ONE

THE OFFICE IN THE FIELD

Building Survey Infrastructures

It is market day at Mangochi turnoff in southern Malawi, and the trading center is bustling with activity. Buyers and sellers of *kaunjika* (secondhand clothes), sneakers, vegetables, printed fabrics, and batteries bargain over prices and socialize, creating a low buzz of voices against a backdrop of persistently blaring minibus horns. On a sunny June morning in 2008, I walk a short distance away from the busy trading center. Passing an open-air butcher shop where young men sit beneath a tall tree hung with two goat carcasses, I arrive at a large compound. Surrounded by walls hand painted with bright advertisements for Boom washing powder and Panadol pain relievers, a squat rest house sits back from the open gates: a favored stop for truck drivers, the rustic motel is called Mpaweni, or Other People's Place.

There is no vacancy at Mpaweni. Its rooms have been taken over by the fieldwork teams—American researchers and graduate students and Malawian fieldworkers, data entry clerks, and drivers—of the Longitudinal Study of AIDS in Malawi (LSAM), a cohort study that has collected demographic data in villages nearby since 1998. For the next two months, fieldworkers will

survey and HIV test about a thousand Malawians. From a vantage point in the dirt courtyard, a visitor might not notice that one of the motel's conference rooms has been converted into a makeshift field office. Data entry teams tap at the keyboards of LSAM-owned laptops, manually transferring data codes from the dusty pages of completed surveys administered the day before to a growing database. Boxes of Lifebuoy body soap and Sunlight laundry soap are piled neatly around the periphery of the room, gifts that will compensate research participants for answering the questions that make up this year's twenty-five-page survey. A photocopier and printer whir quietly, printing off endless copies of questionnaires, consent forms, and log forms that will soon be filled in with data and information. Electrical cords snake underfoot, ending in overworked power strips that protect the electronic devices in the room from the periodic power surges and outages so common in Malawi. Parked helter-skelter around the compound are minibuses that carry fieldworkers to the project's sample villages, all within an hour's drive of Mpaweni: one by one, fieldworkers will visit the households where the members of the study sample live.

Mpaweni is the temporary headquarters for LSAM for the duration of data collection fieldwork. In the words of local residents who notice the visitors around town, "Akafukufuku abweranso! [The researchers have come again!]"

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The scene at Mpaweni hints at the massive human and material infrastructure that must be built in order for large-scale survey research to be carried out in a corner of Malawi far from LSAM's home office in the Population Studies Center at an elite research university in the United States. Reams of paper, laptops, and extension cords must be carried to the field from abroad or from Lilongwe; minibuses must be rented to ferry field teams to and from rural households; fieldworkers must be hired; housing must be found for researchers and fieldwork supervisors for the duration of data collection; and green bricks—in 1,000-kwacha increments rubber-banded together—must be withdrawn periodically from cash points to pay the salaries and per diems of fieldworkers employed by the project. Trips to the airport to pick up arriving researchers or imported items, such as weight scales to collect anthropomorphic data and HIV test kits to collect samples from respondents, were a weekly occurrence. Sometimes items such as the test kits would get tied up in customs bureaucracy, necessitating complex efforts to free them. Building the temporary infrastructure of people and things necessary to carry out

peripatetic survey research in one of the poorest countries in the world is a Herculean task.

This chapter shows how planning and designing field survey projects entails imaginative work on the part of researchers who aim to translate standards—conjured in the office—into clean, high-quality data produced in the messy space of the field. Adopting the position of an anthropologist among the demographers, as discussed in the introduction, I first elaborate how the human infrastructure for survey research, made up of foreign and Malawian experts who bring different expertise to the table, is built in difficult conditions. I draw attention to the disparate material and academic investments of foreign and Malawian researchers in data collection, often obscured by the discourse of partnership or collaboration central to development, humanitarian, and global health worlds today (Mercer 2003; Crane 2010b; Watkins and Swidler 2012; Kenworthy 2014; Thoreson 2014; Brown 2015; Gerrets 2015b). In the second half of the chapter, I articulate the epistemological dreams and standards that call into being the infrastructure for data collection in the field. In analyzing debates between Malawian and foreign collaborators around cultural and linguistic translation and the fine-tuning of survey concepts, instruments, and questions, around plans for where surveys should be administered, and around what should be the objectives of research, the chapter excavates the multiple interests and forms of expertise that coalesce in the pages of a survey, even before it is administered to the first household in the field.

The survey questionnaire is the tool at the core of data production and operates as a framing device that aspires to make Malawi visible and intelligible as data or numbers that circulate among demographers or policy makers: "The world appears to the observer as a relationship between picture and reality, the one present but secondary, a mere representation, the other only represented, but prior, more original, more real" (Mitchell 1991, 60). The survey—as the key mechanism of ordering, counting, and framing the division between real and represented—plays a central role in effecting what demographers experience as a good-enough representation of the really real: data. As I show, the questionnaire itself and data practices in the field reflect a fundamental distrust of data on the part of the most diligent demographers, who recursively ask themselves and others, Are the data good? Is this the right question to answer our research problem? Are people lying? Are fieldworkers cooking the data? These questions arise in the prefieldwork meetings discussed in this chapter, but, more importantly, they are a quality of data themselves. If one assumes one can collect data that transcend these questions

and the uncertainty they signal, posing such questions indicates that uncertainty is inherent to data themselves. This chapter's central interest is in how questions, standards, and tools that eventually produce quantitative data that are devised in the office are translated into and for the field. I theorize translation as an ongoing and improvised practice that privileges the epistemic investments of those who design the survey, one that betrays their shared imagination of a cultural Other who will answer their questions, leaves the culture of demography itself unmarked, and prefigures the nature of data to be collected.

From the Office to the Field: The Spatial Politics of Data Production

I sometimes get depressed when I come to Malawi. I'm used to sitting in my office crunching numbers and having the categories be anonymous, not personified. . . . But I'm pretty wedded to coming to check up on things. . . . If you don't come now and then you have no idea what is going on in the field if you don't hover over people's shoulders there. —Dr. Jones, economist and MAYP coprincipal investigator, September 20, 2007

The way you enter the village the first time, will remain in the minds of the people and will also determine the success or failure of your objectives. . . . As a fieldworker [you] should know that the [villagers'] culture has been there for ages. . . . To effectively work with the community you also have to be participative in the community, attend funerals, attend village meetings to show you are not just there to work, but you are one of them. However . . . attending political rallies [is not advised]. . . . You might only be a part of one group thereby losing the other. . . . Refrain from any political gatherings or debates to be part of the whole community. —LSAM Fieldwork Manual 2008

Reading these two texts alongside each other—the first an excerpt from an interview with a Marriage and Youth Project (MAYP) researcher and the second an excerpt from a fieldwork manual designed by Malawian supervisors working with LSAM for many years—I am struck by their shared construction of a place called "the field." Dr. Jones sets up a clear contrast between being in her office "crunching numbers" and being in the field. Implicit in this contrast is an assumption that the office is a clean space for data analysis and tinkering with numbers while the field is a messy place where numbers become people. In the office, it is easy to "forget that the numbers once represented people with real communities and real histories and complex genealogies" (Jain 2013, 36), but this becomes more difficult when researchers like Dr. Jones confront poverty and suffering firsthand on a visit to Malawi.

Jones acknowledges, however, the importance of visiting the field now and then to check up on the activities transpiring there, hinting at their potential influence on the data that wind up in the office. Although when we met she had been in Malawi for only a few days, she asserted her difference from other economists who never set foot "on the ground" (in the field). Her insinuation that things might go awry in the field if one doesn't "hover over people's shoulders" connotes epistemological and structural hierarchies that characterize survey projects: she looks over the shoulders of potentially unreliable Malawian fieldworkers on the front lines of data collection, implicitly acknowledging their ability to mess up or dirty the data to be ferried to the office.

Finally, the distinction she draws between anonymous numbers and categories and personified realities indexes the interest of this chapter in how abstract standards and ideals for clean data translate into the field and hints at how subjective practices in the latter might erode the objective status often granted to statistical data. Across a large body of published work on guidelines, methods, and survey design across cultural contexts, the construction of the field as a place of "difficult geographic topography" rife with "weather and seasonal impediments" and "danger[s]" that threaten to "bungle" a survey is consistent (Pennell, Levenstein, and Lee 2010; see also Bulmer and Warwick 1983). "The field" compels the translation work needed to link standard survey methodology and procedures to "environments of stringent budgetary constraints in countries with widely varying levels of survey infrastructure and technical capacity" (Yansaneh 2005, 5). To manage impediments to smooth and timely data collection in remote or rustic locales, survey projects sometimes selected the sites for their data collection based on their proximity to the office. For example, MAYP's research proposal notes that researchers selected Salima District to administer its surveys because working in only one district (as opposed to several) would allow the field staff to monitor data quality. As an added benefit, Salima is close to the national capital, thus reducing project transport and infrastructure costs. Even before the first survey is administered, then, behind-the-scenes decisions determine and delimit the nature and quality of data to be collected, in this case, via convenient bounding of the sample area.

The Malawian supervisors who authored the fieldwork manual (cited above) meant to provide guidelines to fieldworkers implementing LSAM's 2008 survey similarly construct the field as a place of difference, distance, and complexity. They cast it as foreign to the fieldworkers who will enter it for the

first time and attempt to prepare the teams for the culture they will find there, presumably more pronounced, dense, traditional, visible, and different than their own culture, which, of course, is not recognized as such. Fieldworkers are advised to walk a fine line between being participative and maintaining proper distance from the villagers they will interact with in the field. They are encouraged, for example, to attend funerals and community meetings for the duration of data collection, but discouraged from getting involved in local politics, which might serve to alienate some research subjects and make them less willing to answer survey questions. For Dr. Jones and fieldworkers alike, the field is a place whose uncertainties and stumbling blocks must be imagined prior to fieldwork such that their influence on data quality can be minimized. The talk and practices of researchers and fieldworkers make the field intelligible by inventing it, facilitating their ability to imagine themselves and the data collection tools they employ as translators between the field and the office (Wagner 1981).

Holding steady a vision of the field as container of data facilitates the collaborative effort to assemble high-quality data. Whereas chapter 2 explores this imaginative labor and its entailments specifically from the perspective of fieldworkers, this chapter focuses on how the field undergirds and directs the efforts of researchers in the office to design survey questions, tools, and research plans that self-consciously aim to manage the messiness and unpredictability of the field. Before data are collected, this chapter shows, rural Malawi and its residents must be recast as "the field" and "research participants," respectively, enabling researchers to translate their epistemological dreams into a contained—and manageable—space of difference.

Demographers leading survey projects in Malawi were very clear about the simultaneously marginal and core role that the field played in their research efforts. On the one hand, they agreed that survey researchers "rarely, if ever, step foot in the field" and don't see the fieldwork component of research as important to their work.[1] Dr. Payson, MAYP demographer, suggested that her disciplinary kin tend to "parachute in and out of countries," echoing critiques by anthropologists and others that "the demographer could study a society without . . . knowing much of anything about it. . . . Visits to the country, if required at all, could be confined to short stays in western luxury hotels" (Kertzer and Fricke 1997, 11).[2] Payson suggested that for those who work on survey projects in Africa, doing fieldwork is actually detrimental to furthering one's career in academia: disciplinary norms—and, by proxy, tenure expectations—see a researcher being too heavily involved in the field side of things as a waste of time that could be instead directed toward writing new

research proposals, publishing results, or analyzing data.[3] She was frustrated that her investment in qualitative methods and longer-term fieldwork as accompaniments to collecting numbers was squashed by disciplinary norms and structures. Dr. Canton, a Canadian social demographer leading projects in Burkina Faso, Kenya, and South Africa, echoed Payson's claim that the disciplinary norms of demography disallow long-term fieldwork: "Fieldwork is seen as a vacation; its point is not understood at all."[4]

In such disciplinary renderings, the field becomes a distant and exotic site that is hierarchically situated far beneath the space of calculation, intellect, and analysis that is the office. This spatialization likewise grafts on to the actors who are expected to populate each of these spaces: fieldworkers and villagers in the field and expert demographers crunching numbers in the office. Indeed, the space between these two sites is crucial to producing the kinds of knowledge expected by the epistemic community of demographers: dispassionate, objective, and universally circulating numbers. Dr. Matenje, a Malawian demographer based in South Africa, emphasized the ways in which number crunching simultaneously made him aware of harsh on-the-ground realities and made him feel helpless: "As a demographer, when I started analyzing the DHS data, I realized what was killing people was AIDS. . . . I understand how important that data is, but it just incapacitated me. I couldn't do anything about [the people dying]."[5] Matenje, like Dr. Jones, suggests that crunching numbers in the office—the everyday labor of the demographer—necessitates an emotional distance between himself and distant realities, one that nonetheless compels him to consider the moral implications of his work. Numbers, as portable placeholders for people themselves and stand-ins for human suffering, operate to make realities appear as taken-for-granted givens to be measured or enumerated rather than structurally produced inequalities and suffering to be meaningfully ameliorated.

Other researchers spoke about how their multiple and competing commitments made spending time in the field virtually impossible: those based at academic institutions, for example, suggested they found it difficult to escape for too long from committee work, teaching, or obligations such as chairing their home departments. Researchers based at the World Bank and academic institutions alike mentioned, as well, the difficulty of spending good chunks of time in one field when you have so many fields (and projects) ongoing across sub-Saharan Africa, reinforcing the notion of the field as a bounded and interchangeable data container, delinked from politics, geographic specificity, textured local life, or people themselves (Justice 1986; Pigg 1996). This, of course, departs starkly from the anthropologist's affective attachment to

his or her field site, often cast in disciplinary rhetoric as a peopled site of meaningful friendships and obligations, a lifelong other home, and a place one is politically, morally, and epistemologically invested in. While in both the anthropological and demographic disciplinary imagination, the field is constructed as a distant, different place of roughing it, for the former, the field, and specifically the long time an anthropologist spends there, are central anchors in his or her claim to disciplinary legitimacy (Gupta and Ferguson 1997). In contrast, time spent in the field is, for demographers, largely a liability to career advancement or a pursuit incompatible with their expertise.

At the same time that researchers heading data collection efforts across sub-Saharan Africa acknowledged that the field was a place they rarely, if ever, had the opportunity to travel to, they well understood the important effects that the practices and processes that constituted fieldwork could have on the quality of data collected: hiring "bad" fieldworkers or turning a completely blind eye to fieldwork activities on the ground would result in messy, cooked, or bad data, from their perspective. Researchers invested time and money, then, in putting in place mechanisms that would enable them to monitor data collection activities: short-term visits to Malawi to check up on fieldwork themselves, assigning Malawian research collaborators this surveilling role, and implementing intensive training sessions meant to standardize fieldworkers' behaviors and practices.

While the last of these is examined in detail in chapters 2 and 4, in what follows I show how hierarchies of expertise and structural inequalities inform the kinds of work performed by those who occupy different levels in survey research infrastructure. The metaphors and rhetoric employed by researchers hint at the unequal division of labor: being on the ground in the field has the largest effect on data but—from the perspective of researchers—the activities of fieldworkers are framed as menial labor performed by easily replaceable and interchangeable individuals (see chapter 4). Between the office—here coded as the office at one's home university or the World Bank in the United States, Canada, or Europe—and the field, however, lies the liminal space occupied by Malawian researchers collaborating with foreign-led survey projects. While these individuals by no means visit individual households to ask survey questions, they are expected to more regularly check up on the progress of data collection activities in the field and to manage logistical, technical, and social issues that come up in the course of field research. The local expertise they offer, then, is not in designing research or writing proposals but comes as an additive to a project conceived in a distant office. The hierarchy of the field and office maps on to the kinds of work those at differ-

ent levels of research projects are expected to contribute. These hierarchies are embedded in political-economic structures that privilege the knowledge work that is the purview of Western academic researchers over the so-called unskilled labor performed by fieldworkers. Meanwhile, as we will see, Malawian research collaborators occupy a middle space that is both constructed by and fraught with power and economic inequalities.

Recruiting Necessary Collaborators: Hierarchies within Partnership

In Malawi at the time of my research, the National Health Sciences Research Council (NHSRC) and the College of Medicine Research and Ethics Committee (COMREC)—both local ethics boards discussed in further detail in chapter 3—mandated that research proposals submitted for local review by foreign researchers list a Malawian coprincipal investigator and include a detailed letter of affiliation to a local institution. Research guidelines also provided clear instructions to guide coauthorship of articles produced by research. The contract for collaboration between foreign and Malawian researchers has a wider sweep whereby benefits or resources also flow to the institution where the latter is based.[6] The acting head of the National Research Council of Malawi explained that national review boards were increasingly vigilant about ensuring that proposals submitted by foreign projects put in place solid plans for genuine collaboration; for example, Dr. Jones described how MAYP's initial proposal did not pass review because NHSRC claimed that the institutional collaboration between the American team and a Malawian university was "not meaningful."[7] In response, the team secured a Malawian economist as a collaborator and created a memorandum of understanding (MOU) with a Malawian university that, among other things, specified the number of computers to remain in Malawi once fieldwork ended.

Also, LSAM incorporated capacity-building activities into their proposals: in October 2007, I attended a presentation by a graduate student affiliated with LSAM to a group of thirty-five students and faculty at the University of Malawi's Chancellor College. In addition to providing a PowerPoint tour of LSAM's activities and data collection in-country since 1998, she also emphasized how the continued collaboration between the university and LSAM would benefit the students, including access to LSAM data, a resource center near the university, access to libraries online, an Internet hot spot, and training courses in STATA, a statistical software package. During LSAM fieldwork in 2008, graduate students overseeing data collection in the field led smaller-scale

activities to enrich the skill sets of field supervisors. Field supervisors were invited to wake up before dawn to attend workshops on preparing a curriculum vitae, becoming competent with STATA, writing a cover letter, and so on. Due to the long and grueling hours of fieldwork days, however, many supervisors preferred to sleep during these sessions.

Despite the detailed scripts and guidelines meant to guide collaboration in Malawi, collaborators from both the North and the South generally agreed that collaborative relationships were unequal and imperfect when measured against global health's prevailing rosy rhetoric of partnership. In interviews with researchers based in Malawi and other locations in the global South, it was clear that they recognized their expedient and instrumental—rather than substantive—role as a rubber stamp on foreign-led projects (see also Crane 2010a, 852).

A Malawian demographer and collaborator with both the LSAM and MAYP survey projects, Dr. Kamwendo, put it this way: "I think these days a typical research group is you have one group in the North, maybe someone in the South, but the person in the North brings money to the person in the South. But, the people in the North cannot get the money in the first place without the collaborator in the South."[8] In Kamwendo's words, we note how North-South collaborations often find their connective tissue in money: only if Northern projects secure a local collaborator can they access grant monies and the field in which data will be collected. The general model for sourcing a collaborator—consistent across the four projects at the center of this book and others in 2007–2008—is to make contact with a Malawian researcher who is invited to collaborate; if the researcher agrees, his or her name is printed in the blank space left for "Malawian co-PI" on the cascade of forms to be submitted to NHSRC or COMREC. Importantly, though this process produces Malawian or Southern collaborators as autonomous actors who engage foreign researchers and institutions out of free and rational choice, it also obscures the relative inequalities between the two parties (Geissler 2013b). Prior to the establishment of a relatively informative and comprehensive website to guide foreign researchers, the role of a local collaborator early in the project especially entailed guiding foreign researchers through ethical review procedures and other bureaucracies to be navigated before setting foot on Malawian soil to implement projects. Barring any real objections to the plans for the project outlined in the proposal, the Malawian co-PI takes up a role as a kind of local expert. Notably, however, the co-PI usually takes up this position long after the research study has been conceived and sometimes after it has already been funded.

Following successful ethical review, foreign researchers may make a short visit to Malawi to meet local collaborators face to face and to work with them on prefieldwork tasks such as tweaking the survey, translating questions, or choosing suitable research sites (the second portion of this chapter discusses survey design in more detail). In the contracts drawn up between collaborators, Malawian researchers are granted payment in return for specific kinds of expertise itemized in the budget appended to a proposal: participating in meetings with local research gatekeepers, selecting project supervisors and fieldworkers, assisting in translation and back-translation of questionnaires, spending at least a few days supervising data collection fieldwork on the ground, and generally providing oversight to the foreign researcher.[9] In this list of activities, Malawian coinvestigators are called upon to perform a middleman role. Yet despite their more consistent proximity to the field of data collection for the duration of fieldwork, Malawian researchers often shirk their duty to visit the field sites of projects, a fact bemoaned by foreign collaborators and interpreted as a case of the former failing to live up to their end of the bargain.

Malawian researchers, meanwhile, attributed their inability to participate more meaningfully in collaborative projects to being overworked and overextended by the work of collaboration itself. For example, some collaborators on survey projects were academics based in departments at the University of Malawi or at the Centre for Social Research (CSR), an institutional arm of the university, established in 1965, with its own budget—funded by UNICEF until it was taken over by the government in 1982—whose main function is to house rotating faculty from the university who oversee collection of data for research projects in the national development interest (interpreted loosely). The imperative to undertake policy-relevant research today finds historical corollary in postindependence rhetoric of research in the national development interest. At the conferral of the first degrees earned at the University of Malawi, then-president Kamuzu Banda said in his speech, "Malawi has no time for ivory tower speculation. . . . What the country needs is the commitment of its academic elite to the solution of practical problems in Malawian life" (quoted in Joffe 1973, 517; Hunnings 1981). A 1982 report on CSR's activities, meanwhile, noted that the "Centre has done very little in the way of basic research since staff [faculty members] have been busy with commissioned . . . research projects" ("Centre for Social Research," CSR/16/82) a trend that has been exacerbated by the global health boom. Dana Holland (2006, 128) argues that the creation of centers for the study of poverty or education in Malawi tend to align with donor interests and are major culprits in drawing

academic social scientists further away from the traditional university via de-institutionalization, an observation borne out by my own findings.

In 2007–2008, three faculty affiliates to CSR were each collaborators on upward of ten projects at one time, including, for example, monitoring and evaluation research for UNICEF-funded community-based child care centers, a UN Food and Agriculture Organization study on rural aging and livelihood, and an assessment of how Malawian farmers experienced input subsidies in 2006–2007. As Malawian academics explained it, they accumulated these collaborations because of the small size of the country, the small number of people holding master's or PhD degrees in Malawi, and the high density of research networks through which collaborations were forged.[10] One might argue, in fact, that a rite of passage for academics working in universities in sub-Saharan Africa is becoming skilled at finding those opportunities (conferences, consultancies, workshops) outside the university's walls that can most supplement normatively meager salaries with handsome consultancy fees, per diems, and travel to foreign locations. During dinner at a conference held in Zomba, Malawi, sponsored by a foreign African studies institute in late 2007, the young African academics in attendance—mostly PhD students or junior scholars at African universities—complained that the sponsoring institution had not provided them with pocket money or per diems. A young Zimbabwean historian gave a passionate monologue:

> We live off per diems! We search the Internet for conferences to attend constantly. We make money that way. A number of us are familiar with this one man who presents almost the exact same paper every time he goes to a conference in slightly different form. . . . This guy is a real expert at rewriting his abstract again and again. He tones his topic [drought] toward whatever are the larger interests of the conference in question. Drought and HIV/AIDS orphans, drought and global warming, drought and development [everyone laughs]. That man makes money, let me tell you![11]

This account of a character familiar to others at the dinner hints at the central importance of per diems as supplemental income for African academics, which only intensifies as one moves up through academia from graduate student to faculty member and requires money to raise a family, support less wealthy rural kin, and so on. Living off per diems entails intensive labor that distracts academics from their research and writing, symbolized in the repackaged drought paper delivered at multiple conferences by the character described above. Amid the rise of per diems as income supplements in global

health and research worlds in sub-Saharan Africa (Lwanda 2005; Heimer 2007; Ridde 2010; Conteh and Kingori 2010; Vian et al. 2012), Malawian collaborators on projects such as the surveys are often unable to spend time on the ground as laid out in their contracts. A Malawian demographer and frequent collaborator to foreign-led survey projects, Dr. Chirwa, described why she enjoyed traveling to conferences outside the country: "It is nice to have respite from people knocking on my office door constantly and some time when I can just read my e-mails in peace!"[12] However, as another Malawian collaborator pointed out, constant travel takes a toll on one's mind, body, and intellect. He described 2008 as his "worst [year] yet" amid traveling once or twice a month to diverse locales to interface with collaborators: Pretoria, Johannesburg, London, Norway, Uganda, Geneva, and the United States.[13]

The socioeconomic asymmetries that produce lopsided collaborations between institutions and researchers from abroad and within Malawi were a recurrent theme in interviews I conducted with Malawian researcher-academics, who largely suggested that partnership is little more than a performance (Mercer 2003, 759). A senior faculty member at the University of Malawi and collaborator on survey and other projects, Dr. Mponda, articulated the multiple demands he faces:

> One of the major problems we face is, quite simply, our low salaries. . . . How can I pay for groceries, fuel, my children's school fees? It happens that many older people spend all their time doing consultancies instead of building a solid academic foundation in this country by publishing and researching and teaching. . . . I feel that if we got a little more money we would be more devoted professors to our students and do original research and stop moonlighting on consultancies. . . . We cannot compete for research money at a global level. . . . Proposals for consultancies I've mentioned [e.g., for evaluation of NGO and government projects], on the other hand, are not as comprehensive. If you submit a [proposal] in country, you hear in two weeks [whether you were successful], get the money, and life goes on. The research may not be intellectually stimulating but it pays.[14]

Senior Malawian academics such as Mponda earned a salary at the time of around $500–600 per month; consultancies paid hundreds of dollars per day at the time (Holland 2009). Moonlighting becomes less a distraction than a norm, leaving research collaborators stretched thin and unable to develop their own research interests, especially in a university climate that is not invested in faculty research, and largely devalues the social sciences,

except when mobilized toward applied and technical ends (Swidler and Watkins 2009).[15] To make ends meet, they have become savvy at marketing themselves as experts in multiple capacities; as Holland (2006, 2009) points out, however, their entrepreneurial success is likely inversely related to their academic success.

Thus, while Malawian collaborators are key ingredients in establishing a research infrastructure on the ground, they, not unlike their foreign counterparts, tend to play only a minimal role in the field phase of research, making the labor of Malawian fieldworkers and supervisors central to everyday data collection. Nonetheless, both foreign and local researchers invest much time and energy in creating the recipe or template in the office that will guide and—in their imagination—standardize and harmonize the collection of clean data by fieldworkers in the field. Data need to be imagined as data to exist and, as such, close attention to how they are imagined before the fact can shed light on their material forms as culturally coded rather than given: there are no data behind the various practices that do data (Law and Lien 2012, 366).

Survey design meetings and discussions are a central site in which we can observe how the culture of demography emphasizes and instantiates modes of knowledge production that privilege the comparability of concepts over space and time and the harmonization of methods and modes of data collection (Randall, Coast, and Leone 2011, 220). After briefly describing the nature and intentions of a survey, this chapter considers the politics of translation. While I am attentive to the translation of words and concepts from source (English) into target (e.g., Chichewa) languages, I also analyze survey instruments, concepts, and questions to show that a focus on how respondents will hear or interpret them necessitates the invention of a cultural Other and allows the culture of demography itself to go unremarked.

What Is a Survey?

"Survey" operates as both noun and verb, and, notably, the Chewa term for research is likewise the term for survey (kafukufuku). A survey is, in the first sense, a tangible collection of papers with questions compelling responses (a questionnaire) and, in the second, a method whereby information is gathered from a sample of individuals who are surveyed. A survey questionnaire is a systematic, organized method of gathering quantitative data from a sample of individuals, and survey methodology is largely seen by demographers as a science, where surveys derive data to test hypotheses (de Leeuw, Hox, and

Dillman 2008). Unlike a census—which also relies on face-to-face encounters between an interviewer and a respondent—a survey does not endeavor to measure or count all members of the population, but rather extracts data from a population of interest, the sample.

A sample, or group of people "living at a specific time in a defined region, belonging to a specific societal stratum, sharing specific characteristics, etc." (Mohler 2006, 11), is the anchor for data quality because it is incorporated into algorithms and calculations that determine whether a given data set is good or bad. The quality of data is arbitrated by calculations that measure construct validity, measurement error, sampling error, nonresponse error, processing error, and so on (Anderson et al. 1979; Groves 1989; Hansen et al. 2010). The point of sampling is to economize resources but also to draw inferences from the sample to a larger population of concern through the application of statistical tools that ensure ahead of time that data will be good enough to do so. A larger sample means smaller sampling error, but in places like Malawi, there are often cost and time constraints that act to limit sample size. Further, developing valid constructs and minimizing error enables the standardization of information across countries and regions (Adams 2016b, 28). As an axiomatic category of demographic analysis, the sample must be imagined as a bounded container or a closed population, demographic abstractions or workable imaginaries that make data collection possible (Adams and Kasanoff 2004).[16]

Demographers are invested in rendering complex entities such as the family or the household into standard sets of categories to communicate and enumerate difference across time and space. As will be seen in chapter 4, these standard categories, not unlike the data talk mentioned in the introduction, are part of the cultural parlance of demographers, and, for this reason, intensive training sessions for fieldworkers function to entrain them into a new linguistic and cultural community whose core preoccupation is collection of high-quality data (Higgs 2004). Fieldworkers not only follow a script by reading it off the survey pages to their respondents but must also understand the aim of each question and the meanings of terms and concepts that may be foreign to them as nondemographers.

Ample critiques of enumeration show that counting is never a straightforward, neutral activity; depending on who is doing the counting and why, people may be allocated to different categories, left uncounted, and so on (Prohmmo and Bryant 2004, 245). Analyses and comparisons of different data sets for the same country illustrate this well. For example, UN and WHO projections and household survey-based estimates of the fraction of children

aged below fourteen years who are maternal, paternal, or double orphans in Malawi differed significantly in the early 2000s (Grassly et al. 2004, 210). In Malawi, HIV prevalence differs depending on whether one consults LSAM data or Malawi government data (Thornton 2008). Further, we often overlook the fact that numbers about health in Africa are based on estimates, rather than real counts (Wendland 2016, 65–67). At a finer-grained level, demographers recognize the powerful influence that individual data collectors—their practices, biases, behaviors, and intentions—may have on the numerical output of enumeration efforts even within a single project, as is evident in chapters 2 and 4.

Latour and Woolgar (1979, 49–50) show how, in the space of the laboratory, samples extracted from rats undergo a radical transformation into paper sheets containing figures, graphs, and so on. Designing surveys entails a similar transformation of the real into representation, where responses provided to data collectors become pencil marks on a page and then data points in databases. The survey form, even as it aspires to collect raw data, is a framing device whose apparent objectivity hides its cultural story and commitments (Gitelman and Jackson 2013, 5–6). The finalized field-ready survey is the key actor in an ontological choreography that features demographers' efforts to make data in the same way over and over again amid unpredictable human and nonhuman actors in the field (Thompson 2005).

Survey design is a negotiation constrained by a number of factors: financial resources, the capacity of the organization that will implement the survey, and the willingness of household members to provide the desired information, for example. As they translate survey questions and negotiate the final form of the survey, those present at survey design meetings have in the back of their minds a number of questions: How many households will be sampled and how long will a fieldworker need to spend at each? What will be the costs of training fieldworkers, particularly if the survey employs a large number of complex sections or questions? How long can the survey be before participants grow tired of answering questions? What information will respondents be reluctant to provide or unable to recall? These queries point to how the questionnaire itself can introduce error into the data collection process: Information collected can be ambiguous, not well defined, or inconsistent. The order of questions may affect responses gathered, and as Bledsoe (2002, 330) shows, the thematic order of a questionnaire betrays the chronological naturalism and logics of its designers. Open-ended versus closed-ended questions may produce different results. Even the actual, aesthetic look of a question-

naire may affect the interviewer's mind-set and ability to administer it in a clean fashion.

As implied above, designing a survey with the target population in mind entails tensions between reducing errors of all sorts and the cost of reducing these errors. In this sense, the survey—and data themselves—incorporate uncertainty: their final forms are merely good enough. The next section analyzes survey design sessions to illustrate how demographers' shared notions of good data inform the survey questionnaire, and how the questions and translations aim to predict and mitigate human and other forms of error in the data set.

Designing Surveys: The Politics, Perils, and Possibilities of Translation

On the covered verandah of a lodge in Zomba, Malawi—colonial Nyasaland's capital and the present-day site of the University of Malawi—a team of MAYP researchers sits together on a Saturday evening in mid-January 2008, heads bent over piles of survey papers. As rain pours down, we work late into the night to give a final polish to the questionnaires that will be piloted in a few days. The main purpose of this meeting is to make sure the survey questionnaires are field ready, so we painstakingly review the questions one by one, considering the quality of translation from English to Chichewa and the precision and clarity of the queries. Present at the meeting are a diverse group of MAYP's research collaborators: the American principal investigator, Dr. Payson (a sociologist); a graduate student in economics at a Dutch university heading data collection for a related World Bank project in Zomba; a graduate student in economics at an American university who will oversee data collection for this project; two faculty members from the University of Malawi (Dr. Mponda, an anthropologist, and Dr. Kalenga, an economist); Chifundo (a Malawian fieldwork supervisor); and myself.[17]

Our discussions not only center on accurate linguistic translation from English into Chichewa; we also speculate about how survey respondents will hear (interpret) the questions. Demographers understand translation as a multifaceted endeavor and concern themselves with semantic equivalence across language, conceptual equivalence across cultures, and the ability of a translated text to adapt to local social norms. The imperative to ensure questions are heard in the same way by all respondents to a survey takes form in survey design meetings as collaborative wordsmithing, where changes to the

literal words on the page or to the questionnaire structure are imagined to improve the accuracy of the answers to be solicited and the validity of numerical data. As the late Etienne van de Walle (1993, 124), longtime demographer of Africa, suggests, "small differences in phrasing of survey or census questions can yield extraordinary differences in [meaning]," with implications for data quality down the line. The perils of inaccurate translation are borne out by research suggesting that close attention to standardized translation of surveys, despite its costs, often pays off in the form of higher-quality data (Weinreb and Sana 2009), though some suggest that thoughtful selection and intensive training of interviewers is just as important as translation (Bignami-van Assche, Reniers, and Weinreb 2003). Translation of survey questions from English into Malawi's three primary languages—Chewa, Yao, and Tumbuka—is necessary, as well, to ensure that project interviewers will read each question exactly as written in the local language, increasing reliability of data collected and decreasing noise in the data.

For example, we deliberated over how participants might interpret a question inserted into the section of the survey titled "Social Capital" by Dr. Payson: "Are you comfortable walking to the market alone?" She explained that the question was meant to examine respondents' experiences of intentionality, community, and security; her interest in this measure reflects rising interest in the link between health outcomes and social networks and support in international health research (Harpham, Grant, and Thomas 2002). Chifundo and Drs. Kalenga and Mponda immediately raised concerns, suggesting that a person could go to the market multiple times per week and in a different fashion each time, and, further, that going to the market could never be something one does entirely alone since each time one goes, one meets many people along the way. The question would be misheard by respondents, they cautioned, and generate dirty data resulting from its confusing construction. Ultimately, this question was made more precise by providing respondents with a hypothetical scenario: "If you wanted to go to the market during the day and no one was available to go with you, would you walk alone?" Here, the rephrasing of the question built confidence among the researchers that the answer generated would be more reliable and accurate than the answer elicited by the previous version of the question, which, according to the Malawians at the table, did not make sense culturally, regardless of its linguistic translation.

A few hours later, all of those present at the meeting grew weary after a lengthy discussion about a section of the survey that focused on religion—the Malawians at the table suggested we clarify a question on religious identi-

fication because of people's tendency to switch religions. One of the graduate students argued that we should move on from such "small points": "It's not as if Malawians change their religions enough to warrant all this discussion," he suggested. At this, the Malawian researchers laughed, and Dr. Kalenga explained, "Malawians change their religions all the time! Constantly. We need to spend more time here [on this set of questions] for sure." The elite Malawians present proceeded to joke about how rural Malawians will strategically join different faiths and churches without much thought if they hear that there are benefits (bread, blankets, etc.) to such conversion. Later, Chifundo, longtime fieldwork supervisor, reinforced Kalenga's claim: "When you are talking in English, these things can be straightforward, but in Chichewa [they are not]." Dr. Kalenga's claim not only contests the foreign graduate student's knowledge claim, but also enacts a kind of boundary work that points to his lack of local knowledge and naïveté about Malawi and Malawians (Gieryn 1999). Survey design meetings foreground how the survey frames and contingently aligns the interests of demographers from across geographic and cultural contexts in a form that attempts to mitigate and anticipate deviations or modifications in its translation into the field.

The scene on the verandah foregrounds not only the implicit culturally shared disciplinary norms of demographers, all of whom are invested in collecting data that will achieve the epistemic standards and virtues held in common, but also the different roles of the multiple experts present at the meeting. As indicated above, Malawian research collaborators contribute nominally, if at all, to research proposals or plans. It is when foreign researchers arrive on the ground in Malawi that they take up their primary role on the project as translators who are meant to reassure foreigners that survey questions will make sense to an imagined rural research subject. The expertise they offer entails not only translating English tokens into local vernacular, but also ensuring that the survey itself will act as a sufficiently good recipe to collect the clean and accurate data disciplinary norms dictate.

The roundtable of experts who tinker with the survey design and translation is a presurvey administration ritual in which hierarchical forms of expertise and knowledge are expressed in the debates and discussions of the specific items that constitute the instrument. Mohler (2006, 13) argues that such ritualized meetings often feature the principal investigator acting as "Machiavelli's Principe" with the final say, although in my experience the degree to which this was true depended on the topic being discussed; when it came to questions around linguistic translation, Malawian collaborators were often the chief arbiters, for example. In what follows, I examine how

demographers' commitments to clean data—defined as data that are accurate and reliable, efficiently collected, and collected from sufficiently large, representative samples—are embodied in the categories, queries, and form of the survey itself. Indeed, the final version of the survey that becomes the recipe for data collection is the outcome of a process in which hundreds of decisions—with high stakes for data quality—are made (Glewwe 2005a, 36; Kasprzyk 2005). The questionnaire tool itself carries the dreams and ambitions of researchers into the field and plays a leading role in determining the quality of data collected down the line.

Visualizing Wealth and Health: The Steps Instrument

One question—included in the questionnaire by economists—asked survey respondents to locate themselves on a set of steps (depicted visually on the survey page) based on their perception of their relative wealth within their community (*gulu*) (figure 1.1).[18]

Respondents could indicate verbally or by pointing with a finger whether their family belonged on step 1 (the poorest), step 6 (the wealthiest), or somewhere in between (figure 1.2). This exercise acts as an indicator of an individual's broadly defined quality of life, which encompasses perceived or felt relative wealth. Researchers employ a psychometric tool to convert the subjective judgment or feelings of a respondent (e.g., "I am very poor") into a form suitable for statistical analysis ("I am very poor" becomes 1).

As we considered the merits of this question, Kalenga, Mponda, and Chifundo raised concerns about the translation of the word "community," suggesting that respondents would interpret the term in its current form (gulu) inconsistently and undo both the reliability and validity of the data collected. They argued that the question should be narrowed—that community should be written instead as village (*mudzi*)—so as to elicit the most precise responses and avoid respondent confusion. The Malawians' suggestion to replace "gulu" with "mudzi" was taken up in the version to be read to respondents by fieldworkers. Whereas the former Chewa word refers loosely to a group (used conventionally in forms such as *gulu la akuba* [gang of thieves] or *gulu la anthu ambiri* [a crowd or group of lots of people]), "mudzi" aims to anchor the question in a specific and clear location: the people who live in the area a respondent designates as "his or her village." In fact, the translation that was finally settled upon aimed to specify even further the spatial unit for comparison: *mudzi mwanu* ***muno*** (my emphasis) tacks on the emphatic demonstrative pronoun used to denote precise locality across Bantu languages:

Imagine six steps, where on the bottom, the first step, stand the poorest people in your community, and on the highest step, the sixth, stand the rich in your community. SHOW THE PICTURE OF THE STEPS.		
5. On which step are you today?	6. On which step are most of your neighbors today?	7. On which step are most of your friends today?

FIGURE 1.1. The steps question from the MAYP questionnaire, 2008.

"Your village, the one *here.*" One might read the elevation of the Malawians' translation as best—and its inclusion in the final draft of the survey—as a moment when local expertise trumps or eclipses that of the foreign experts. However, it is important to note that the survey form traveled to Malawi in drafted whole form, underpinned primarily by the theoretical interests of the Western collaborators; linguistic wordsmithing becomes the genre of additive, rather than substantive, expertise proferred by Malawians who are likewise trained as demographers. The expertise they offer in the survey design meeting, however, is logistical and linguistic and manifests in the genre of "add culture and stir," whereby the cultural knowledge of local collaborators is expediently, rather than substantially, incorporated into a fully formed survey questionnaire as well as the research project more broadly.

In its technical nature, it is hierarchically subordinate to the intellectual, theoretically informed expertise that produced the survey in its given form; further along data's life course, the Malawians likewise rarely earn a spot as lead author on journal articles resulting from data analysis. As we will observe in the next chapter, close attention to local expertise in survey design meetings or everyday fieldwork practices helps destabilize presumptions that it is an entity of local origin or implies mastery of the local. Indeed, the rise of a global health apparatus produces a particular kind of commodified local expertise that presumes its context of emergence: lopsided global-local partnerships and collaborations. The production of this form of expertise, in fact, relies on the spatialized difference between the field and the office rooted as it is in imperial geographies of knowledge.

While the content of the question about wealth (its linguistic translation) was deemed a worthy object of discussion in the survey design meeting, the form of the question went unremarked. Neither the Malawian nor the foreign researchers at the meeting questioned the fundamental validity of the steps visual aid or speculated how respondents might react to or interpret the instrument itself. In this sense, among the demographically minded experts

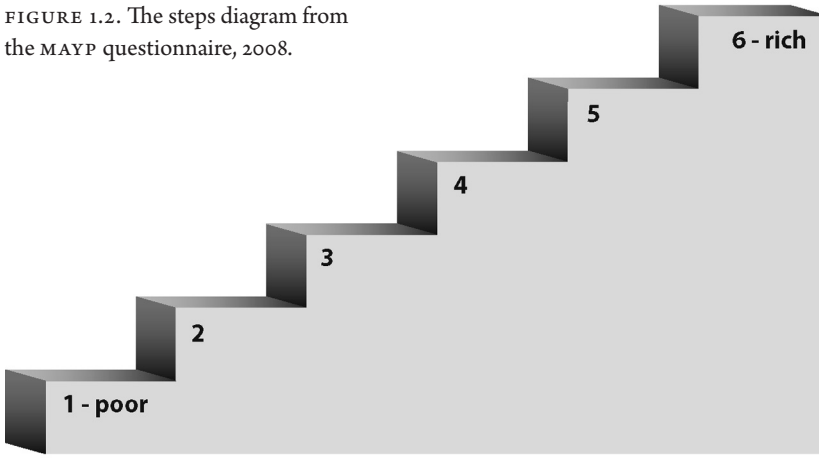

FIGURE 1.2. The steps diagram from the MAYP questionnaire, 2008.

at the table, a shared image of the ideal-type villager set a consensual basis for the form of the question, one that makes clear their own interpretation of the steps as a translation tool appropriate to the target population, takes for granted its validity as an instrument, and foregrounds their focus on linguistic rather than conceptual equivalence (Bowden and Fox-Rushby 2003, 1299). This shared imaginative labor on the part of survey designers—conjuring the villager who will respond to their question—resembles the ways in which filmmakers who produced colonial health education films in Nyasaland and Northern Rhodesia relied on the imagined creation of an audience who would view them (Vaughan 1991, 196). The scalar or metric equivalence of the steps instrument—that is, whether the act of ranking the self on a spectrum is consistent across cultures—is presumed and largely untheorized (Herdman, Fox-Rushby, and Badia 1997, 243). The steps instrument becomes a stabilized fact whose origins and history are obscured by demographers' tacit knowledge that it is a well-functioning, familiar, and routinized metric whose dimensions are not necessary to discuss explicitly each time it is included in a survey (Latour 1987, 43). The steps attain a kind of universal validity through their importation into surveys whose designers hold steady a shared conception of the low-literacy research subject.

The steps exercise likewise embeds certain assumptions about its target audience of low-literacy, largely rural respondents. The question aspires to field readiness in its provision of a visual, rather than textual, prompt. The imperative to imagine a cultural other entails producing forms of difference and ways to manage them that recall imperial, racialized hierarchies of intel-

ligence. Namely, visual-analogue survey tools such as the steps presume text to be read by enumerators is too complicated to be heard properly by respondents, recalling Carothers's (1953, 87) claim in his ethnopsychiatric study *The African Mind in Health and Disease* that the "African mind" lacked logic and capacity for abstract thought. Visual or pictographic scales are often preferred for use in surveys because, as demographers suggest, they are "easy to understand and to handle by the respondents" and carry "low cognitive load" for respondents.

The steps exercise resembles many other nonverbal scales, including visual questions and image-based responses used in clinical and research settings, such as thermometers, ladders, truncated pyramids, symbols, and figures (Smith 2002, 74–76). The most familiar of these visual scales to the reader is likely the FACES pain scale—first developed for pediatric use—that ranges from a smiling to crying face associated with no pain and extreme pain, respectively (Tomlinson et al. 2010). In international research, pictographs and scales such as empty and full pill bottles have been used to measure adherence to HIV drug regimens, and feeling thermometers are used to measure subjective health status. Bolton and Tang (2002, 538) suggest that standard instruments developed in Western countries (such as the Work and Social Disability Scale, WSDS) contain too many "culture bound" questions that are difficult to adapt, citing questions on a respondent's ability to climb stairs or go shopping as examples. To address this, they substituted a nonverbal response card using sketched images of a woman in local dress carrying an increasingly heavy sack and clearly burdened to elicit the same constructs as the WSDS in rural Uganda and Rwanda (539).

Also, LSAM used a pictograph—a health state thermometer—on the last page of a questionnaire administered by its voluntary counseling and testing (VCT) team (see figure 1.3).[19] The accompanying question asked respondents to draw a line to a point on the thermometer that best captured their health status ("indicate how good or bad your own health is today, in your opinion"). The thermometer, however, caused some confusion in the field. For example, respondents sometimes pointed to their weight instead of their felt relative health (e.g., pointing to 50 to capture a weight of 50 kg), leading interviewers to double-check responses to this instrument by asking probing questions or confirming the respondent's choice.[20] The thermometer tool carried with it intertextual references to other contexts in which respondents had encountered scales, thermometers, or measuring devices related to health and well-being, meanings that had the potential to interfere in the collection of clean data.

Similarly, the steps visual aid falls prey to an absolutist concept of wealth as the same across the world (Herdman, Fox-Rushby, and Badia 1997). In the field, the steps exercise faced difficulties—not because people did not understand the words on the page, but because the question of how to define wealth generated confusion. Respondents struggled to pinpoint their location on a continuum of relative wealth, often deliberating aloud about how to assess whether a neighbor was richer or poorer than he: Does he own land? Do his relatives have jobs, and what kind? Did he have a good crop this year? The steps instrument carries with it assumptions that come into tension with Malawian notions of wealth as socially distributed and potentially obscures how having "long legs" or many associates and patrons—wealth in people—might mean a respondent is poor and rich at the same time (Barnes 1986, 78). Further, as Elias Mandala (2005, 14) has shown, rural Malawians do not see feast and famine as mutually exclusive and are well aware that some people are always full and others often go hungry, perhaps confounding some of the questions elsewhere in the questionnaire regarding food stocks and famine. Similarly, in the Gambian context, Bledsoe (2002, 95) shows that survey questions about whether or not women use contraceptives embed assumptions that contraceptive use could only ever function toward limiting fertility, obscuring the tactical ways in which Gambian women use contraceptives as a form of birth spacing oriented toward having the largest number of healthy children.

Analysis of the steps and thermometer instruments embedded in the MAYP and LSAM surveys, respectively, sheds light, first, on how survey designers imagine their research subjects and, second, on how translating surveys is concerned primarily with linguistic dimensions of conversion and with ensuring that tools make cultural sense—from the perspective of researchers—in a local context of administration. Despite demographers' best efforts to predict how questions will be heard (or seen) by respondents, their administration in the field brings many surprises (Nations and Rebhun 1988, 32–33). Yet, even as misunderstanding abounds, the tools themselves retain their status as valid instruments that expediently collect data from respondents. Even if they appear to fail from the perspective of individual interview encounters observed by the author, they succeed from the perspective of demographers whose standards for data already ensure such tools will work. The steps tool, for example, collects a certain number of 1 responses entered into a database down the line that tell us the percentage of a sample of Malawians who identify as very poor. Missing from the number, of course, is the thought processes and discussions that manifest in the interview encoun-

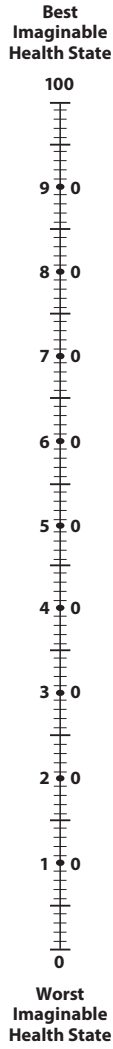

FIGURE 1.3. The health thermometer tool from the LSAM VCT questionnaire, 2008.

ter, or attention to how the fundamental construct (wealth) is interpreted in the Malawian context. In losing sight of these things—in excising them as nondata—the statistical claims enabled by the thousands of data points linked to this instrument become facts and evidence.

Clean data—well-collected raw numbers—contain within them thousands of stories of their messy contexts of production that remain silenced by the narrow definition of demographic data as codes recorded on a page and transferred to a database. Considering the politics of translation in action, it is notable that rhetoric presumes that original survey tools, technologies,

and methods need to be altered for or adapted to both the field and to the low-literacy respondents they will measure. In the meantime, the culture of demography, reflected in its tools and instruments, is unmarked, presumed to be the natural origin or starting point from which translation into an Other's space, language, or culture is compelled. Assuming demography to be naked of culture upholds the fiction, as well, that raw or clean data exist. In casting itself as the acultural original or source language for survey knowledge production, demography obscures, specifically, the deeper tracks that carry data from the field to the office and convert it into fact. As Latour (1983, 155) observes, "scientific facts are like trains, they do not work off their rails." Quantitative evidence collected by surveys takes for granted the existence and validity of one important epistemological anchor: the household.

Statistical Household as Epistemic Anchor

For demographers, the statistical household is the unit of enumeration underlying censuses, DHS surveys, and the surveys discussed herein. Finding and interviewing members of this unit is the primary labor of fieldwork. "Seeing like a [survey] research project" has at its optic core this tangible, visible, and measurable category (Biruk 2012), which became the standard and ubiquitous unit of enumeration in the 1980s (Randall, Coast, and Leone 2011). A barrage of critiques anthropologists and others have leveled against the household as a standard unit of enumeration suggest that it fails to account for patterns of residence, kinship, and economic organization (Yanagisako 1979; Guyer and Peters 1987; Morphy 2007; Randall, Coast, and Leone 2011). For these critics who draw on comparative evidence, households are not bounded groups and fail to encompass networks of resources and support; they are not fixed forms but evolving, and they are differentiated by gender, generation, and so on. While some survey researchers have taken such critiques seriously and attempted to devise a notion of the household that is more capacious and flexible to its various contexts (Kriel et al. 2014; Randall and Coast 2015), demographers have, for the most part, not incorporated such critiques into survey design, even if survey researchers acknowledge that processes of household dissolution, formation, or alteration can result in changes in the representativeness of a sample over time (Deaton 1997, 20).

Van de Walle (2006, xxii) argues that the household is an expedient and preferred category because it is "obvious" to a superficial observer (fieldworkers), even if it is a "necessary evil." He suggests that "the ubiquity of the

household in social, economic and even agricultural studies all over the world may reflect a lack of sensitivity to local particularities of the forms of social organization but it is a fact of life for the analyst" (xxvi). Townsend et al. (2006, 36) likewise suggest that the fact that the household has a physical location and structure presents logistical advantages for survey teams interested in counting and mapping them. Yet, as Kriel et al. (2014, 1317–1319) illustrate, fieldworkers for a household wealth survey in South Africa saw "household" as an external analytical concept developed for the survey, and struggled to translate it into their research practice and to respondents, with implications for who was included or excluded from the sample. Despite the shortcomings of the definition of the household as identified by anthropologists, then, survey researchers are invested in its demographic utility, take it as a fact of life, and require conceptually simple, relatively unambiguous flexible ways to code relations that are comparable across cultural contexts (Townsend et al. 2006, 37, 56), especially in an era when producing readily accessible and standardized forms of knowledge amid "a world of indicators" is a priority (Merry 2011; Rottenburg et al. 2015). The survey itself acts to anchor abstract epistemic standards for data to the key visible and countable unit of demographic knowledge production: the statistical household.

Even as the household determines the movements and practices of field teams, it acts as a flawed placeholder for a more complicated reality on the ground. Fieldworkers struggle to locate households. Once the proper household is found, making sure of the correct respondent is often a problem—especially when the survey is longitudinal and the same respondent needs to be interviewed in each survey wave. Some respondents hide, not wanting to spend time on the survey; others in the household of interest, however, might take the respondent's name in the hope of benefiting from the survey, as we will see in chapters 3 and 4. Naming conventions in Malawi, too, make identifying the correct respondent difficult. A person might go by Gift and Mphatso (Chichewa for gift) alike, or a respondent might take on a nickname he stops using before the next survey wave begins, or he might take on a new name following a religious conversion. And even if interviewers find the correct respondent, they have to negotiate in real time whom they will or will not list in the household roster administered by projects (see appendix). The household functioned as a top-down expedient optic tool that is imagined as a container of valuable data (consider the name "household-level survey"), with implications—discussed in chapter 4—for data quality down the line.

Survey Validity: The Fetish for Codes

Beginning with the very physical unit at which the survey tool is administered, then, anthropologists level critiques against a demographic rendering of the world: the household, they argue, is yet another "demographic category [among other] . . . western folk categories dressed in scientific garb" (Szreter, Sholkamy, and Dharmalingam 2004, foreword). This scientific garb obscures or renders unnecessary that which stands outside of the parameters, measures, and universe of possible responses that capture that which counts as data collected by a survey. Indeed, it is the ability of demographers to narrow as tightly as possible the representation of reality the survey produces that ensures data are of good quality.

The major dimension of data quality is validity, or the extent to which a test (question, instrument) measures that which it is intended to measure. In other words, how well does a question perform in the field? Validity hinges, of course, on adequate translation, as discussed above; for demographers, the objective of translation is to produce the same response from a respondent in a target population that the survey question would had it not been translated from English to Chichewa or another language. Even as this investment in equivalence presumes cultural and other forms of likeness between respondents, it speaks to the epistemic interest of researchers in devising and fine-tuning questions that correspond with what they believe is the true value of a construct: poorly designed questions, for example, may produce reliable results—meaning that all respondents hear them in the same (wrong) way—that are neither accurate nor valid. Further, as will become clear in the following chapters, the validity of a question may be undone in the field via fieldworkers' tinkering with or inconsistent administration of instruments across respondents. For example, one section of LSAM's 2008 survey included a set of ten anchoring vignettes inserted into the questionnaire by a sociologist. King and Wand (2007) and Harkness et al. (2010) describe vignettes as cameo descriptions of hypothetical situations or individuals. These allow for individual assessments to be broadened, ostensibly increasing the validity of data.[21] Vignettes have been used in over eighty countries and deemed efficacious at constructing a common scale of measurement across respondents, mitigating the differential item functioning associated with differences in how respondents understand ordinal response categories of degree (strongly disagree, disagree, etc.; King and Wand 2007).

Underlying administration of these tools are assumptions that respondents assess their own health in the same way they assess that of a fictitious

V4	On some days Grace travels to a larger market in a nearby trading center about 5 kilometers away. She enjoys this trip because there are more goods at this market and she can meet up with friends to chat. She doesn't ask his permission to go to this market because he gets suspicious if she goes to the market alone. How much power does Grace have to travel when and where she wants?	A lot Some A little None Don't know	1 2 3 4 88

FIGURE 1.4. A sample vignette from the LSAM questionnaire, 2008.

character and that scenarios presented in the vignettes are perceived in the same way across respondents. As in the case of the pictographic instruments, the vignettes, too, attempt to address potential cross-cultural incomparability in survey research. Vignettes are meant to describe someone like the respondent, evident in the names assigned to these fictitious individuals (see figure 1.4). In LSAM's case, the skeleton version of an anchoring vignette used to measure women's travel autonomy stages a particularly Malawian scene: a woman named Grace, markets, and a suspicious male lover. In addition to making respondents hear the question correctly, the wide inclusion of these instruments across cultural contexts enables comparability of constructs previously deemed incomparable. In the process of rewriting the bare-bones form of a question—imported to the LSAM survey from external contexts—researchers invent culture as manifest in local names and locally resonant scenarios. Paradoxically, then, vignettes aim to make commensurate the seemingly incommensurable, in line with the culture of demography's emphasis on the comparability of concepts over space and time and their harmonization (Randall, Coast, and Leone 2011, 222). Comparability becomes an end in itself.

Demographers and field teams place the burden of blame for poorly functioning questions on themselves; as Andrews, longtime LSAM field supervisor, told a new crop of interviewers in training: "In research we don't blame the respondent."[22] This phrase was often repeated during training sessions, reinforcing the assumption that researchers (in the office) and fieldworkers (in the field) have control over the quality of data collected through meticulous attention to survey design and translation and standardized implementation, respectively.

Another practice that helps ensure questions' validity manifests in the preferential inclusion of closed questions, what I—an anthropologist among the demographers—in my field notes referred to as a "fetish for codes" in survey research. In LSAM and MAYP surveys, closed questions are the dominant genre of query, illustrating the presumed link between this form of question and data quality. As Glewwe (2005a, 44) advises, "questionnaires should be designed so that the answers to almost all questions are pre-coded." Precoded closed questions leave less up to the interviewer, who, in survey design guidelines and literature from the colonial era to the present, is consistently framed as unreliable, a source of error, unskilled, and untrustworthy. The familiar specter of the interviewer cooking data lurks in the imagination of researchers, informing their investment in the simplest, most easily administered questions with a clear set of possible responses. Closed questions, as well, help ensure that the appearance of a survey is "neat" and "uncluttered," ostensibly making the interviewer's work easier and faster in the field (Casley and Kumar 1988, 72). A code-based survey, however, requires an appendage packet of questionnaire codes to go along with some questions, which in LSAM's case was seven pages long. As demographers are well aware, these codes stand in as representative of a respondent's answers; indeed, the interviewer's action of recording a code on the survey page is the act in which information is transformed into data. Nancy Luke (2006) points to what is lost in this conversion. She examines Kenyan census data from 1989, looking specifically at questions that asked respondents to choose their marriage status from among six possibilities. In western Kenya, where *ter* or widow inheritance—whereby a widow is "inherited" by her deceased husband's brother—is very common, she shows how the expectation that inherited women would self-identify as married was misguided; considering the attention given to ter by policy makers and public health programming, the data, then, likely are not very useful (Luke 2006, 209–210).

Echoing Luke's critique of closed questions, I noted instances across a number of interviews in which the codes provided for LSAM interviewers in the coding packet did not sufficiently cover the responses given by respondents. Section 4 (page 8 of 25) of the LSAM survey "Investment in Children's Education" contained one question that was exemplary in this regard. The section began with the interviewer recording the full names of all children of school-going age living in the household on a roster. Following this step, the interviewer proceeded to ask a series of eleven closed questions—each with corresponding possible codes—about each child named. The last of these questions (D13) asked respondents to mention what they expected each child

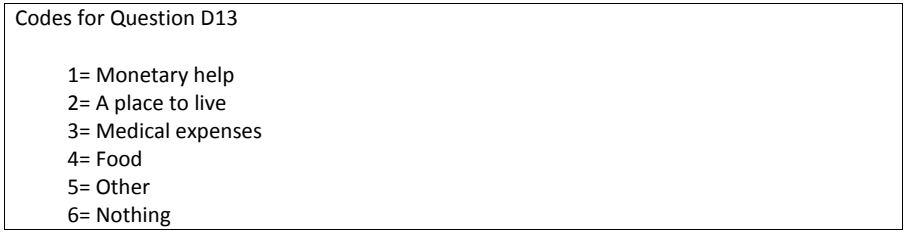

FIGURE 1.5. Codes to accompany question D13 from the LSAM questionnaire, 2008.

to provide them with when they were old ("When you are very old do you expect this child to provide you with:"). Interviewers were instructed to record up to three responses, drawing from the numerical codes in the accompanying code guide (see figure 1.5).

After the interviewer asked question D13, respondents often laughed, rather than providing a response. For example, during one interview in central Malawi in June 2008, Grace, the interviewer, struggled to deal with her respondent Esther's response to D13. Upon hearing the question, Esther laughed loudly and said, "Eeee, they are my children!" She implied that Grace knew already that her children should provide her with "everything," which she later said explicitly: "Everything! [Zonse!] They are my children after all." Grace awkwardly attempted to get Esther to narrow her response such that it would correspond to one of the codes itemized in figure 1.5, but to no avail. In this case, Grace followed the advice she had been given in prefieldwork training sessions: she best approximated her respondent's answer with the possible codes, listing 5 among two others to capture "everything" by adding in "other," despite researchers' continual advice that interviewers use "other" as a code only sparingly across all closed questions. In this research encounter, then, it is through cooking up an answer not mentioned by Esther that Grace embodies her role as a good interviewer adherent to standards for data collection. In the field, we note that even the most closed questions—meant to mitigate the influence of fieldworkers on data collected—rely on improvisation and creativity on fieldworkers' part. Whereas in the field, "Other" becomes a kind of saving grace for interviewers who struggle to convert the words of their respondents into preset codes, in the office, it is the least desirable form of data because as a catchall, it encompasses too much to be useful in data analysis.

Conclusion

In mid-2008, I witnessed a debate about survey translation during a training session for HIV VCT counselors working with LSAM in a district in central Malawi. A few of the interviewers objected to the way that a "circumcised person" was referred to in Chichewa on the questionnaire (*mdulidwe*, or one who has been cut), claiming that people in central Malawi—where cultural male circumcision is infrequent—would not know how to respond or would be confused. This debate, which interrupted the flow of the training for half an hour, indicates that even after surveys have been pored over in survey design meetings, they undergo negotiation and alteration as they travel out of the office. One interviewer suggested, "In Balaka [a Yao and predominantly Muslim area further south], people might know because it [circumcision] is their culture and tradition, but here in Mchinji [central Malawi], we will find people don't know. . . . People would only know it from the Bible, or just say to us, 'The Yaos do that, not us.' " As people nodded in agreement, another interviewer suggested that the current translation made it appear as if the man was naked, making a very personal question even more embarrassing to ask. Eventually, the group came to the consensus that instead of translating circumcision as "mdulidwe" (personified, "he who has been cut," from *kudula*, to cut), it should be referred to as *jando*, a Chiyao word that refers to the circumcision ritual experienced by male Yaos. Even in Christian areas, people know that Muslims practice jando. Thus, the group concluded, referring to circumcision as jando would ensure that data collected were consistent and accurate. For this particular question, field teams found both English and Chichewa inadequate, borrowing instead a word from Chiyao to ensure a question was as straightforward as possible.[23]

Notably, the final version of the VCT questionnaire—following deliberations about this point between Malawian supervisors and foreign researchers—employed the verb *kukotola* (close to the original kudula, "to cut," also "to strip from") dismissing both of the earlier translations. This vignette about translating the term "circumcision" depicts many of the themes this chapter has explored. First, it highlights that translation entails not mere conversion of words from one language into another: in this case, interviewers agree that the best token to capture the meaning of the word comes from neither the source nor the target language but another altogether. They also enact a kind of imaginative labor that mirrors that of the demographers who penned the initial question: they conjure an ideal-type respondent who, for example, on cultural or other grounds might be offended by the original token used to discuss cir-

cumcision. Finally, the instability of the word on the page itself as the survey travels into the field is a metaphor for the instability of the data the question later collects. The interviewers who debate the question have absorbed and legitimated researchers' epistemic investments in straightforward questions that will generate the best possible data, even if their position in the apparatus means their suggestions to researchers are rarely acted on.

The disciplinary culture of demographers, statisticians, and survey professionals encompasses international standardization, households as sampling frame, investments in clean data, and comparability. This chapter has traced the important role of translation in the early stages of survey research in places like Malawi. Demographers first conjure an other space—the field—that informs survey design enacted in the office. In the process, they enlist local experts who provide knowledge about Malawi that is instrumental in planning survey fieldwork, even as this knowledge and its forms reflect structural and material inequalities between foreign and Malawian experts obscured by partnership rhetoric. Finally, ethnographic analysis of the processes and relations embedded in the survey as a recipe for data collection indicates the important role that translation plays in assuring validity and quality of survey data as it travels its life course, even as it carries into the survey unremarked assumptions held by those who design it. It is these assumptions that ensure ahead of time the quality of data.

The translative efforts on the part of both past and present survey administrators in the African context foreground how relations across cultural distance bring the original into the translator's purview and stoke the translator's desire to make meaning commensurate (Pratt 1991). In the late 1930s, the Nyasaland Survey Unit noted difficulties with translating qualitative food schedules administered as part of the Nyasaland Nutrition Survey:

> One of the chief difficulties found in eliciting information for filling in these . . . schedules has been the native's interpretation of the term "food." The answers to the question, "Have you eaten to-day?," or "Have you cooked to-day?," will be yes if they have eaten a main meal consisting of porridge made from the staple foodstuff, maize or other cereal or cassava, together with some side dish or dishes. A reply in the negative does not mean that no food at all has been eaten, but that the informant has not had what he considers to be a proper meal. (Berry and Petty 1992, 27)

Translation's primary connotation invokes its ambitions to determine linguistic equivalence: for survey researchers, correctly chosen word tokens

carry potential payoff in the form of better, more valuable data. Whether in the case of the Nyasaland Survey Unit anticipating discrepancies between the token (food) and its interpretation by the "natives" or in the debates about translation among survey design teams introduced above, translation attempts to process the knowledge and practices of one society into the context of another. In this conversion, we note the contradiction between making apprehensible and preserving cultural difference, as rooted in epistemological and economic hierarchies that commodify translation: from words themselves, data (and value) are produced. Yet, neither the English original nor the vernacularized translation are fixed and persisting tokens; they lack essential quality and are continually transformed in space and time. Nonetheless, the claim that words can be made commensurate through careful translation and attention to the probable thought processes of an imagined low-literacy research subject betrays the epistemic commitments of researchers that define the criteria by which data are evaluated and valued. Translation is a technology that produces the subject positions and epistemological relations necessary to making bits and pieces of knowledge into valuable data.

In participating in survey design sessions and prefieldwork meetings as an anthropologist among the demographers, I was especially attentive to deployments and investments in the term "culture," that intangible and slippery aspect of human realities that anthropologists guard as their own. The invention of culture in survey research worlds plays many roles, not least of which includes compelling the translation of survey tools, metrics, and technologies into other spaces and populations than the ones they were conjured in. For demographers, culture and the field are imaginaries that enable the planning and implementation of data collection. They facilitate the building of extensive human (local fieldworkers and supervisors) and material (makeshift field offices in rough conditions) infrastructures that are social and technological hybrids reflecting demography's normative disciplinary investments.

Much of the literature on designing, implementing, and ensuring data quality in the context of survey research in developing countries centers on the problem of translation: How do we translate dominant standards for high-quality data into foreign and remote places rife with impediments that threaten to undo data's value? How do we translate survey instruments—and their underlying constructs—from one language and one cultural milieu into another without sacrificing their validity and reliability? Notably, however, these questions at the core of demographic research in places like Malawi presume demography itself as an acultural endeavor rooted in science and in possession of objective measures and instruments with universal value for

collecting clean numerical data. Recent work in critical demography, science studies, and anthropology has troubled this assumption, even if its insights have failed to migrate into survey research worlds. This body of critical literature often assumes that scientists' myopic focus on their end goals narrows their vision and causes them to overlook important local factors or miss the blind spots in their own research. In her reevaluation of the Nyasaland Survey Project's well-known failures, for example, Cynthia Brantley (2002) attributes its shortcomings to a "clash of values" between the British survey team and the African ways of life they aimed to study. Surely, they may have been "puzzled that Africans viewed wealth differently [than they did]" and possessed faith in a universalizing science that promised—but failed—to provide answers to problems (51, 58–59). However, close attention to the culture of demographers in action indicates that they come to see exactly what they want to see: the survey forecloses alternative optic possibilities so that data will be clean and valid.

In analyzing how the material and human infrastructure for survey research comes about, and how the survey—the core framing device and recipe for data collection in the field—materializes, I have shown how demographers adopt heuristic tools that require imagining their objects of study as stable, fixed, and unitary, as in the case of population or household. These tools make it possible to see and represent realities of interest, to make commensurate the incommensurable, and to measure even the most intangible and subjective of constructs in an objective manner. Returning to Ferguson's (1994) insights, the remainder of the book shows specifically how the form of discourse that is the survey and its underlying epistemic fields constructs objects of knowledge and creates an infrastructure of knowledge around those objects. From the perspective of anthropologists and other critics, this structure might be said to traffic in false pictures or oversimplified numbers. The main thrust of this book, however, is not to show that demography gets it wrong, but to show that the institutionalized production of certain ideas and measures of Malawian realities have important effects and create new social worlds and possibilities. Indeed, my question is not how can demographers do better, but how do demographers generate a particular usable representation of Malawi for themselves and others who assign it legitimacy (Ferguson 1994, 71)?

Sample and household, as we have seen, are central categories in demographic analysis, and both graft a certain false fixity onto shifting and complex human realities that far exceed the ambitions of the bounded container metaphor central to the demographic imaginary. They very much influence data

collection methods and the kinds of instruments inserted into surveys. Individuals behave in uncontained ways—but demography has ways of capturing and measuring such unruly behaviors as migration, refusals to answer a survey question, death, and so on, such that they do not threaten the imagined bounded container that underlies epistemic virtues. The bounded container model incorporates well-formulated checks and balances that enable statements to be made about data's reliability. These categories are not simply objective or statistical but culturally embedded, as this book elucidates. Categories do not merely measure something out there but are symptoms of the foreclosure of other ways of seeing.

As chapter 2 illustrates, the meaning of the categories and terms enshrined in the questionnaire can only be understood in terms of the discursive processes in the forms and instructions used in the collection of data (Higgs 2004, 90). In its movement from the office discussed in this chapter into the field that is the geographic setting for the next three chapters, the survey form's standardizing, clean, and neat ambitions and dreams are threatened at every turn.