FIVE

WHEN NUMBERS TRAVEL

The Politics of Making Evidence-Based Policy

Chifundo, lead MAYP supervisor, hands me the keys to the project storage room, from which I am meant to fetch pens and clipboards for fieldworkers. As I push open the heavy wooden door, made too big for its frame by the dampness of the rainy season, I am immediately struck by the large volume of paper all around me: hundreds of completed surveys (collected last year) are stuffed into boxes piled on sagging shelves. The papers are yellowing, dusty, and covered in spider webs and the room smells musty and damp. The back room, attached to MAYP's field office, is a storehouse for raw data; each survey contains fading pencil and pen marks that have by now undergone data cleaning and been converted into codes ensconced in MAYP's growing database and enlisted into claims as evidence. Months later, I have a similar experience standing amid boxes of completed surveys in LSAM's storeroom (see figure 5.1). While boxes full of surveys are the forgotten detritus of data collection in years past, they index present and future temporalities in which the information they contain now circulates in different, cleaner form.

FIGURE 5.1. Boxes of completed surveys in LSAM field office storage room. Photo by Joshua Wood.

Meanwhile in the main room of the field office, data entry clerks sip sugary tea from blue plastic cups as they tap diligently at the keyboards of project-owned laptops. Next to each clerk is a marked-up survey collected the previous day by field teams: their labor is converting the pen marks—raw information provided by respondents—into neater and tidier entries typed into a growing database. A few weeks later, after all information has been entered and data collection has wound down, the makeshift field office will be locked up until the next round of data collection, and project employees will seek out the next job in their project-to-project livelihood strategy. The end of fieldwork—packing up and leaving a rural field site—is a logical bookend to the opening scene of chapter 1, which foregrounded the immense work required to set up and carry out field research under difficult and remote conditions. Yet the life course of data does not end in a dusty store room: in their repackaging as statistics and numbers, data are immortal, their future travels and uses yet unknown.

. . .

We have thus far traveled with data along their life course, exploring the human and nonhuman actors that help them along and coming to understand that even though raw data are fictional, imagining they exist does important work for demographers interested in ensuring that research proceeds smoothly and numerical data attain disciplinary quality standards. This chapter examines what happens to data after they are collected in the field. Following others, I critically examine evidence amid the rise of evidence-based rhetoric as the default language for conceptualizing the link between research and action in global health and other scientific worlds (Goldenberg 2006; Lambert 2009; Adams 2013; Biehl and Petryna 2013, 8; Fan and Uretsky 2016). In particular, I take interest in evidence-based policy, which, in the public and expert imagination, is an important site into which data are absorbed as evidence to justify claims about the distribution of resources and political energy to national (or international) problems such as the HIV epidemic. As others have shown, evidence-based global health rhetoric privileges and presumes a certain kind of evidence: good, clean numbers. This book has shown that numbers contain multitudes; in their life course, they not only represent but constitute and reflect the particular social worlds and infrastructures necessary to birth them. While numbers, statistics, and enumeration are the underlying objects and processes by which knowledge meets particular "rules of verification and falsification" in global health research worlds, it is important to understand how, why, and under which conditions specific numbers become facts and evidence (Foucault 2008, 36). Critical accounts of numbers and enumeration paradoxically often take for granted the authority, rule, and hegemony of numbers as a form of evidence, yet, as this chapter shows in detail, numbers do not stand alone but require cultural, social, and other scaffolding and negotiation to be propelled through the world (Knaapen 2013). Further, as this chapter suggests, sometimes good numbers fail to convince their audiences of their validity, and data lose out to other criteria. Evidence not only reflects the ideological or epistemological conventions of those who produce it but is verified and achieves circulation via aesthetic and performative gestures and within located social relations. Responding to Gieryn's (1999) call for detailed examinations of local and episodic constructions of science in its downstream sites of consumption after it leaves the laboratory, field, or office, this chapter probes the cultural boundaries of surveys and their product—data—in several sites where numbers are enlisted into stories and knowledge claims as evidence.

Thus far, we have observed that quantitative health data are produced by heavily negotiated social relations that, in essence, cook reality to fit templates for research. The unreliability and contingency of numbers we usually take at face value is clear. In this chapter, I move away from survey projects in the field to focus on where and how quantitative and other evidence travels beyond sites where it is collected. I then present two extended vignettes to analyze how data in their finished form (as statistical evidence) are negotiated in unfolding social relations in downstream sites, just as they have already been in the field. In the first case, I show how numbers drawn from consultants' careful literature review were altered and took different form when they made it into national AIDS policy. In the second, I show how well-collected and scientifically validated numbers about the prevalence of HIV among men who have sex with men (MSM) in Malawi failed to travel or to convince their audiences. I illustrate that the use and evaluation of data may sometimes rest less on whether it is good or bad by epistemic standards than on users of data cooking numbers toward their own ends. While this sounds insidious, it is my hope that this chapter will instead show that the numbers underlying evidence-based claims in the policy-research nexus are never stable and always subject to processes of cooking, even in finished form.

To accomplish these dual objectives, I draw on interviews and conversations with demographers, policy makers, and bureaucrats, as well as participant observation at conferences and meetings where numerical data figured heavily in discussions and debates about the AIDS epidemic in sub-Saharan Africa.[1] I also read policy, gray literature reports produced by NGOs or other organizations outside formal publication channels, and journal articles to analyze the role and performances of quantitative data within them. The analysis of evidence in this chapter is enacted against the backdrop of the rest of the book, although the evidence analyzed below is not drawn directly from the databases of the survey projects discussed in chapters 1–4. The first vignette ("The Black Box of Culture in AIDS Policy") traces the travels of data I helped collect during my time with LSAM in 2005 as a cosupervisor of the Cultural Practices Study mentioned in the introduction, showing how it was mobilized toward diverse ends in its travels between 2005 and the present. The vignette explores how and why nonexistent (ghost) numbers became a good enough evidence base to inform national AIDS policy. The second vignette ("The Case of an Unsavory Risk Group") analyzes statistical evidence—based on data collection overseen by a Malawian NGO and a major research university in the United States—of high HIV prevalence among MSM, showing how, despite its merits, it failed to inform national AIDS policy until many years later. Throughout, I foreground the continued ways in which data are cooked as they move along a life course that stretches from the office of survey design to the downstream sites where they take form as evidence. While statistics like those discussed here are often considered to be the final representative form of knowledge (clean, cooked according to scientific standards), it will become clear that in their circulation through diverse spaces, they continue to undergo transformations and critical evaluation from their audiences. How are numbers operationalized in downstream sites by their users? How is their context of production foregrounded or obscured, and to what ends, as they are enlisted into representational projects? Evidence's validity and authority—rather than being inherent to it—are performed as it travels across boundaries between actors attuned to different goals at multiple scales of policy-research bureaucracies, this chapter suggests.

The second half of the chapter analyzes the rhetoric of a policy-research gap, discussed by my informants, common to global health worlds. I argue that gaps such as this are not merely an empty space or failed communication between researchers and policy makers. Rather, this gap is best conceived as a confluence of multiple and competing interests and frictions that is full of pre- and misconceptions, which determine not only the kinds of evidence that gain authority in the policy-research nexus, but also the efficacy (or not) of translation between the two spheres (Apthorpe 1997, 55).

The sites discussed herein—conferences, presentations of findings, meetings, and policy itself—are conceptualized as stages, or places where performances of knowledge take place. Centering my analysis on the scripts, props, supporting actors, and aesthetic and generic features that propel evidence in its travels through networks and spaces that define the scientific community (including demographers and others who produce and circulate numerical data) and its overlap with policy worlds, I show that evidence making is a process that transpires within social relations, and reflects and calls into being norms and standards that arbitrate whether evidence is good or bad. In examining the stories that numbers tell in specific places, it will be clear that they project and point to pasts and futures, and index a world outside the spaces they circulate within.

What Is Evidence?

The circulation of numbers such as those produced by LSAM, MAYP, and other kinds of research projects is central to the global health apparatus; these quantitative data knit together people and institutions in diverse sites and with diverse interests. Numbers, anthropologists have shown, are the primary form of authoritative evidence for making policy decisions, funneling resources, and measuring health-related phenomena at the national, regional, and international levels. Recent initiatives to make survey and census data publicly and widely available mean they are potential forms of evidence accessible to diverse researchers, policy makers, and activists (Zuberi 2005; McCaa et al. 2006).[2] Numbers convert lives, deaths, and social phenomena into portable forms that circulate widely and can be made to tell important stories about, for example, the AIDS epidemic in Malawi. Most centrally, numbers are the evidence base for international and national policy and have gained new authority, for example, as indicators that determine which interventions should be funded or measure how well countries manage aid in a climate fixated on aid effectiveness and, increasingly, return on investment (Segone 2004; Cornish 2015; Erikson 2016). The rhetoric of policy-relevant and evidence-based policy has trickled into national-level documents, including in Malawi. The government-produced 2015 Malawi AIDS response progress report, for example, explicitly notes that the recommended strategies and interventions it proposes were "informed by research evidence" (GoM 2015, 3), and the Malawi HIV Prevention Strategy (2015–2020) terms itself "evidence-based" (NAC 2014, 11), as such policy documents have for many years now.

Media representations, policies, government statements, and public discussions rely on numbers to bolster the claims and stories they circulate. The political power of numbers lies in their ability to go unquestioned, to be taken for granted, and to shape narratives that carry with them the power to cast certain citizens as backward, to direct resources here versus there, and to insulate institutions and governments from accusations of resource mismanagement (Briggs and Mantini-Briggs 2003; Briggs and Hallin 2007; Redfield 2013, 113–114). In an era of indicators, numbers may also act to challenge the authority or status quo of governments or to reinforce northern paternalism and imperialism. They help powerful people determine which lives count or are worth saving or improving (Packard 1989; Petryna 2002; Nguyen 2010, 163; Nelson 2015). Even as national priorities and concerns around HIV in Malawi have shifted since the early days of the epidemic (see the introduction), the role of numbers in giving these stories credibility and authority has remained consistent. Projects such as LSAM have, over time, incorporated shifting and diverse concerns into their surveys, coming up with new and better ways to measure or count them along the way. Data collection happens again and again, adjusting to meet the time-sensitive needs, funding cycles, and fads of global health practitioners and institutions. The changing form of LSAM's survey questions since the late 1990s, for example, mirrors shifts in international policy and research priorities over time.[3]

Scholarship in critical global health studies has tracked the rise of evidence-based global health, showing how numbers act as a universal currency and play expedient and often unquestioned roles in how we (think we) know about health problems such as AIDS in Africa (Erikson 2012; Biehl and Petryna 2013; Adams 2016a). As Adams (2013, 57) suggests, "For evidence to say anything valid about 'how to prevent or treat a known health problem,' it must speak the language of statistics and epidemiology." To this end, in what follows, I examine some of the processes through which raw data come to speak this language (or not) and become real (or not) in the eyes of actors in the policy-research nexus in Malawi.

From its earliest etymology, "evidence" has carried connotations of transparency, obviousness, conspicuousness, and clarity: evidence seems to need nothing but itself to stand in as proof for belief or claims. Yet its etymology likewise carries meanings associated with displays or appearances from which inferences may be drawn; evidence is an indication, trace, or token (from the *Oxford English Dictionary*, online). The duality of meaning points to evidence as a thing to be taken at face value and a thing whose face value relies on shared interpretive frameworks. As suggested in the introduction and chapter 1, demographers form a "population-based epistemic community" that constitutes an array of actors located in policy institutions, government health and aid ministries, census and development bureaus, a range of family planning and development NGOs, and academic centers of demography and public health (Halfon 2006, 794). These actors conceive of, speak of, and theorize the world in similar ways and form a sociotechnical network that stabilizes, coordinates, and disciplines ways of talking about AIDS and other population-based issues. The surveys we have become familiar with in prior chapters—as documents and tools of scientific measurement—are not always present when such actors come together but help establish discourse and action by forging rituals of knowledge making, methods, and legitimate sources of inference (Halfon 2006, 785).

Serving as the underlying template for good data, the survey reflects and constitutes the standards by which the quality of data—and, later, evidence—will be arbitrated. For demographers leading projects like LSAM, MAYP, and GSIP, formal avenues of face-to-face communication such as policy meetings, meetings of the Population Association of America, the International Union for the Scientific Study of Population, and the Union of African Population

Scientists, journals such as *Population and Development Review, Studies in Family Planning, International Family Planning Perspectives*, and *Demography*, and documents such as demographic and health survey country reports are central sites in which the data they collect in the field aspire to become good evidence for claims made about the lives of rural Malawians (Halfon 2006, 794–795). These venues in and through which quantitative data circulate—what I term the policy-research nexus—are bounded by and reproduce population-based epistemic investments (Riles 2000, 3). Data are used to make evidence and this evidence is located, embodied, and reflective of the interests and social positions of those who enlist it into knowledge claims (Mosse 2004; Cornish 2015, 274). However, appeals to the authority of evidence, particularly quantitative evidence, obscure the subjective elements of knowledge production.

Evidence-based policy making presumes its foil. "Evidence" carries connotations of transparency, accountability, objectivity, and neutrality. Policy not based on evidence, then, is presumed to be mired in or tainted by power relations, corruption, ideology, and arbitrariness (Timmermans and Berg 2003). Further, the assumption that numbers stand alone as representative of reality overlook the complex scaffolding that propels them and enrolls supporters to the claims they bolster. In what follows, I begin in two different downstream sites in the policy-research nexus: national HIV/AIDS policy and a district-level local research dissemination meeting. In each, I present a claim about the HIV epidemic in Malawi that relies on data collected long before the claims were made. I aim not only to trace the lives of the numerical data that seem to inform such claims but also come to understand why, whether, and how claims find traction and enjoy further circulation or not.

Ghost Numbers, or When Data Lose Out: The Black Box of Culture in AIDS Policy

In 2009, Malawi's National AIDS Commission (NAC) published "National HIV Prevention Strategy: 2009–2013." The strategy notes that "harmful cultural practices" are one of the "well-documented factors that facilitate the . . . spread of HIV in Malawi" (NAC 2009, 10), and reducing risk of HIV transmission through harmful cultural practices is itemized as a strategic approach for HIV reduction (29). In the portion of the strategy that discusses the action plan for implementation, harmful cultural practices and beliefs again surface as important sites of intervention and education activities to be implemented by key organizations including NGOs and the Ministry of Health (42). This has important effects, especially considering that the action plan calls for provision of material and financial support to structures that will mobilize against harmful practices and promote positive ones (52). Indeed, the strategy describes itself as a tool for "planning, implementation, monitoring and evaluating and resource mobilization for HIV prevention interventions." It is also self-consciously "evidence-based" and "data-driven" (1).

Malawi's recent National HIV and AIDS policy, for 2011–2016, also lists "harmful cultural practices" as a major "risk factor that fuel[s] the HIV and AIDS pandemic" (NAC 2010, 8). A look at Malawi's 2015 AIDS Response Progress Report indicates that "harmful cultural practices" are a "human rights violation that promotes HIV transmission" (NAC 2015a, 26) and notes that such practices were a major theme in information, education, and communication materials distributed in the country in 2013–2014. Documents such as Malawi's Prevention Strategy, HIV and AIDS Policy, and AIDS Progress Reports play a key role in performing Malawi's priorities and commitments to both its citizens and outside states and donors, and in determining flows of money and energy during the time periods covered. Malawi, not unlike other donor-dependent countries, is notable for "the multiplication of policy documents and an absence of real (implementable and implemented) policies beyond the very short term" (Booth et al. 2006, ix). Nonetheless, in policy documents covering the past decade, "harmful cultural practices" finds a consistent place in the local Malawian expert imagination of the epidemic (Watkins and Swidler 2012, 5). Drawing on my ethnographic work in both 2005 and 2007–2008, this section takes the claim that harmful cultural practices are a major driver of the epidemic in Malawi as a starting point and aims to excavate the nature of the evidence that supports it. I focus on how information related to the claim was gathered, what was perceived as credible evidence by different actors in the policy research nexus, and how and why information was ignored, reinterpreted, and distorted, and by whom.

Preparing policy documents is a long and complicated process that requires gathering of data relevant to policy narratives and statements ahead of time. Policy analysts have shown that the ideal model of policy making—where good research evidence makes its way directly into policy—rarely materializes (Walt 1994; Crewe and Young 2002; Hutchinson 2011), and, as is examined later in this chapter, my informants perceived a policy research gap, and suggested that research findings too rarely made it into policy. Nathanson (2007) argues that the credibility and authority of knowledge and its potential transfer into policy are contingent on political regimes in place, the maneuverings and interests of knowledge brokers, and conjunctions of timing and opportunity. Scholars and local critics have documented overuse or privileged use of foreign or expatriate consultants in the policy-research nexus of the global South, which likely reinforces the gap between policy makers and researchers: the latter are presumed to possess higher-order expertise to evaluate, inform, or bolster local policy. Further, the construction of evidence discussed above as neutral, objective, desirable, and transparent maps onto racialized hierarchies informed by the postcolonial politics of collaboration in places such as Malawi, hierarchies that still structure talk and rhetoric in development, global health, and aid worlds (Crewe and Axelby 2013, 79).

There are significant material stakes in winning a consultancy and, as we saw in chapter 1, a culture of moonlighting draws local experts away from basic research and university-level teaching and toward high-paying consultancies. Consultancies are advertised in Malawi national newspapers and often recruit both Malawian and foreign consultants. In the period leading up to preparation of Malawi's 2009 National HIV Policy, the NAC hired two consultants to collaborate to review literature and collate the evidence that would inform the policy.

In June 2008, I sat around a table with five other people: Dr. Richard Castells, the American epidemiologist mentioned in the introduction and an expatriate consultant hired to evaluate HIV prevention strategies in Malawi; his Malawian coconsultant demographer Blessings Chimanda; and American graduate students in biology, demography, and sociology affiliated with LSAM. Richard was in Malawi for a short time and sought information from the other individuals present, all of whom—including the author—had spent more time on the ground in Malawi than he had. Castells's and Chimanda's findings would inform the NAC's National HIV Prevention Strategy for the coming years, which was, at the time, in preparation.

Richard kicked off the discussion with a series of queries through which he sought to ascertain the role of risky cultural practices in fueling the epidemic. While Dr. Castells would review and collate, in collaboration with Blessings, a boatload of documents, reports, and studies on HIV in Malawi before finalizing his report back to the NAC, this meeting was a chance for him to seek information in person from a group of individuals who might contest or reinforce the dominant narrative threading through such documents. His question about cultural practices was unsurprising, given researchers' and policy makers' interest in how traditional rituals, practices, and norms might exacerbate the spread of HIV at the time; included in this category were a wide range of activities under the sign "culture," ranging from traditional male circumcision rituals to widow inheritance to *fisi* to erotic dancing at ceremonies to *kulowa kufa*.[4] Further, during my time in Malawi in 2007–2008, cultural practices were often in the national newspapers' headlines, and featured prominently in discussions I had with Malawian policy makers and NGO staff, who told stories about intractable harmful cultural practices in the villages (Page 2014; Esacove 2016). Journalists captured public attention through sensationalized representations of the traditional beliefs and practices of the country's rural residents, who were consistently portrayed by radio and newspapers as wearing a "veil of ignorance" and being "killed by attitudes and . . . lack of knowledge" about AIDS, for example (Chandilanga 2008; Mpaka 2008).

As the anthropologist at the table, I was skeptical of the equation of culture and risk for reasons that will become clear as this section unfolds. I stated that I thought the focus on cultural practices was overblown and worked to draw attention away from other more pressing issues and from the failures of government-led and foreign-influenced policies, structures, and interventions by placing blame on backward culture and society's most vulnerable (Briggs and Hallin 2007). On the heels of my statements, Blessings, the Malawian coconsultant, counterargued that there is significant evidence that cultural practices were fueling Malawi's epidemic. When Castells asked him for citations they could enlist as evidence in writing the report for NAC, Chimanda stated that "a number of studies have been done" and verbally noted studies implemented in recent years by NAC, the Malawi Human Rights Commission (MHRC), and UNICEF. Positioned as he was as a local expert hired for his knowledge of matters such as these and, as a Malawian, more expert on Malawian culture than others at the table, Blessings's claim became the final word—at least that afternoon—on cultural practices: he had tentatively been extended epistemic authority by those present, winning this particular credibility contest (Gieryn 1999).

The validation of Blessings's claim as evidence—after all, it is unlikely that Castells would proceed to closely read the studies for himself given the time constraints placed on consultants—that culture should be a central site of national and international intervention worthy of funding and scrutiny relies on the few studies on the link between HIV risk and cultural practices that had been completed prior to our conversation that day. Although none had documented a link between engaging in a harmful cultural practice and contracting HIV, it was these studies that stood in as evidence to support a claim that gained momentum. The most well-known and comprehensive study of cultural practices in Malawi at the time of our meeting in 2008 was a 2006 report by the MHRC referenced by Blessings, titled "Cultural Practices and

Their Impact on the Enjoyment of Human Rights, Particularly the Rights of Women and Children in Malawi." While this 137-page report focuses on the threats that practices related to marriage, initiation, funerals, and chieftancy pose to the human rights of participants, it also presumes that such practices have a role in transmitting HIV.

In the section discussing male initiation, for example, the report suggests that the practices associated with initiation are "quite risky" in the face of the epidemic (MHRC 2006, 107); the researchers, however, did not collect HIV-test data to support this claim, making the claim about HIV transmission quite a flimsy one as judged by typical standards for evidence operative in global health worlds. The study was, as the authors suggest, based on both "quantitative and qualitative data"; the former was collected via a structured survey administered to 262 respondents in nine districts, purposively sampled to capture ethnic identity differences. The qualitative data, meanwhile, drew from a total of ninety-nine focus group discussions held across all the districts sampled. Although the survey administered contained only close-ended questions, the bulk of numbers cited in the long report capture what percentage of respondents mentioned specific cultural practices as familiar (e.g., "polygamy," 98.1 percent; *jando*, 16.5 percent, pp. 14–16).[5] The report's appendix includes the survey itself and illustrates that respondents were asked whether each of six cultural practices (in the case of practices related to rites of passage) were "found in [their] home area" (125). In this sense, the bulk of the long report relies heavily on claims made by respondents in the focus group discussions, citing throughout the perception on the part of respondents that various cultural practices pose risks for HIV, for example: "Most respondents were of the view that dances such as *mtungo* and *magolowazi* should be abolished because they promoted promiscuity and . . . the spread of STIs, including HIV/AIDS" (35).[6] Although this evidence would normally not pass muster in the eyes of demographers, epidemiologists, or policy makers under the spell of the hegemony of numbers, the MHRC report has enjoyed long citational life to this day and, as we will see, in the case of Blessings's claim above became one key part of the solid foundation for policy making in 2008–2009.

The MHRC report, and those few others that have been published in its wake, references two other studies that are often mentioned in discussions of risky culture in Malawi: a study of a single village in Lilongwe District (UNICEF 2001) and a study based on focus group discussions with village leaders in a single district (Phalombe) carried out in 1997. The latter presumed a link between cultural practices and risk of HIV transmission: the study was meant to provide a basis for advocating behavior change (Kornfield and Namate 1997, v). Again, none of the studies meant to draw a connection between cultural practices and HIV risk-tested people for HIV, nor did they ascertain the presence of other negative health conditions, such as sexually transmitted infections.

In justifying his claim about risky culture, Blessings also mentioned a study of cultural practices implemented by NAC in 2005. Though the findings of this research were never officially published, Blessings (and others in research worlds in Malawi) would have had access to it via word of mouth or knowledge of the study. Incidentally, I was involved in collecting data for this study alongside the 2005 wave of LSAM's data collection in three Malawian districts. It is worth returning briefly to my field notes and documentation of the context and processes of data collection in 2005 so that we can best understand the nature of some of the data that became evidence in our conversation three years later around the table in 2008. The Cultural Practices Study was funded by NAC and drew on the resources—particularly transportation and fieldworkers with LSAM experience—being used by LSAM during its 2005 fieldwork season in three districts. Its main objective was "to identify the extent and type of high risk cultural practices that increase transmission of STIS including HIV/AIDS" and it also sought to "explore how communities have modified risky cultural practices."[7] In its focus on "cultural guardians," the study presumed—like many projects in Malawi and elsewhere in Africa—that involving these persons in AIDS-related interventions was imperative, reflecting, I suggest, global public health's obsession with culture as simultaneously a stumbling block and possible enhancement to global designs (see also West 2016, 114–119). The data were to be collected from respondents via administration of a survey. The proposal noted that the data would act as a complement to existing sets of quantitative data drawn from sources such as Health Information Management Systems, Community Health Sciences Unit, NAC, and National Statistical Office (NSO).

The fieldwork for the project was headed by a Malawian demographer, Dr. Chirwa, but field activities were largely overseen by Malawian field supervisors and the author, who, at the time, was a graduate student in anthropology. Fieldwork for the project unfolded on a tight schedule. For example, in one site, Balaka District, two interviewers—who had previously worked with LSAM—interviewed a wide spectrum of "cultural guardians" from June 25 to July 13, 2005, and interviews were first translated from multiple languages into English (Chewa, Yao, Sena, etc.) and then transcribed by five transcribers (including the author). By July 13, fifty-five interviews had been conducted in total in Balaka, one of the three districts in which fieldwork would be undertaken.[8] These persons included chiefs, deputy chiefs, *ngaliba* (male circumcisers for initiation rituals, jando), *azamba* (traditional birth attendants), *chitonombe* (counselor in charge of initiation camp), *asing'anga* (traditional healers), and so on. Some individuals in each of these categories were interviewed according to a predesigned interview guide with questions that were meant to lead to an understanding of the dimensions of the cultural practice in question and to ascertain whether or not its practice posed risks for transmitting HIV. Male initiates who had recently undergone jando, for instance, were asked questions about the instruments used to cut them and whether they were sterilized after each initiate was cut, about whether any traditional medicine was used to heal their wounds, and about whether parents or others in their communities pressured them to go for initiation. In an interview with a male initiate, the interviewer (I) makes clear his interest in unearthing cultural practices as risky, foregrounding the important role that interviewer-interviewee dynamics play in the research encounter and in the way that data takes form:

> I: How many initiates use one knife [to circumcise]?
> R: One per person.
>
> I: One per person and they [initiator] throws it away?
> R: Yes.
>
> I: Maybe they used one on many people?
> R: No. . . .
>
> I: But the practice of using one knife per person happens in other [places]. Maybe they use one knife on more people or after using it on one person they put the knife in hot water? . . .
> R: No! I should say all [initiators] practice these methods [of using one knife per person]. . . .
>
> I: Do you see any dangers of initiations?
> R: The practice has no dangers because it is good for a person to be initiated. . . .
>
> I: Is circumcision risky?
> R: Yes! Because sometimes [initiators] make a mistake and cut the head of the penis. . . .[9]

Here, the interviewer embodies the researchers' interests in unearthing or discovering that male initiation is risky for HIV transmission, and is likely invested in presenting himself to the respondent as modern and educated. In the transformation of the boy's experience into data, the object of culture is itself produced as a visible entity that can be studied (Langwick 2011, 284). Kaufulu (2008) helpfully illustrates, in reflecting on his position as an outsider interviewing cultural guardians among the Sena of Malawi, how research instruments and perceptions of researchers by informants answering questions about cultural practices moralized in the era of AIDS often lead to scripted responses, a form of cooked data in their own right.

Although the initiate clearly states that the initiator uses one knife per boy, the interviewer probes three times: it seems that he presumed that the initiate did not initially tell the truth. Further, although the interviewer assumes that the dangers of initiation would be related to HIV, the initiate's response to his question about whether circumcision is risky circumvents HIV altogether, citing a story he heard about an initiator's knife mistakenly cutting the wrong portion of the penis. Recalling my earlier discussion of probing, we might infer that the qualitative data collected are cooked in particular ways. However, my interest here is less in exposing how contingent or coercively extracted responses become data—as we have seen already—but rather in how the findings of this study and others like it became worthy of mention by Blessings in the earlier scene and worthy of citation in future reports and even policy. Certainly, his claim that culture is a major driver of Malawi's epidemic is not assigned validity based on what he claims to take as impeccable data or on claims that he had closely read the studies he cites; rather, it is his presumed status as a local expert, in possession of uniquely Malawian knowledge, that validates his claim in a particular conversation with a foreign expert.

This scene adds new depth to the concept of local expertise in global health research worlds: as we have followed data in their life course, we have likewise witnessed how and when certain individuals are deemed expert and on what matters. For example, in chapters 2, 3, and 4, we saw that while Malawian fieldworkers are considered experts in translating the needs of the project into the field, their advice and knowledge are rarely influential on the top-down templates that govern research (e.g., their criticisms of survey questions often went unaddressed by demographers). Similarly, while Malawian researchers are valorized as local experts in the context of survey design meetings described in chapter 1, the kind of expertise they proffer rarely influences the vision, design, and organization of a survey project. In the case of Blessings, however, we observe that on the matter of culture in particular, he is assigned expertise that helps propel cultural practices into national policy and ensures his claims are taken seriously. This series of examples reiterates, then, just how slippery local knowledge or local expertise are: these formations only gain credibility, value, or influence within sets of social relations, and the particular form they take reflects power asymmetries and the shifting value and meanings that global health assigns to the local or cultural.

COOKING CULTURE IN THE POLICY-RESEARCH NEXUS

Blessings's investment in the claim that culture is risky for the spread of AIDS became much more important months later, when it surfaced again on a different stage. Months after their meeting in Balaka, Castells's and Chimanda's findings—the results of the consultancy—were presented to two audiences by two different people: (1) to an audience of Malawian policy makers and government officials by Blessings; (2) to a regional audience by a Malawian researcher not involved in the consultancy. Prior to these presentations, Richard furnished Blessings with slides and graphs assembled from their joint findings about key drivers of Malawi's epidemic—none of which had to do with harmful cultural practices, but rather with the relevance of other potential routes of HIV transmission. The slides did have numbers, based on models that characterized the different routes of transmission (e.g., sex with sex workers, serodiscordant couples), and the percentage of new HIV infections attributable to each route of transmission, termed the HIV Modes of Transmission Model (Case et al. 2012; Shubber et al. 2014).

However, Richard later learned—after leafing through the slideshow Blessings attached to an e-mail message—that in their translation from the skeleton form of a presentation into the actual PowerPoint slides used by Blessings when he presented the results to NAC, the findings had changed.[10] Blessings, he said, had filled in the blanks by featuring his own view of the primary routes of infections: specifically, Blessings identified culturally accepted intergenerational sex as a key driver of the epidemic, despite the fact that this was not a route of transmission considered in the model. When Richard shared the slides with a demographer more familiar with the Malawian context than he was, she responded, "When it comes down to mismatches between what the data say and what the conventional wisdom is (or what Blessings believes, which is probably close to the same thing), the *data lose*."[11] Yet, even as Blessings's PowerPoint presentation may have misrepresented numerical evidence amassed about the epidemic's routes of transmission, the information it contained was propelled into other spheres: it was later presented to more Malawian stakeholders by another Malawian demographer at a regional AIDS conference.

One slide in Blessings's PowerPoint presentation, titled "Risk Factors (Drivers)," lists nine drivers, ostensibly based on the statistical data presented on the prior slides and gathered through the research and literature review he and Castells collaborated on. Three drivers, however, stand out because they are not borne out by the data (numbers) presented on other slides: intergenerational sex, transactional sex, and culture. The numerical data show that none of the three was a significant driver of the epidemic. For intergenerational sex, a few slides later, when the actual numbers are presented, less than 1 percent of women aged fifteen to seventeen had nonmarital sex with a man who was ten or more years older. Similarly, only 5 percent of men fifteen to forty-nine years old reported that they bought sex in the past year. Most interesting for the purposes of this section, however, is the slide's claim that culture is a driver of the epidemic.

On another slide, titled "Initiation Rites," Blessings presents a bar graph of "male adolescents who have ever had sex by circumcision status" to support his claim that those who have been circumcised are more likely to be sexually active compared to those who have not. Though the slide fails to cite the source of the numerical data that indicate, for example, that 77 percent of males ages fifteen to nineteen who have been circumcised have had sex, while only 53 percent of those uncircumcised have, some sleuthing on my part discovers they are drawn from a 2007 article published in the *African Journal of Reproductive Health*, which draws on data collected in 2004 by the Protecting the Next Generation: Understanding HIV Risk among Youth (PNG) project conducted by the Guttmacher Institute, a reproductive health nonprofit organization, in five African countries between 2002 and 2006 (Munthali and Zulu 2007). To locate the origin of the data source cited in the 2007 article, I dug up the PNG report itself, which describes the two-pronged sources of data for the project. Quantitative data were based on the 2004 Malawi National Survey of Adolescents, a nationally representative household survey organized by Malawi's NSO (4,031 adolescents, ages twelve to nineteen), whose survey document contained a section titled "Sociocultural Practices" that asked respondents twenty-one questions about participation in initiation, circumcision status (men and women), age at circumcision, and experience with scarification (PNG 2004). Qualitative data, meanwhile, were based on eleven focus group discussions with fourteen- to nineteen-year-olds in 2003 and 102 in-depth interviews, also collected in 2003.

A slide titled "Initiation Rites" also contains a claim—sans accompanying graph this time—that "80% of the women and 60% of the men in [the] South undergo initiation ceremonies." Considering this is on the slide prior to the data taken from the PNG project and its 2004 survey, the numbers should ostensibly match those solicited by the corresponding question (no. 1001) from the same survey: “Have you ever participated in a puberty or initiation rite?” While Blessings’s general claim that initiation is most common in the southern region of Malawi is correct, the precise numbers on the slide do not correspond to the data collected by question no. 1001: according to the 2004 data set, 43 percent of males and 57 percent of females in the South underwent circumcision. Whether or not the numbers on the slide are blatantly cooked—it is difficult to tell without direct citation of a source—the inclusion of this evidence on the slide is curious, considering that, as other documents have shown, many initiation ceremonies do not directly involve sexual intercourse or actual circumcision anyway (20 percent of males living across Malawi have been circumcised, while only 2 percent of women have undergone any type of circumcision (which may include actual cutting or not), even as many participate in initiation ceremonies of various kinds (Munthali and Zulu 2007).[12] A few slides later, Blessings presents a bullet list of other cultural practices including polygamy, wife inheritance, bonus wives, fisi, and kulowa kufa; again, I reiterate that in this era of evidence-based decision making and policy, at the time of his presentation, there was no quantitative data linking any of these practices directly to HIV transmission risk.

Despite the lack of any quantitative data on the correspondence of cultural practices with HIV transmission, “cultural practices” made it into the National HIV Prevention Strategy published by NAC (2009), suggesting that Blessings was not alone in disregarding evidence. To recapitulate, the strategy notes that “harmful cultural practices” are one of the “well-documented factors that facilitate the . . . spread of HIV in Malawi” (10), and reducing risk of HIV transmission through harmful cultural practices is itemized as a strategic approach for HIV reduction (29). This strategy was ostensibly at least partly informed by the research prepared by Blessings and Richard Castells as consultants to the evidence-based policy-making process. I do not suggest that Blessings’s PowerPoint presentation was the sole reason “harmful cultural practices” appears in the policy. Indeed, the rhetoric of harmful cultural practices appeared across multiple discursive spaces, including media, religious, development, and donor worlds, due in part to its familiarity and because it is, as Watkins and Swidler (2012) argue, a realm of intervention that everyone can agree on. Notes on a series of consultations spearheaded by NAC in mid-2008, for example, indicate that actors ranging from community-based organizations to people living with HIV/AIDS to human rights groups yielded feedback—meant to inform the 2009 strategy—that harmful cultural practices were furthering the spread of HIV and should be modified or eradicated.[13] Nonetheless, we can safely conclude that quantitative data to substantiate the claim that they are linked to risk of contracting HIV did not exist at the time of the policy's authorship (nor do they now). Further, even as the rhetorical investment in evidence-based policy has intensified since 2009, "harmful cultural practices" continue to find space in Malawi's national policy, despite continued absence of a study or studies that have directly linked cultural practices to HIV risk.[14]

While anthropologists and critics of global health's number-centrism have clearly demonstrated the power of numbers to travel widely and be imbued with confidence and authority, the example of Blessings, Richard, and the cooked PowerPoint slides indicates that the means and criteria by which evidence is assessed might prove more central to whether or not evidence becomes real than stand-alone good numbers. In this case, evidence relies on ghost numbers that remain invisible. First, Blessings is assigned credibility as a local expert by Dr. Castells in their initial meeting. Next, in preparing the slide show to be presented to NAC that reports on their research findings, Blessings has some latitude in determining the content of the slides. Whether Blessings fudged or cooked the data in his presentation is not my interest; rather, I aim to show how a claim not founded in quantitative data makes it to its final downstream site (policy documents). A close analysis of Blessings's slides indicates that the text written on the slides does not always align with the numerical data, graphs, and evidence. Nonetheless, the cultural practices claim makes it to the next stage in the policy-research nexus, perhaps because it aligned so well with what Watkins and Swidler (2009) have termed "conventional wisdom": the commonplace and widely circulating narratives that surround "African AIDS," including that backward culture fuels the epidemic in a geographic space that continues to stand in for the untamable and the premodern (Patton 1990, 77–97; Comaroff 2007, 197; Watkins and Swidler 2012). In discussing the case of ghost numbers, I do not suggest that more quantitative data should have been collected, or even that the policies discussed here are not evidence based. Instead, I aim to show that numbers, and evidence more broadly, do not stand alone, waiting to be enfolded into policy: they are helped along a life course and altered by social relations and transactions along the way.

As others have shown, culture becomes an anxious and moralized site of contestation and claims making in times of social upheaval, political uncertainty, and epidemics, often working to scapegoat society's most vulnerable or to rhetorically protect or distinguish certain groups in society from others (Forster 1994; Briggs and Mantini-Briggs 2003; Kogacioglu 2004; Peters, Kambewa, and Walker 2010; Biruk 2014a; Page 2014). As a Malawian, Blessings was likely afforded some measure of credibility and authority when speaking in front of an audience of fellow Malawians, all or most of whom it is likely were known to him, considering the small size and tightly knit nature of policy-research worlds in Malawi, as discussed in detail in chapter 1. As a trusted speaker and knower, Blessings's claims were bolstered, as well, by their lack of novelty: even before his presentation, it is likely that his audience expected to hear about harmful cultural practices. The phrase had, by 2008, become a buzzword (even with its own acronym, HCP), a kind of packaging or lingua franca that encased evidence and propelled it forward. The familiar form of PowerPoint slides with their graphs and numbers—the aesthetic props of dissemination in the policy-research nexus—distracted audience members from any potential disjuncture between the numbers themselves and the claims Blessings was making (the text on the slides).

As the comprehensive literature review of an unpublished report on the matter of cultural practices (primarily initiation) and HIV risk in Malawi prepared by a consultant and others for an international organization (2015) and made available to the author suggests, the evidence linking cultural practices to abuse of young people's human rights and to risks to their sexual health (such as HIV infection) is largely anecdotal, a word whose deployment immediately signals "non–evidence based." Evidence in the global health nexus is always already presumed to be quantitative. The 2015 report, however, goes against the grain and against the conventional wisdom about cultural practices by refuting the link between such practices and HIV that has been taken for granted in Malawi since the early 2000s (Page 2014). The study of 645 youths across six districts collected information on respondents' sexual and reproductive health histories, focusing on indicators such as history of STIs, contraceptive use, HIV test history, HIV status report, and so on.[15] Their findings are clear: "across all SRH [sexual and reproductive health] indicators, there were no significant differences between those who had been initiated and those who had not, suggesting that initiation ceremonies in Malawi do not have a positive or negative effect on the sexual and reproductive health of youth." It remains to be seen, in the coming years, whether the conventional wisdom of cultural practices will retain its momentum or fizzle out in the continued absence (or, perhaps, future presence) of numerical evidence, especially considering the trend whereby policy cites itself in a recursive and reproductive fashion year after year (Esacove 2016).

When Numbers Fail: The Case of an Unsavory Risk Group

In the case above, I note that a knowledge claim made it into policy in the striking absence of numbers to prove it: evidence in the form of the few studies on the link between such practices and HIV was cited and recited in a kind of evidentiary palimpsest beneath which were not good numbers but rather what we might term ghost numbers. This example helps challenge anthropologists' and others' assertions of the hegemony of numbers as the primary source of evidence in global health worlds. As they have well shown, numbers are less real, stable, and certain than they are taken to be and often fail to measure well the realities they claim to represent. Yet, as we have seen in peeling away the palimpsest underlying cultural practices rhetoric as it appears in policy, evidence is not *always* rooted in numbers, whether good, bad, or imperfect.

The case of the rhetoric of harmful cultural practices is a particularly useful lens through which to observe how social and cultural scaffolding and framing operate to define and propel evidence through spaces we might code as number-centric. The same data can wear many costumes and carry many meanings and agendas (Hodzic 2013, 100). Blessings becomes a spokesperson who is charged with not only presenting but translating evidence that stands in for and points outward to the real-world phenomena and people it seeks to represent. His slides contain miniaturized artifacts—numbers—that carry the outside inside and, in the process, make that outside make sense to a specific audience (Callon 1986). In what follows, I juxtapose this story with a quite different scenario, one where numbers—good numbers by demographic standards—are available and present, but nonetheless fail to convince their audiences and lose momentum in the world, ultimately preventing meaningful inclusion of a risk group (MSM) in national AIDS policy.

In October 2008, I attended an NAC-sponsored conference in northern Malawi, held at a posh hotel. In the years leading up to 2008, NAC had publicly stated its commitment to finding novel ways to disseminate research findings to Malawian citizens, and this conference was a pioneering effort.[16] This commitment emerged from ongoing discussions, particularly at the 2005 Research Council Meeting, that centered on how to ensure that community-based organizations (CBOs), coded as the grassroots, might best benefit from the information collected by government and outside research endeavors. As the research officer at NAC put it, NAC wished to allow people "who do not have the opportunity or means to attend the national meetings" to "hear" what was said there, behind closed doors.[17] The conference's main objective was to “discuss key findings of surveys . . . conducted in the country [Malawi].” A diverse group of individuals representing the grassroots was invited to this first meeting: chairpersons of CBOs, members of district AIDS coordinating committees, district HIV programming officers, and so on, all drawn from in and around the northern district where it was held. In order to ensure that financial barriers did not prevent these people from attending, NAC paid for participants’ accommodation and transport.[18]

About forty people attended the conference, and the presenters included NAC’s research officer and a collection of Malawian, American, and Canadian demographers and other AIDS researchers.[19] Amid the various PowerPoint presentations that researchers shared with the audience, one, given by a researcher-activist stood out. Felix, cofounder of a human rights NGO in Malawi, presented findings from a cross-sectional study of the behaviors of MSM in Malawi. As Felix set up his presentation and projected its title on a slide, the audience chuckled. The member of an AIDS prevention CBO sitting next to me mumbled under his breath, “There are none of these MSM here [in Malawi]!” This claim—alongside similar sentiments expressed by other audience members throughout Felix’s presentation—directly contradicted Felix’s central claim: that “MSM are more significant in Malawi’s epidemic than ever imagined.”

Felix, expecting negative reactions to his presentation, came equipped with numbers as his major source of evidence. He began his presentation by locating his findings in a larger landscape of comparative quantitative data on MSM in other nearby countries. Aware that his audience might be unfamiliar with the acronym “MSM,” he explicitly defined it. Next, he presented the statistical evidence to support his claims about MSM vulnerability and risk. Explaining that the data came from a larger four-country study, he led the audience through the numbers on his slides: HIV seroprevalence for MSM in Malawi was around 21 percent. Accompanying this statistic were absolute numbers and the confidence interval for the data (42/200, 95 percent confidence interval). Felix elaborated on the gravity of the situation for MSM in Malawi. They faced, for example, low access to health care (only 10 percent had disclosed to health professionals that they were MSM), and MSM’s high perception of AIDS as their main health risk was cited as evidence that interventions should be targeted at this risk group. Finally, Felix’s data indicated that MSM were often beaten, raped, or afraid to come out. Numbers—the primary props in Felix’s presentation—were framed by a set of accompanying scripts and actors drawn from other contexts: for example, new infections in MSM comprised 10–15 percent of the global AIDS burden.

Taken together, all of these numbers aspired to status as evidence that prevalence of HIV in MSM in Malawi is higher than generalized national prevalence, which was, at the time, around 12 percent. Other statistics presented alongside prevalence data drew on findings from a structured survey with respondents and aimed to bolster his claim that social stigma against MSM makes them invisible and excludes them from prevention messages targeted at other risk groups in Malawi. Felix called for sensitization of policy makers, HIV/AIDS key players, and other stakeholders, and for research that would explore sexual behaviors, practices, and social stigma experienced by MSM in Malawi. In addition, Felix suggested Malawi was far behind its near neighbors in accepting LGBT persons and achievement of human rights.

The numerical evidence cited in Felix's presentation to this audience was drawn from a multicountry study of MSM living in Malawi, Namibia, and Botswana. Participants in all three countries were recruited by community organizations working with this population who utilized snowball sampling to, in the case of Malawi, source 202 MSM-identified males for HIV screening and administration of a structured survey instrument. Notably, although the study was approved by the IRB of Johns Hopkins University, ethical approval was sought locally from the NAC in Malawi, but no response was given after many months, a fact that the authors of published research on the study and Felix himself attributed to possible aversion to the politically unsavory material of the study in a homophobic country (Baral et al. 2009). Symbolically, this chain of events stands in to demonstrate the noncommitment at the national level to issues related to MSM as a risk group.

In front of the audience at the conference, Felix's evidence failed. While those present, for the most part, were not policy makers and had very little influence over whether or how evidence might make it into policy, they nonetheless stood in for the commodified grassroots to whom evidence in the policy-research nexus should circulate and represent. First, the degree of departure of his claim from prior, tacit knowledge held in common by those in the audience was significant. When Felix described the main avenue of transmission for MSM (anal sex), for example, audience members responded with visible shock and moral outrage, calling anal sex unnatural and expressing disgust. "That doesn't happen here!" one woman shouted from the back. While audience members persisted in establishing Malawi as a decent nation where homosexuality does not exist, Felix tried to diffuse their outbursts with his numerical evidence. Nonetheless, the numbers on the screen challenged powerfully held convictions that served as a moralized and staunch evidence base for the counterclaim that MSM do not exist within Malawi.

In mobilizing this moral evidence, audience members employed two main tactics to discredit Felix's numbers: (1) attacking the credibility or motives of the researcher; and (2) questioning the quality of the evidence itself. Felix was asked twice to disclose his sexual orientation and accused of harboring a hidden political or other mission. Attacking the evidence he presented, one man called the presentation "hearsay," asking, "How can you put this on paper? What is your *proof*?" The calling into question of this evidence drew on personal experience of the audience members, who insisted that they had never seen or heard of men having sex with men. A pastor in the audience stood up and began loudly preaching against homosexuality, calling the MSM research "unscriptural," and suggesting that it was upon seeing MSM that God burned down Sodom and Gomorrah. It was clear—in the rhetoric—that Felix's numerical proof of MSM HIV prevalence, carrying with it the assumed route of same-sex transmission, offended the moral and religious convictions of most of those present. The emotional politics of homophobia, we might suggest, make its propagators "impervious to arguments and evidence" that might unravel their affective investments in the status quo (Ioanide 2015, 6).

Although this presentation generated the most conversation in the halls of the conference venue that day, it was by far the most conclusively invalidated by the audience. Despite the quality of Felix's numbers—by the epistemic standards of demographers or epidemiologists, and reflected by their publication in peer-reviewed journals—they did not gain traction in the room but fell flat. Despite this poor reception, an NAC officer approached Felix after the presentation and suggested he apply for NAC monies to do more studies. Over lunch, however, Felix shared that he had already submitted materials to NAC for their review but had not heard back from them for many months. Historically, he elaborated, the government had been very unsupportive of efforts to educate and mobilize MSM. As mentioned earlier, the study he was presenting was possible only because he relied on the cooperation, funding, and influence of an elite foreign university. While NAC endorses evidence-based policy and typically takes numbers as the pinnacle of evidence, they stalled in disbursing support or money to Felix, capitalizing on their ability as a public trust to publicly endorse transnational causes or fads but privately exert power over where pooled donor monies would flow once they arrived in Malawi.[20]

Importantly, however, the rejection of Felix's evidence gave the same knowledge claim legitimacy on other stages where it likewise sought to enroll supporters. Though his paper met a similar reception when he presented it a few months earlier at a conference in Lilongwe, Malawi's capital, he also said the paper had "really helped [him] move around." Felix had, in 2008 when we first met, recently traveled to workshops in Mexico, Geneva, Zambia, South Africa, and so on to present his findings to audiences there. Because MSM and HIV was a hot topic for epidemiologists and global health researchers and practitioners, Felix was frequently abroad in the United States for trainings associated with the multicountry study the NGO was implementing: we met up a number of times when he was visiting major northeastern American cities in 2009 and 2010. Following the 2009 global attention to the arrest, conviction, and ultimate release of a Malawian same-sex couple, Felix and his NGO garnered increasing support from outside funders and organizations, achieving a kind of agency through local victim-hood or suffering (Hoad 1999). Soon after the event, Felix said, "After all this publicity, NAC can no longer ignore our evidence. . . . They have to pay attention!"[21]

Though "people engaged in same-sex sexual relations" were first mentioned in Malawi's National HIV/AIDS Policy (NAC 2003) in 2003—albeit only cursorily—they were not allocated funds for prevention or treatment by the NAC until 2013. When NAC delayed disbursing the NGO's first payment installment, Felix had to warn them he would go "directly to the Global Fund" if he didn't receive it soon. By 2014, a survey and HIV-test study of MSM driven by respondent-driven sampling (RDS) was under way in Malawi, headed by the NGO and funded and supported by an American research university, UNAIDS, and PEPFAR. In July 2014, the NGO was negotiating with another major American research university to set up a research partnership that would also include capacity building and educational exchanges for local staff. However, according to Felix, although NAC is very interested in the data from the RDS study—it is important for national AIDS bodies to present good evidence to donors that they are working on and with key populations—they had not provided the NGO with, for example, a car to help with the rural sampling.[22] Further, in 2014, the NGO was lobbying for inclusion of lubricants in the national list of essential drugs because they are crucial to preventing HIV/AIDS transmission among MSM.[23] When NGO staff were invited to comment on the draft of Malawi's most recent National Strategic Plan, they made comments throughout the document to draw attention to this need; an officer at the NGO had to aggressively push for the "lubrication question" to be put on NAC's agenda in July 2014. The policy document that resulted from these discussions does suggest that lubricants will be targeted at "key populations" (NAC 2014, 45).

The failure of Felix's numbers demonstrating the gravity of the HIV epidemic among Malawian MSM to convince both grassroots and national-level audiences, who dismissed them and dragged their feet in responding to their call, respectively, pokes a hole in the hegemony of statistics in convincing, swaying, or impressing audiences. Indeed, even if the numbers attained validity when measured by epistemic standards, their circulation was either blocked or slowed through spaces in the policy-research nexus in which moral commitments trumped epistemic goodness. Felix's numbers did not come to inform policy meaningfully until the political context was fertile to accept them. Since 2008, when he first presented data from the study of MSM to national audiences, his NGO has intensified their evidence-based activism, relying primarily on foreign partners to fund and support research on HIV in MSM populations that will produce data and information that can be used to lobby for more inclusive policies, more funding, and so on (Wirtz et al. 2013). This strategy has borne fruit, at least partly due to Malawi's dependency on donors and international actors who are sympathetic to the cause of gay rights in Malawi and in the wake of NAC-gate, a scandal that compelled the state organization to make explicit in proposals to the Global Fund its commitment to key populations including LGBT persons living in Malawi (Wroe 2012; Chanika, Lwanda, and Muula 2013; Biruk 2014a).[24] Thus MSM are marginalized but not marginal to the global AIDS response, largely due to the role of international actors in developing and diffusing the MSM category, which produces an array of social relations and transactions in the policy-research nexus (McKay 2016; see also van de Ruit 2012, on the category "orphan" in South Africa).

The Policy-Research Gap

Thus far, this chapter has taken interest in how, where, and why data become evidence, and, in particular, how quantitative data make their way (or do not) into policy in the era of evidence-based policy. I have shown, through the presentation of the case of Blessings's ghost numbers and Felix's failed numbers, the processes—external to the data itself—that determined whether and when data became evidence that could justify decisions whether or not to include cultural practices and MSM, respectively, in national policy as sites of intervention and attention. While it is clear that "numbers are god"—as a Malawian colleague at the Centre for Social Research told me in 2007—in global health worlds, they also require specific cultural and social scaffolding or packaging in order to perform their "god trick," that is, to appear completely and autonomously detached from their context of production or from the subjects who handle and use them (Haraway 1988, 582).

Evidence, especially in the form of statistics, is often used to rationalize action or intervention, and its construction often eliminates the background factors and processes that elevate it to be taken seriously or for granted. As Goldenberg (2006, 2623) suggests, biases that underlie the processes that characterize evidence's context of discovery are often eradicated from the "purifying process of the context of justification." A closer ethnographic look at the everyday, mundane ways in which two kinds of claims made it into national AIDS policy indicates, however, that the path between the office where numbers are made from raw data collected in the field and the downstream sites of the policy-research nexus is not straightforward. In the case of cultural practices, for example, we observe how a knowledge claim continues to find its way into national policy, despite the absence of high-quality data that we might expect would be needed to justify its inclusion. Evidence, in this case, takes the form of ghost numbers. Meanwhile, in the case of the long noninclusion of Malawian MSM in national policy, we observe how the presence of well-collected, clean numerical data failed to serve as convincing evidence in front of audiences ranging from local community-based organizations to national-level policy makers. In both cases, numbers—even as they are now outside the hands of data collectors—continue to be cooked as they move further along their life course and into policy or papers. Numbers, despite their power, are not endowed with fixed authority but are enlisted into ongoing contests of credibility between social actors and within performative contexts. Notably, credibility contests in the policy-research nexus not only arbitrate the value of numbers or other evidence by assessing their proximity or distance from shared scientific standards, but also reveal the ever-shifting interests of the actors who enlist them into claims.

In the examples presented thus far, the ideal of good research making its way into national policy often faces challenges when it enters the local networks and social relations of the policy-research nexus: there is a gap, I suggest, between research and policy, confounding the underlying assumption of evidence-based policy making, as articulated nicely by an officer at Malawi's NAC: "[Policy and research] is a constant back and forth. Back and forth."[25] Yet this officer, and many other Malawian and foreign researchers, donors, and policy makers, agreed that the policy-research gap was a major problem in need of attention. Closing the gap was very much on the minds of actors in global health worlds in 2008 and continues to be up to the present. This gap is conceived of as a space of nontranslation, a chasm of sorts, between policy makers and researchers, between those who would use and those who produce data. Closing this gap has been prioritized in international and national research and development agendas. For example, the National HIV/AIDS Action Framework (GoM 2004, 35–36) active in 2007–2008 included in its budget funds for research and development, and the relevant section of the framework emphasizes dissemination of research findings that can inform programming and interventions, evaluating policy making and program development in light of research findings, and presenting summaries of authenticated HIV/AIDS research to decision makers and policy makers. This investment, monetary and rhetorical, aims to build bridges between research and policy via dialogue, translation, and dissemination of information.

Dialogue between policy makers and researchers is framed as a key antidote to the gap. The investment in dialogue often results in forums such as conferences, advisory boards, partnerships, or workshops where both sides in the policy-research nexus can effectively communicate, share information, and network, despite their differences. Members of both sides articulated the nature of the gap between them. A clinical researcher, Dr. Hanson, for a major tropical medicine research collaboration between a European university and Malawi's College of Medicine, suggested, "Malawi's no different to the U.K. in that policy makers want quick answers . . . their focus is not on scientific rigor; their focus is on access to some information that will allow them to make a decision quickly. . . . I think the policy makers see [researchers as] a lot of ivory tower–type people who lack a perspective on real life, and probably academic researchers see policy makers as sort of politically driven, affected by winds of change, people who just shoot from the hip."[26]

His comments on the differences he sees between policy makers and researchers serve two functions. First, they reinforce the gaps between policy and practice or policy and research. As he explains it, the needs, interests, and orientations of policy makers and researchers are divergent. The former are "affected by the winds of change" and "shoot from the hip" and the latter "lack a perspective on real life," stuck as they are in an "ivory tower." The kinds of expertise inherent to each category of person relies on binaries similar to those that differentiate the foreign and Malawian collaborators with MAYP and LSAM we encountered in chapter 2: whereas policy makers and those preoccupied with the real world might collate or refer to studies or research in their policy making, they are not the ones who engage in the intellectual labor necessary to produce good data and may even be unable to differentiate between good and bad data. His comments suggest similar dynamics between researchers and policy makers in the United Kingdom and Malawi, but they also index the inequalities between Malawian researchers who feel they are mere rubber stamps on proposals and foreign researchers who enjoy the time and resources necessary to engage in basic academic research. Further, while research in Malawian global health worlds often carries an implicit association with foreigners, policy denotes the nationally bounded container of Malawi and refers to the technocrats who aim to govern it.

Later, Hanson reflected explicitly on what he termed the "policy-research gap": "What we don't have is a good, frequent dialogue between ourselves and policy makers. There's an initiative . . . to develop research infrastructure [and] to improve the communication back and forth between policy makers and researchers. . . . But of course it has to be two ways. We [the project] try to send representatives [to relevant conferences] whenever possible. I hope our science communication officer we just hired will open some of those channels."[27] The differential habitus of the ideal-type researcher and policy maker he mentions contributes, then, to the lack of dialogue between the two. The closed channels that impede effective back-and-forth between them are framed by Hanson—and my other informants—as a problem in need of solutions to open channels and close the gap. In the case of his project, a technical working group meant to improve communication, a research capacity-strengthening initiative, and the research partnership itself were cited as initiatives to improve communication. These efforts mirror the capacity building of projects such as LSAM and MAYP, indicating that the policy-research gap is likewise a gap between wealthy projects and researchers and Malawian collaborators, whether researchers or policy makers.

On the other side of the gap, policy makers likewise identified a communication problem. One self-identified Malawian policy maker called Mr. Manda, whose main task is compiling and synthesizing research studies to inform policy, told me, "[There is] antagonism between policy makers and researchers. Researchers [in the past] were sort of standing aloof. . . . 'We are the academicians' and what have you. [There is] very little effort to involve the policy makers, but nowadays . . . when you are setting the research agenda, the policy maker[s] are [involved]. Everybody is involved. So when a piece of work [research] is done, it's something the policy maker was already looking for. So it's easy now to get [research] into policy."[28] He provided a specific example of how research gets into policy: "This afternoon we are leaving for Mangochi [a lakeside town in Malawi]; we are going for a think tank meeting because we want to develop an HIV prevention strategy. What should the country do in terms of HIV prevention? . . . We [draw on] different studies that have been conducted, such as an intensive study that covered all areas of HIV in Malawi. We will use . . . a number of research documents pertinent to the development of a good HIV prevention strategy."[29]

Manda's characterization of the policy-research gap resonates with Dr. Hanson's: researchers "stand aloof," which creates antagonism with the more practically minded policy makers. He notes, however, that this antagonism is on the decline and suggests that policy makers are now more meaningfully involved in ensuring that research will be useful prior to its execution. Interestingly, the idiom he uses for research ("a piece of work") points to commissioned research, which implies direct communication between researcher and end user, who directs the kinds of questions and methods necessary to answer a specific question. He suggests that he and his fellows at the think tank meeting will draw on "different studies" and "research documents" in charting a way forward for Malawi's HIV/AIDS fight. It is important to note, however, the diverse kinds of research and studies carried out in Malawi and by whom.

As discussed in chapter 1, many Malawian experts, including faculty members at the universities, find work moonlighting as project consultants. Such consultancies pay handsomely and, as Dr. Mponda suggested, are easier to secure and more quickly carried out than the kinds of research conducted by LSAM or MAYP, for example. A researcher hired to evaluate whether an NGO's home-based care intervention is working or not, or to conduct a literature review of a certain topic, for example, has a short deadline by which to complete the labor and submit a tangible report. These commissioned studies are more accessible to those who will be meeting in Mangochi than the published papers of LSAM, which find homes in academic peer-reviewed journals locked behind paywalls.[30] The form of the peer-reviewed article does not necessarily compel policy implications or recommended interventions by researchers, except perhaps as an afterthought in the concluding paragraph, as is evident in two published articles that draw on data collected by LSAM and GSIP in 2004–2006 and 2007–2008, respectively (Hennink and Stephenson 2005). Angotti et al. (2009, 6) suggest that confidential, convenient (door-to-door) HIV testing should be widely implemented to increase testing acceptance, and Baird et al. (2011) gesture toward policy makers in concluding paragraphs subtitled "Concluding Discussion and Policy Implications." Both journals, *Social Science and Medicine* and the *Quarterly Journal of Economics* were only accessible to the author via password.

Many of those present at the meeting in Mangochi have likely worked as consultants to many different projects, and are more likely to draw on that knowledge and experience—or that of close friends and colleagues who have done the same with other organizations—than they are to draw on findings that have been validated by rigorous disciplinary standards governing peer-review publication but are largely inaccessible to them, as both producers and consumers of knowledge who often face "nondiscursive" impediments to having their work published by journals based in the West (Canagarajah 1996). Finally, the very form of the commissioned report makes it easily accessible to people like Manda who must quickly get a sense of the field in his role as a policy maker. When one has little time, executive summaries and short reports are much more useful than jargony and lengthy research write-ups, though there is no guarantee that even the most efficiently packaged studies will be read or will come to inform policy (Justice 1986; Hennink and Stephenson 2005; de Waal 2015).

In recent years, there has been increased interest in creating synergy between researchers and policy makers, and in training the latter to quickly assess whether evidence is good or bad (e.g., the Knowledge Transition Platform in Malawi, a partnership between a medical and research NGO and Malawi's Ministry of Health; Berman et al. 2015). Finally, this kind of research, carried out and written up rapidly, accumulates quickly and circulates more easily than the more familiar long peer review process. Reports such as those to be studied in Mangochi are known as "gray literature," documents that are not formally published, not peer reviewed, transient in nature, and difficult to locate due to lack of an archive or incentive to preserve them (Gray 2013). Nonetheless, this gray literature would be highly accessible to local policy makers, many of whom might in fact be incentivized to attend meetings and workshops funded by donors where results are distributed. Conversely, noncommissioned (academic) research such as that of LSAM and MAYP is limited in its distribution to peer-reviewed journals or academic conferences, neither of which Malawian researchers nor policy makers are likely to have access to.[31]

The lack of a central storehouse in 2008 for research findings made accessing studies a piecemeal affair, even for a consultant hired to collate and review research on Malawi conducted in a set time frame and to identify gaps in need of attention (Mwapasa 2006). It is clear that policy is informed by evidence, but that what counts as evidence in the policy-research nexus is a social artifact, reflective of the social positions, interests, and economic constraints of those who craft it in social relations. As Feierman (2011) has shown for the case of clinicians working, respectively, in African government and American university hospitals, different concepts of evidence are not a result of culture but of the material conditions under which evidence can be put into action.

Closing the Gap?

Even as my informants across the policy-research nexus acknowledged the policy-research gap, they also invested time, energy, and funds in closing the gap. The connotations of the gap, as well, are capacious and exceed its reference solely to the chasm between the researchers and policy makers I met. Indeed, the gap speaks more generally to the divide between theory and practice, basic and applied research, and wealthy and poor countries. The NAC Zonal Conference where Felix's numbers failed, for example, is a symptom of government efforts to make their results more accessible to a broader range of participants, including those most affected by the knowledge and policies usually presented behind closed doors. As we saw in chapter 3, researchers are increasingly held accountable by their research subjects, who call upon them to share the information they collect, to invest more meaningfully in communities where they work for long periods of time, and so on. Academic research projects such as LSAM have made efforts to share the data they collect more meaningfully, to build the capacity of their local collaborators, and so on. For example, LSAM researchers consistently present findings at local AIDS conferences sponsored by NAC and the National Research Council. In March 2016, LSAM—with the help of funding from the Economic and Social Research Council—held a conference at the University of Malawi's College of Medicine on how longitudinal research might inform health and family policies after the peak of the AIDS epidemic. The conference included international and Malawian researchers, and focused on presentation of evidence that was "of potential importance for policy makers to develop new policy agendas to address . . . shifting health and demographic patterns."[32]

With help from a research project he consulted with, a senior colleague at the University of Malawi was funded to spend four months as a visiting scholar at a U.K. university. Though this opportunity was meant to allow him time to work on "[his] own projects" and have at his disposal the library and other resources of a major university in the global North, he recalled how his faculty host failed to make him feel welcome. He said he was given an office, but that it was largely useless to him because he didn't receive his school identification card for weeks and couldn't access the Internet on campus. He said he spent much of the four months seeking company with fellow Malawians not affiliated with the university but living nearby.[33] Endeavors such as these, and the many others like them, indicate the continued emphasis on closing the gap, networking, increasing dialogue, and information sharing and attempts to bring LSAM's findings in front of policy makers rather than storing them behind passwords in elite demographic journals. As mentioned earlier, LSAM's data are likewise publicly accessible for use.

Formal initiatives to close the gap—increased workshops, conferences, meetings, committees, and novel forms of data sharing—might paradoxically serve to exacerbate it: While these efforts appear to convincingly fill or shrink the gap, their effects are likely largely cosmetic, because they fail to address the larger structural politics that produce global health worlds as lopsided sites of collaboration and partnership. As Riles (2000) has shown, the discursive premium placed on networking is so high that quantity is emphasized over quality of such human connections. Klenk, Hickey, and MacLellan (2010, 954) subjected a large research network to social network analysis, finding that the benefits of belonging were unevenly distributed among different collaborators. This, too, is the case in global health research worlds, where one's relative benefit from and investment in research itself reflects one's position in a larger social field. Indicators used by NAC or research projects to measure improvements in communication and dialogue between policy makers and researchers include numeric counts of fora engaging policy makers and researchers. The fundamental knowledge structures that marginalize researchers in the global South, produce policy makers as mere wonks unable to properly assess or enlist good numbers or evidence, and maintain hierarchies of knowledge and power in global health worlds are not addressed by such broad metrics focused on countable measures of success.

Creating dialogue depends firmly on both parties being on equal footing. Policy makers' and researchers' different interests and habitus reflect the terrain of the social field in which they are formed, further visibilized by the politics of knowledge production in the policy research nexus. Foreign researchers for LSAM and MAYP, for example, are first authors on publications in prestigious journals in demography or economics, continue to attract funds for innovative research proposals, and make substantial decisions regarding data collection in Malawi. Malawian researchers, meanwhile, are second or third authors at best on such academic papers, flit from project to project and consultancy to consultancy, lack skills and time to write competitive proposals of their own, and become glorified policy makers. To measure to what degree the policy-research gap is shrinking, NAC also deploys indicators to count the number of policies that are informed by evidence. Yet even as policy may enfold more evidence, the specific nature of the evidence is left unevaluated, and the persistent gaps between North and South, academic journals and gray literature, and academic and applied research are reproduced even as indicators and metrics may perform their amelioration.

As this chapter has shown, the evidence produced by research does not come to those who would use it fully formed. Instead, it is cooked through interested performances and relations. Numbers indeed have a hegemonic grip on the imagination of actors ranging from demographers to policy makers, but numbers require packaging, props, framing, and translation to travel across boundaries and communities of practice or knowledge (Peterson 2009, 42). Whether numbers or other data become evidence is not just a factor of their epistemological rigor (Behague et al. 2009). Hodzic (2013) traces the interconnected and dispersed mechanisms of policy authorship by uncoupling acts of writing and interpretation of evidence from sovereign subjects. Whether our interest is in how numbers travel (or do not) into policy, or in how numbers are assembled in the field and travel to the office, nobody is fully in control (Hodzic 2013, 104); instead diverse actors along data's life course leave their mark on data that are variously described as cooked, clean, raw, or dirty. While evidence carries connotations of transparency, neutrality, and objectivity, and presumes clean data, this chapter has shown that evidence, quantitative or not, is as cooked in its sites of consumption as it is in its sites of production.