioning everything. Wow, we're getting all of these numbers, and we have never been to the communities. So, you're relating a bunch of numbers to something that *you've never seen*. After I came back, I didn't trust the [ELEMENT] data! And I'm in the lab reading the papers and worrying that if I keep that train of thought, I can't *do anything*.

Laura then described realizing then that team had to trust each other's numbers "so we can move on."

While Laura's recognition that standard epidemiolocal approaches might simplify reality (making numbers about communities that researchers had never seen) is somewhat gratifying, I find it urgent to address her sense that knowing "communities" makes it impossible to "do anything." The slow science of bioethnography does not demand paralysis or descent "into a mire of doubt and criticism" (Pigg 2013, 128). It does, however, insist on long-term, labor-intensive "sitting," which is also a form of "doing" that, as Stacey Leigh Pigg (2013) argues, can counter the normalized moralized global health ethic of speed and efficiency. Bioethnography involves taking the time to know how places and people are situated and to facilitate what Laura knew to be crucial—for researchers to trust each other enough to transform the way they make knowledge.

It's taken years for ELEMENT researchers to experience ethnography as worth slowing down for. And in these years the ethnographic MEXPOS team has had to speed up. We have had to learn to work collaboratively and at a quicker pace. We have had to learn what kinds of information to bracket for now. Most challenging of all, we have had to learn to tame, then harness, the excessiveness of ethnographic big data, turning my unruly seedbed of knowledge about neighborhood dynamics, toxicity, and women's lives into clear questions that can be asked and answered in numerical terms. Understanding how neighborhoods shape bodies has required bracketing some of the dynamism of ethnographic observations—for instance, by limiting participants to one residential address. But this limitation has led us to new findings about neighborhood diversity, toxicity, and vitality. To produce numbers that may demonstrate a link between water availability and bodily states, we visit more households for a shorter time than is ethnographically ideal. To construct a means to link chemical exposure to gendered labor, ethnographers and epidemiologists would have to agree on well-defined thresholds of chemical exposure. These compromises are part of constructing a bioethnographic approach that integrates ethnography into the complex work of making legible numerical knowledge about environmental health.

This process can be uncomfortable. Critical cultural and medical anthropologists aren't usually willing to narrow their aperture of engagement. Nor do we typically become complicit in making numbers, because we know that no matter how fervently critical theorists of science and medicine might insist that all numbers are cooked through contingent relations, they tend to be treated as autonomous things by those who make them and policy experts (Nelson 2015; Poovey

1998). We also know that, like any tool, if bioethnography is successfully applied, it will become an apparatus that shapes the reality it measures (Barad 2007), which is an enormous responsibility. In multidisciplinary bioethnography, it will likely be the anthropologists who have the responsibility for keeping numbers tethered to the "big judgement" of ethnographic big data—for instance, by acknowledging that the toxicants that pollute a neighborhood and harm its residents might also play a role in protecting them from police violence. It's our "big judgement" that will be essential for ensuring that the intra-active complexity of dynamic bodily, neighborhood, and geopolitical phenomena informs the better numbers and the better knowledge that we and our collaborators *slowly* produce together.

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