

CONCLUSION

ANTHROPOLOGY IN AND OF (CRITICAL) GLOBAL HEALTH

This book has considered data's social lives, focusing on how quantitative data reflect and cohere the social worlds from which they emerge. In tracing data's life course—beginning with the formulation of the survey in the office through the collection of data in the field and ending in the downstream sites where data aspire to become evidence—I have centered the many actors who help data along their life course, with particular focus on the knowledge work and expertise of fieldworkers. In the process, I have attempted to problematize assumptions of researchers from the colonial period to the present that fieldworkers are merely instrumental and interchangeable, unskilled cogs in larger research infrastructures. While such representations cast fieldworkers as unreliable and prone to mistakes that threaten to mess up or dirty data, I argue it is the innovative, ad hoc, and important body of expertise they develop as they live from project to project that makes research work.

Cooking data, usually leveled as an accusation against those who occupy the lowest rungs in survey worlds, presumes its opposite: raw data. This book has suggested that data that are clean or free of any social and cultural impuri-

ties is a fiction, but one that nonetheless undergirds demographers' dreams of high-quality data. From survey design to data collection to presentations of statistics at conferences or in policy, assembling data that will eventually become evidence is a process that reflects and reproduces demographers' culture and values, and their interest in clean, high-quality data. Cooking data, however, is a figure that helps us better understand survey research worlds as a space where liberties and necessity overlap, where standards for data collection are reinvented and modified, and where data come into being and gain meaning. Taken together, the chapters of the book show that survey projects I spent time with in 2007–2008 succeeded: by their own standards, they are good projects that managed to produce data evaluated as high quality upon completion (Krause 2014). Further, these data will inevitably come to justify more research projects in the future. Global health projects march onward, largely taking as their justification that no one can be against improving human health outcomes or reducing mortality from preventable diseases. Global health is reproduced—in its own and in popular narratives—as a progressive movement rooted in good evidence and with benevolent intentions.

Like many of its contemporaries in the genre of critical global health studies, this book was conceived with the assumption that the final account would manifest the insights of an ethnographer of global health science skeptical and suspicious of the intentions and politics of global health: it would act as a critique of the kinds of global and universalizing projects that have become anthropological fodder in the wake of Ferguson's (1994) *The Anti-politics Machine*. Yet even as this book has been critical of numbers and survey projects, it does not aim to represent the world better than they do but rather to show what kinds of worlds come about through numbers, raw, cooked, or otherwise. Medical anthropologists proffered their expertise on culture and local people to colonial governments and others to expedite local populations' adoption of biomedicine; the discipline's place in contemporary global health has likewise long been to broker cultural knowledge, to translate between insiders and outsiders, or to give advice on how to make global projects work better (Baer 1990; Scheper-Hughes 1990). In the shift from colonial health to global health, the role of the anthropologist, too, has shifted: from provision of local knowledge or culture as things to be altered or replaced by biomedicine, to provision of local knowledge or culture as necessary context for global health's interventions and science. Anthropologists were called upon, for example, to share their expertise—and attain "relevance"—during the 2014 Ebola epidemic, largely to help bring knowledge of context to global health's urgent importation of templates, logics, and technologies (Beisel 2014; Henry and Shepler

2015; Benton 2017). Yet applying our expertise on culture to real-world problems sits alongside what Eve Sedgwick (2003, 141) terms a “negative” orientation to our objects of study, a critical impulse to expose, for example, global health’s shortcomings, problematic logics, and hierarchies of knowledge, and to foreground its historical continuities with colonial health projects. In other words, for some anthropologists, the enduring call to provide necessary context for interventions or projects may be more worthy of critique than response; being useful sits in tension with being critical.

The rhetoric and form of anthropological theorizing today—and the payoff of ethnographic evidence itself—often presumes the anthropologist’s privileged access to interpretive and critical modes of knowing better (say, about how to improve health outcomes or how to effectively measure this improvement) than the informants: The ideal-type medical anthropologist’s role in global health seems to be to hold tensions contingently together, forge shaky order from them, and say something useful about them. Yet our informants today are not always or only the traditional healers, villagers, or chiefs whom global health sees as its subjects, but rather the clinicians, scientists, intermediaries (such as fieldworkers, community health volunteers, nurses), and health officials who devise and implement projects. Like neoliberalism, global health and its effects have become objects of study and frameworks for understanding our other objects of study in anthropological work (Ortner 2016, 51).

The conjugation of critical with global health necessitates shifts in the anthropologist’s method, theory, and location that have important consequences for knowledge production. It is the easy juxtaposition of anthropology and demography, of anthropologists and global health scientists, and of qualitative and quantitative knowledge that makes critique the purview of the anthropologist. Following Foucault (1997b), critique is a practice invested in maintaining and performing a distance and difference from its objects. This difference is rooted in disciplinary norms for what counts as good data. The anthropologist fortunate enough to have access to grants or other funds has the privilege of slow time: he or she can spend a year or two in the field, while the projects being studied (global health) are constrained by funds that devalue long-term fieldwork, by disciplinary norms for data collection and analysis, and by the questions they are interested in answering. This is evident in the fact that my time in the field was too long to be able to spend time with only one project: in the time I was in Malawi, I was able to do fieldwork with four projects that remained in the field only as long as they needed to collect timely data (a few months each). Survey projects such as the ones in this book are governed by norms of timely data, by the need for standardized

data, and by an investment in clean data. Anthropologists, on the other hand, take their time, celebrate messy or dirty data, and see questions not as conclusively answerable (especially with numerical data alone) but as provocative of new questions. The anthropologist surrenders control of the field, while the demographer seeks to control the field even from afar. Surveys collect data in standardized form; they order it as they digest it into databases. Anthropologists, meanwhile, chew on data for a long time, only ever coming to contingent order after dwelling in their raw field notes.

In general, literature authored by anthropologists of global health embodies critique in two main ways: (1) through para-ethnography or studying of global health experts, logics, and spaces of interventions such as clinics, laboratories, NGOs, humanitarian organizations, and hospitals characteristic of the projectification of the global South (Rottenburg 2009; Wendland 2010; Bornstein and Redfield 2011; Fassin 2012; Geissler 2013a; Whyte et al. 2013; Adams 2016a; McKay 2018); and (2) through fine-grained analyses of the effects or failures of state and other health projects on the ground, with particular attention to foregrounding the suffering and trauma of the world's downtrodden and precarious (Farmer 2004; Biehl 2005; Knight 2015; Wool 2015; Briggs and Mantini-Briggs 2016).

Both strands presume the anthropologist as critic, ontologically reliant on the compulsion to make visible that which dominant systems and practices of intervention, representation, power and knowledge eclipse. This project inevitably reproduces, even as it is conscious of, the temporal and spatial politics of anthropological knowing. Adams and Biehl (2016, 124) suggest that critical global health "begins from the idea that ethnographic methods can highlight the conceptual and practical conundrums arising from contested notions of evidence and efficacy. The 'global' of global health must thus be interrogated as both a political accomplishment and a means of producing other kinds of evidence." Implicit in the call for anthropologists to interrogate the global in global health is, again following Foucault (1997b, 327), a call to detach oneself from it. As anthropologists of this global health, we necessarily produce Others in our projects of critique (Fabian [1983] 2002), and often neglect to explicitly acknowledge the ways in which we, too—as critics—are produced along the way. For those of us who "study up," for example, our difference—our ability to see more or better than our subjects or our audiences—rests on the kind of slow research that gains value in juxtaposition to the fast-paced, sloppy, universalizing, and generalizing imperatives of global health's dominant, quantitative ways of knowing (Nader 1972; Adams, Burke, and Witmarsh 2014). For those of us whose political interests lie in representing

in words or images the suffering of the world's most vulnerable folks, escaping the weight of our discipline's legacy of speaking for others, of complicity with power structures, and of reproducing stereotyped versions of suffering others remains a herculean task (Butt 2002; Robbins 2013; Biruk 2016; Prince 2016).

Like the PowerPoint presentations, policy jargon, databases, and articles from demography journals featured in this book, this ethnographic study finds its place in a disciplinary genre of knowledge whose boundaries are continually reproduced and patrolled by its members. Like the numbers that are the currency of demographers, ethnographic representations gain value because they fit into a particular niche and reflect the values and interests of an epistemic culture that is so intimately linked to our aspirations to be good anthropologists that we may fail to see its operations: this is the blind spot of critique. Though *Writing Culture* (Clifford and Marcus 1986) ushered in an era of anthropologists studying themselves as they study others, the moment of the anthropology of global health is a fitting one in which to consider the social lives of our own data and to ask what kinds of selves we become as we make it. What particularities and dilemmas might the rise of the global health slot in anthropology bring for ethnographic method, theory, and the ethnographer (Biruk 2014b)? What does the invitation of anthropologists to the global health table portend for how we theorize, represent, and value our selves, writing practices, methods, and analytics? How can we maintain a critical orientation to global health's projects without being merely critics—if this is our goal? (Henry and Shepler 2015, 21; Puig de la Bellacasa 2011).

In what follows, I present two vignettes drawn from my field notes, turning the lens on myself and tracing the social lives of my ethnographic data. I take up some of the long-standing concerns of anthropologists—complicity, the field, and usefulness—considering them from the perspective of a contemporary ethnographer of global health. I hope this conclusion, read alongside the rest of the book, might raise some productive questions about doing anthropology in and of global health, and about the state of critique in anthropology more broadly.

Inventing the Field: A Fieldworker among Fieldworkers

John, Victor, and I left the RAM office around midday to map Anglican churches near the border of Mulanje District, inquiring along the way with people we passed. We were attempting to set up interviews for the next day with church leaders and their congregants, and RAM was

in need of more Anglican-identified persons in its sample. The directions people gave us led us to churches that were not Anglican, but we ended up finding one and meeting with the church secretary, where we booked an appointment for interviews with church elders and worshippers two days later. On the way home from the field, we stopped at a bottle store, where we enjoyed a beer and shot billiards in front of a blaring TV playing South African music videos. “Don’t tell Dr. Smith,” they told me conspiratorially.¹

This excerpt from my field notes captures a bit of fieldwork that usually does not appear in demographic or ethnographic representations: RAM supervisors and I take a break from fieldwork, siphoning time from the project without the knowledge of researchers. As I pen this conclusion, I am acutely aware of my affective orientation to the field: nostalgia for time well spent with people who have remained good friends and fond recollection of the adventurous unpredictability of data collection, which never looked the same from one day to the next. In this sense, my feelings resemble those of the fieldworkers whose rhetoric and practices literally create the field from which data will be collected. In this field, I was an object of ethnographic curiosity: I was a fieldworker among fieldworkers, and my presence in the field was a result of the resources and imperatives of survey projects, as much as it was the result of anthropological grants and training.

Having a beer at the Amazon bottle store and many other actions we partook of during long fieldwork days—playing bao with villagers, drinking tea in tearooms, shopping at traveling secondhand clothing markets, listening to or dancing to music emanating from crackling minibus speakers, buying local chicken to cook for dinner, lingering over long lunches of beef and chapatti, reading newspapers—are minor deviations from the order of things and do not appear to muck up or dirty data as they travel their life course. What interests me here is not exposing the ways in which field teams make do and find ways of making fieldwork more bearable, often by siphoning time and resources from projects: indeed, this siphoning is minor and largely irrelevant to the quality of data collected, and researchers often turn a blind eye to it. However, scenes such as these raise questions about the role and relationship of the ethnographer to her subjects, who constitute the slippery entity “global health.” While much of this book has focused on the experiences of middlemen or mediators in survey research worlds, we see clearly in the bottle store scene that the anthropologist, too, occupies a liminal and mediating space in such worlds.

While anthropologists have long deconstructed the politics, affects, and intimacies that influence our accounts and re-presentations, few have explicitly considered how their ethnographic encounters manufacture knowledge, produce theory, and make new subjects (White and Strohm 2014). It is unsurprising that this book has largely narrated the experiences of fieldworkers. While few have closely examined this set of actors (allowing me to carve out a space for my scholarly work), it is important, as well, to note that my sympathies largely lie with fieldworkers: these were the people I spent most days with, empathized with, learned the most about and from, and found it easiest to befriend. It is through such relations, everyday practices, and conversations that I coconstructed the field that is at the core of this book even as I, at the time, felt committed to preserving my field as different from that of demographers and fieldworkers. My field was a space of critique, while theirs, I told myself, was one of business as usual. Yet I found myself primarily among other fieldworkers, who, like me, were engaged in their own critical projects that stemmed from their precarious and ambivalent position within global health's structures. The distribution of critique—as a form of interpretive labor—among different fieldworkers (those working for projects and myself, the anthropologist), however, is uneven. As others have shown, marginalized groups are persistent critics who tirelessly theorize their position in power structures, even if their interpretive labor is less legible than anthropological critique in academic circuits of recognition (Collins [1990] 2008).

The field has long been the purview of anthropologists, a spatial anchor for their trajectory of work, the site of theorizing or generalizing outward, the place where they were insiders, and, most importantly, the place on which they were experts (Wagner 1981 Fabian [1983] 2002; Gupta and Ferguson 1997; Marcus 1998; Weston 2008). Historically, for example, anthropologists are conjoined in narrations of our discipline with their field sites or regional specialty (and job advertisements are enduring artifacts of the persistence of a geographically bounded field). I hope this book has helped to further destabilize this space as a natural or taken-for-granted anchor of knowledge production and, in the process, our modes and methods of inquiry (Faubion and Marcus 2009). My field was very clearly not my own: it was a crowded place of multiple actors and interests in which I found myself entangled for some time. While such entanglements are not new, the nature of ethnography amid and within global health perhaps makes more visible the ethnographer's reliance on and complicity with the people, resources, logics, and technologies that make up the "global" she critiques (Street 2014). During my time with MAYP, for example, I resided in a house (that also served as the field

office) paid for by MAYP with a demographer who was leading data collection as my roommate. As I saw how the field became a rhetorical container of culture and difference for the demographers and fieldworkers I spent time with, I also recognized that my metamethodology—following along with projects—came to likewise produce and legitimate this unit of knowledge production. As Simpson (2016, 327) reminds us, the roots of anthropological methods lie in the ethnological grid, the kinship chart, and other categories that contained and controlled difference to make it manageable for their needs: as an anthropologist among the demographers, too, I walked the same paths and employed ways of seeing similar to those of my fellow fieldworkers, effectively bounding and making manageable the field in which my potential data resided.

Rather than collecting genealogies of rural Malawians or making lists of local plants used in traditional healers' medicaments like my disciplinary (m)ancestors, however, I made marks in red pen on hundreds of surveys, typed up transcripts from focus groups, organized log books, dislodged project SUVs from mud, printed consent forms, helped fix flat tires, helped lead training sessions, and so on. Rather than living in a tent, alone on a beach like our old friend Malinowski, I lived at run-down inns or simple houses, sometimes alone, but more often alongside or with survey projects' staff members. Rather than staying in a single village for a year or more, in the spirit of global ethnography, I followed along with peripatetic projects without losing sight of how such projects reconfigured and remade the people and places they interacted with (Erikson 2011). All of this has consequences for the relationship between the anthropologist and global health today and for what kind of expertise the anthropologist is expected to have and provide. While the aspiration of anthropology to know what is really going on, whether in the spirit of applying that knowledge, of critique or of both, would seem to rely on a different relationship to the field than that of survey teams in the thick of it, what does it mean that the labor and locations of our fieldwork often overlap, are parasitical on, or reproduce the logics and practices of those we study in new and evolving ways (Neely and Nading 2017)? While I would argue that anthropologists have almost always been parasitical on other projects, the potential disjuncture between the kinds of expertise on culture demographers, clinicians, or development workers continue to expect us to possess and the kinds of expertise we feel comfortable sharing illustrates how anthropology's enduring place in the "savage slot" (Trouillot 2003) rubs awkwardly against its occupation of something like the global health slot (Biruk 2014b). For example, while this book has shown in detail how survey projects (which

resemble in some important ways hundreds of other projects operating in Africa) do not so much intervene, treat, or change the contexts they enter into as they coconstitute them, the anthropologist is still expected to provide the kinds of cultural knowledge that can enhance or fit into culturally relevant programs and plans that take context for granted and reify the tropes of local and global. Further, this niche seems to presuppose that anthropologists have privileged access to a truer representation of the local than do others, even as they—in contrast to demographers—have long willingly acknowledged that their informants lie or share information tactically, not unlike some of the survey informants we have met in this book (Metcalf 2002).

On Being Useful

Dr. Payson has asked me to help MAYP prepare a fact sheet to distribute to villagers. She sent the draft via e-mail and asked me to have the supervisors take a look at it and provide feedback on how to make it more relevant to villagers. The fact sheet is one page and contains basic statistical information about those surveyed by MAYP as part of their 2007 baseline survey in Salima. It includes, for example, facts such as “5 percent of young women and men live in households with a flush toilet,” “43 percent of young women and men are currently in school,” and “99 percent of the sample speak Chichewa.” The sheet also contains three graphs. The first is a bar graph showing highest schooling level completed by gender and the other two are pie graphs indicating living arrangements and religious affiliations, respectively. When I showed it to MAYP supervisors, they suggested that the bar graph would not be understood by villagers and that the facts listed could contain more context and interpretation.²

In addition to recording field notes for my own use down the line, I often provided project researchers with feedback I thought might be useful to them. For example, I informed LSAM and MAYP researchers that people in the field (including not only villagers in the sample but also district officials) often suggested they wished to hear back from projects about what they found after analyzing all the surveys. When I accompanied the LSAM field supervisors and American data collection supervisor to the district offices in Mchinji to meet with the district commissioner and introduce the project, for example, he inquired whether LSAM would be sharing the results so “district staff [could] find out what [they found].”³ That same day, when we visited the

police station to inform them of our presence, the officers likewise inquired whether LSAM would tell them what they found. The district commissioner complained that they never heard anything about the results of all this research, even as the projects came back to the same villages year after year. In response to similar critiques, MAYP decided to design a simple fact sheet to be distributed to the district offices and shared with traditional authorities or respondents in future waves of survey research. Via e-mail correspondence with the MAYP principal investigators in the United States, I was asked to provide feedback on this draft fact sheet, in collaboration with fieldwork supervisors. In addition to the supervisors' feedback documented above, I also suggested that the researchers aim to break down the statistics further to the level of neighborhood, since people in the district espoused strong neighborhood identities.

As an anthropologist among the demographers, I felt acutely the need to be useful. I took on a role as a project fieldworker and engaged in the daily labor practices associated with this role to carry my weight and not to be a burden. (I also had to negotiate between being the eyes and ears of demographers and my loyalties to fieldworkers.) In this sense, I helped produce high-quality data—the very numbers anthropologists are rightfully suspicious of. In the field notes excerpt above, we see another way in which I was invited to make myself useful to projects. I helped brainstorm ways that they might more effectively build trust with respondents over time. Yet, having read chapter 3, the reader can infer that what rural Malawians really want from projects is not a mere fact sheet, even if they do say they want to know what the research finds. These calls for results, for hearing back about the data they provide, act as an idiom in which participants express their deeper frustrations with lopsided interactions between wealthy researchers and poor villagers, and with the lack of change they see in their communities. Like the critique of the gift of soap, the call for more information is a symptom of how global health research inevitably reproduces the asymmetries it seeks to redress.

All this said, then, it is unclear whether I can claim making fact sheets as an example of something useful I did in the field. Despite the long duration of my fieldwork and hundreds of pages of field notes collected in 2007–2008, nor can I adequately or quickly answer the simple question demographers often ask when they hear me present my work: “So, what can we do better?” This question reflects the long history of anthropologists' collaboration with medicine and public health, particularly in African contexts, and aims to embrace the nuanced, contextual, and cultural information that is the purview of the anthropologist as a way to improve numbers, fine-tune data collection,

figure out why people are not taking medicines, and collect input from target populations (McKay 2018). Yet thirty years after Justice (1986) provided anthropologists concrete suggestions for presenting their findings more effectively to planners, we continue to fail by others' and our own standards: our work has not really revolutionized medicine, global health, or development. In fact, by these metrics, the critical development studies and medical anthropology literature—much of which has, since the 1990s, documented how grand projects fail—is also an archive of anthropologists' own continued failure to be useful in the strong sense we may aspire to.

Perhaps amid all this hand-wringing about failure and not being useful, the moment of critical global health studies might prompt us to ask not how we might succeed but rather what kinds of rewards the failure to be useful can offer to us as a discipline (and to global health more broadly) (Halberstam 2011). What can we learn from our own supposed failure to be useful amid what Kingori and Sariola (2015) term the “museum of failed [HIV] research,” for example? Global health, demography, and other projects rooted in quantitative data, timeliness, and standardization presume success to be measurable. Anthropology—and in particular its critical relationship to global health—can help us retheorize failure and its relationship to knowledge production. The enduring potential of an ethnographic mode of critique, I suggest, lies in the figure of the fieldworker, betwixt and between, fetishizing neither the convincing logics and success stories of global health, nor overstating the (possibility of) resistance or counteractions of those in its belly. The fieldworker—whether the anthropologist or Malawian data collector at the center of this book—is well aware of the ambiguities and blind spots on which dichotomies like success/failure, global/local, quantitative/qualitative, and outside/inside are built, and negotiates them carefully without resolving or settling them as he or she lives from project to project.

As long as they have been expected to improve or fix misconceived health projects, anthropologists have struggled to escape their disciplinary habitus and writing practices. Margaret Read, for example, was the official anthropologist on the Nyasaland Survey in the late 1930s. Carried out in the very same geographic territory as some of the surveys discussed in this book, the survey was the result of new colonial enthusiasm around systematic, survey-based research into nutrition and its implications for colonial development. The surviving papers suggest its grand ambitions: “The results of the Survey will be of value to everyone interested not merely in the nutrition but in the general welfare of backwards peoples not only in Africa but in all parts of the world” (Berry and Petty 1992, 17). Dr. B. S. Platt, trained in chemistry and

medicine, was chosen to lead the survey in Nyasaland, one of Britain's poorest dependencies. Read, at the time she was invited to join the survey, was in Nyasaland—in the field—finishing independent field research among the Ngoni as part of her course of study in anthropology at the London School of Economics. Read and Platt's relationship was full of tension and disagreement that centered on the former's investment in ethnographic data, as it sat uneasily with the latter's interest in careful quantitative measurement of land held, crops planted, labor expended, and food eaten. The quantitative investments and methods underlying Platt's survey took easy precedence over Read's slower-form anthropological study, which ended up being a wholly unintegrated appendage to Platt's main report (not very useful, we might say). Read's study, like qualitative data today, was framed, in the words of a Malawian sociologist and colleague of mine reflecting on anthropology, as a mere "side dish" to survey or quantitative data, echoing Justice's (1986, 148) informants in Nepali health bureaucracies who saw sociocultural information as soft data. The well-documented tensions between Platt and Read are perhaps a factor less of clashing personalities than of their habitus as a demographer and an anthropologist and their celebratory and suspicious relationship to quantitative data, respectively (cf. Brantley 2003).

The Colonial Office's interest in knowing about rural African nutrition stemmed from a desire for data that described local conditions before initiating development efforts, a precursor to today's evidence-based policy. In fact, Platt's goal as the head of the survey was to utilize the data collected in the service of future development projects. He was enthusiastic about this agenda, proposing a development project in the form of the Nutrition Development Unit even before the problematic data—dirtied by difficulties in measuring crop yields, labor, and nutritional value of foods—were analyzed (Deane 1953; Brantley 2002, 68); Berry, the appointed head of the unit, was never furnished with a copy of Platt's report and lacked access to any of the voluminous data collected, despite his repeated pleas in letters to Platt in the early 1940s (Berry and Petty 1972, 286–289). Considering the ambitions of Platt's proposed development agenda—which included agricultural education, nutritional education, maternity care and dispensaries, fisheries improvement, and provision of demonstrations outside Kota Kota District, among other things—the project was widely considered a failure and left behind few trained personnel and no infrastructure (Brantley 2002, 140–141, 152).

With hindsight, we see that the survey's ambitions to use or apply evidence to stage better interventions were not fulfilled. It left little material mark on

the landscape, but, as Brantley (2002, xiii) found when she revisited survey villages in the early 1990s, villagers recalled a female researcher they nicknamed “Mwadyachiyani?” (What have you eaten?, in Chichewa) coming to fill in boxes and write down things they said. Research, then, even if it fails—in the eyes of anthropologists critical of its logics and forms, fieldworkers critical of its exploitative labor practices, or villagers critical of its failed promises—leaves an indelible, if less visible, mark on the landscapes it traverses. Research participants, fieldworkers, and the anthropologist are made and remade as they interact with data in their various forms. Data come to reflect the people and places they emerge from, and also redirect their imaginations and cultivate expectations. Rather than exposing the failures and shortcomings of global health’s grand ambitions, or revealing the flaws in the evidence its practitioners spend much time and energy collecting, a less negative mode of critique might entail telling some of global health’s other stories, those not only eclipsed by but constituted by the fetishization of numbers that produces more and more projects. Numbers—and the standards by which they are evaluated—not only misrepresent real worlds but make new ones. In this book, I have tried to keep data themselves at the center of the story, without losing sight of the people, places, and things that cohere around them. In this sense, I hope I have succeeded in telling another kind of story through and about medical anthropology in Africa today (Mkhwanazi 2016).

Having spent much time around anthropologists of global health (an anthropologist among the anthropologists), I am struck by our shared culture of critique and the techniques of the self it manifests. We unearth, uncover, unpack, deconstruct, expose, and bring into relief: nuancing has long been the favored activity of the anthropologist, and it is worth thinking with Healy (2017, 121) how nuance itself might be a manifestation of disciplinary virtue and distinction that risks becoming a species of “self-congratulatory symbolic violence,” an aesthetic gesture or in-group performance. Yet amid all of this, we compel ourselves to be useful, presuming too much nuancing to be the opposite of utility. Pfeiffer and Nichter (2008, 412–413), for example, call upon us to be better at reaching our audiences: “In the anthropologists’ traditional roles as culture brokers, we are often better positioned . . . to document and contextualize the effectiveness of health services as they impact people’s lives.” Hemmings (2005, 97) likewise suggests that anthropologists “need to produce evidence that their ideas can improve outcomes” and that “anthropology is failing medicine.” While applied anthropology arguably occupies a marginalized position in the academic hierarchy, where theory is the goal, we nonetheless continue to witness the call for anthropologists to put their

theories to good use—to intervene—especially during times of crisis and emergency such as epidemics, war, or mass displacement (Calhoun 2010).

“Documenting and contextualizing” (Pfeiffer and Nichter 2008, 413) are the stuff of ethnographic method, and the major medium in which anthropology has found a role as a complement to science and established its niche as unflagging critic amid the global health boom, as a review of the growing literature suggests (Packard 2016). As teachers, too, anthropologists speak to increased student interest in the topic and the rise of voluntourism, and find collaborators in the rising number of global health centers on university campuses (Crane 2010b; Wendland 2012; Locke 2015; Sullivan 2016). A brief look at job ads for the past few years indicates high demand for those who can comment on, engage with, or analyze global health, and public health has risen as a core area of a new global health diplomacy (Kickbusch, Silberschmidt, and Buss 2007; Adams, Novotny, and Leslie 2008; Erikson 2008). Medical anthropologists reside in a global health slot from which they circulate critiques and commentary, one that, not unlike Trouillot’s (2003) savage slot, relies on and reproduces the West and the rest, or the global North and the global South, with consequences for which places and people are included in global health’s embrace (Brada 2011; Meyers and Hunt 2014). Africa, as Anna West (2016) suggests, *is* global health. Yet, not unlike Margaret Read’s anthropological study back in the late 1930s, our knowledge often falls on deaf ears: it is clunky, complex, and doesn’t fit neatly into the number-dominated spaces of global health. From the perspective of many of those who work in global health, ethnographic data are “at best anecdotal, at worst insignificant” (Ecks 2008, S77). But how might we provoke ourselves to imagine ways of being anthropological that are not governed quite so much by either the compulsion to critique and/or to be useful in particular ways (Foucault 1997c)?

Echoing Sedgwick’s (2003) observations that the dominant mode of scholarly critique is rooted in a “negative” relation to our objects of study—disavowal, distance, skepticism—Fassin (2012) considers the difficulties of maintaining critical distance for the anthropologist of, in his case, humanitarian governance. Reflecting on his dual complicity with and critique of humanitarian organizations, he calls for a mode of critique that “includes us—individually and collectively—and not one that leaves the social scientist outside [Plato’s allegorical] cave” (246). Similarly, Puig de la Bellacasa (2011, 92) urges us to think of and represent sociotechnical assemblages—such as the survey project—as “matters of care” to counter corrosive critique in the study of science and technology and to engage more intentionally with their becoming(s). She suggests that we not lose sight of how a “critical cut into

a thing, a detachment of a part of the assemblage, involves a re-attachment”; in other words, critical cuts should not merely expose things, but foster caring and reparative relations (97). Caring about the things we critique entails resisting knee jerk disidentifications from them in order to tend to the daily practices through which they come into being. I have aimed to briefly account for a few of the ways in which I—an outsider to survey worlds—entered into, altered, and came to care about certain things and people I encountered in demographic research worlds. The experience was humbling, and close attention to the specific dimensions of doing ethnography of global health can, I think, call into being new forms of critique that are neither wholly inside nor outside, useful nor useless, negative nor positive. Colvin (2015, 102) invites anthropologists to find ways of doing anthropology that are not limited to becoming either a “culture expert” or an unflagging critic of neoliberal science. In a controversial essay, Nyamnjoh (2015) calls for a more thoroughly evidence-based anthropology, writing against what he sees as the discipline’s increasingly salvationist impulses in Africa. As an anthropologist among the demographers, I came to understand my role as a caring critic whose aim is to show how all data, including our own, depend on the underlying framework against which they are evaluated as evidence and made meaningful (Lambert 2009). This mode of critique does not aim to look beyond numbers or to dismiss them (it is doubtful that demography or global health will ever be “without numbers”; Scheper-Hughes 1997), but to take seriously the ways in which they not only measure and claim to represent but also coconstruct the worlds and relations they emerge from. This moment in which anthropologists consensually malign the rising hegemony of numbers in global health and other neoliberal audit cultures seems a particularly apt one in which to take them ever more seriously, and to seek out ways of knowing and caring about numbers, and ourselves relative to them, more deeply.

Raw and Cooked: Coda

Underlying the trajectory of this book has been my interest in opening taken-for-granted descriptors of data such as raw and cooked that circulate in demographic cultures to empirical study. We have seen that raw data are a fiction that nonetheless determine the forms, relations, and practices of survey research worlds. I have playfully written against normative definitions of cooked data as bad or flawed by arguing that it is the innovative and flexible behaviors and practices that take root in research cultures that ensure the production of good numbers. Like the numbers we take for granted produced by projects

such as LSAM and MAYP, this book converts raw data (e.g., field notes) into a polished, clean form that obscures the shifting positionalities adopted by the ethnographer that are influential on all stages of knowledge production (Dilger, Huschke, and Mattes 2015). Looking across all scales of research worlds, and especially at overlooked actors such as fieldworkers, helps us to see and understand better how and why numbers gain value and authority.

The potential of ethnographies of global health lies in their ability to challenge some of the dichotomies that underlie its formation and narration: global/local, science/culture, raw/cooked, office/field, and so on. Instead, then, of viewing global health and its local sites or researchers and research subjects (or even anthropologists and demographers) as distinct or autonomous formations that come into conflict or clash, it is useful to conceive of research worlds as contact zones, or social spaces where subjects usually distant from one another are copresent and intersect for some period of time (Pratt 1992, 6–8). Survey research worlds, for example, are places produced by and reflective of actors' investments in clean data. Research subjects, researchers, fieldworkers, policy makers, and the anthropologist are constituted in and through their relations to one another and to data themselves. While this book has been critical of numbers, it does not aim to represent the world better than they do, but rather to show what kinds of worlds come about through numbers, raw, cooked, and otherwise. Demographers and anthropologists might have more in common in the age of global health than we think. Both, to start, are “dependent on [their] [O]thers to know [themselves],” whether those “Others” are target populations partitioned into samples or the demographers and fieldworkers who do that partitioning (Pratt 1992, 4). Anthropological knowledge, too, is implicated in and harnessed to the socioeconomic and political processes we so easily associate with global health (Tilley 2007, 12). The mode of critique employed in critical global health studies must leave space, as well, for explicit discussion of how anthropologists come to know and see themselves as they navigate the same global health worlds as their subjects; we should not lose sight of the dimensions of our own ever-evolving critical subjectivity.

Andrews, a longtime fieldwork supervisor, once told me, “I don’t think there are any [places in Malawi] that have not yet been researched. [Researchers] are everywhere here. Even me, I’ve been all over Malawi doing all manners of things, all different districts, everywhere.” His words validate the sense one gets from reading the growing literature in critical global health studies: that projects are everywhere, parachuting in and setting up shop. Andrews also drives home an important insight of this book: research does not merely

document or shape, but rather produces and coheres new worlds, subjectivities, expertise, and expectations. Tracking these processes should be as much the concern of the anthropologist as showing how global health and its evidence fails. Research makes data, but it makes people too. From the perspective of the field, one thing seems certain: Akafukufuku abweranso! The researchers (including anthropologists) continue to come, again and again.