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The Production and Circulation of AIDS Knowledge in Malawi

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Abstract
As the AIDS epidemic continues to spread across Africa, a demand for evidence produced by policy-relevant research means that expatriate-led research projects have become a fixture in highly infected countries. While many have drawn attention to the social and economic consequences of AIDS suffering, few have documented the everyday practices, contradictions and politics of producing AIDS-related knowledge in impoverished contexts. This study examines the ways in which AIDS survey research projects in Malawi produce new socialities and mobilities, generate new exclusions and inclusions and reconfigure expertise and evaluations of knowledge. Rather than focusing on a single knowledge community, the study follows AIDS knowledge itself as it is formulated and circulates through sites of production (the "field"), conversion and manipulation (the office) and consumption (conferences, journal articles or other forums). Drawing on twenty months of ethnographic fieldwork in 2005 and 2007-08 with case study research projects, researchers, fieldworkers, rural research participants and policy makers in Malawi, this study examines how actors' positioning within the social field of "AIDS research" informs their stakes in research and analyzes the tactics they employ to achieve them. Central to the study is an illustration of how boundaries and differences (between people, knowledge and context) are produced and negotiated within interactions between actors with multiple and crisscrossing commitments, interests and ideas.

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The Production and Circulation of AIDS Knowledge in Malawi

To my grandmother, Else Klimeika

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ABSTRACT

THE PRODUCTION AND CIRCULATION OF AIDS KNOWLEDGE IN MALAWI

Crystal Biruk

Sandra T. Barnes

As the AIDS epidemic continues to spread across Africa, a demand for evidence produced by policy-relevant research means that expatriate-led research projects have become a fixture in highly infected countries. While many have drawn attention to the social and economic consequences of AIDS suffering, few have documented the everyday practices, contradictions and politics of producing AIDS-related knowledge in impoverished contexts. This study examines the ways in which AIDS survey research projects in Malawi produce new socialities and mobilities, generate new exclusions and inclusions and reconfigure expertise and evaluations of knowledge. Rather than focusing on a single knowledge community, the study follows AIDS knowledge itself as it is formulated and circulates through sites of production (the "field"), conversion and manipulation (the office) and consumption (conferences, journal articles or other forums). Drawing on twenty months of ethnographic fieldwork in 2005 and 2007-08 with case study research projects, researchers, fieldworkers, rural research participants and policy makers in Malawi, this study examines how actors' positioning within the social field of "AIDS research" informs their stakes in research and analyzes the tactics they employ to achieve them. Central to the study is an illustration of how boundaries and differences (between people, knowledge and context) are produced and negotiated within interactions between actors with multiple and crisscrossing commitments, interests and ideas.

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It is market day at Mangochi turnoff in southern Malawi and the small trading center is bustling with activity. Buyers and sellers of *zovala* (second hand clothes), sneakers, vegetables, printed fabrics and batteries haggle over prices and socialize, creating a low buzz of voices against a backdrop of persistently blaring mini-bus horns. On a sunny June morning, I walk a short distance from the busy trading center. Passing an open-air butcher shop where young men sit idly beneath a tall tree hung with two goat carcasses, I arrive at a large compound. Surrounded by walls painted with bright advertisements, a squat rest house sits back from the open gates: the rest house, a favored stop for truck drivers, is called *Mpaweni*, or "No-Man's Land."

Though the compound is quiet, there is no vacancy at this motel. Its rooms have been taken over by the fieldwork teams of an international survey research project that is in Malawi to study the role of social networks in response to AIDS. In the next six weeks, the fieldwork teams will survey and HIV-test over 1000 Malawians living nearby. The nicer, "chalet" type rooms are occupied by American researchers and graduate students and Malawian fieldwork supervisors. The "inside," small and dilapidated rooms house the Malawian interviewers, data entry clerks and drivers, some of whom stay at another motel nearby. From a vantage point in the dirt courtyard, a visitor might not notice that one of the motel's conference rooms has become a makeshift field office. Data entry teams tap at the keyboards of project-owned laptops, manually transferring data codes from the dusty pages of completed surveys to a growing database. Boxes of Lifebuoy body soap and Sunlight laundry soap are piled neatly around the periphery of the room;

these bars of soap are the gifts that will compensate research participants for answering the questions that comprise this year's three-hour survey. A photocopier and printer whir quietly. Electrical cords snake underfoot, ending in overworked power strips that protect the electronic devices from the periodic power surges and outages common in Malawi. Parked helter skelter around the compound are a dozen minibuses that carry interviewers Monday-Saturday to the project's sample villages: one by one, these interviewers visit the individual households that comprise the sample.

This motel is a temporary but central site in this project's production of knowledge about the AIDS epidemic. The data collected from participants and converted into organized databases will eventually be transformed into claims such as "11 percent of Malawians are infected with HIV"[1] or "45 percent of HIV-infected respondents report that they have 'no likelihood' of being infected in two years."[2] In "No-Man's land," knowledge is being made. In the words of local residents who notice the visitors around town, "*Akafukufuku abweranso*!" [The researchers have come again!] *Akafukufuku*, in this case, encompasses not only *azungu* [foreigners]—American social scientists and graduate students—but also Malawian AIDS research collaborators, and Malawians hired as supervisors, interviewers, data entry clerks, HIV testing counselors and drivers. The open gates of the motel and the bustling market nearby might be read metaphorically: when research projects "touch down" to collect data, they become entangled in local social worlds and enlist a diversity of people into their knowledge projects—Malawian collaborators, local fieldworkers and research participants.

[1] UNAIDS 2009
[2] Delavande and Kohler 2008

Figure 1: Storage room with boxes of completed surveys (right). The thin boxes to the left contain the bars of soap that act as gifts for research participants (Photo: J. Wood).

Amid calls for more data and policy-relevant research on the epidemic, projects such as this one maintain an entrenched if episodic presence in Malawi. The many different people who participate in the production and consumption of AIDS knowledge have different stakes in and expectations of research. Impoverished participants who answer survey questions or take an HIV test graciously accept the small gift of soap they are given and harbor speculative hopes that the project will return to "give" them even more in the future or improve their lives. The academic researchers monitor data collection and data entry in order to produce "high quality data" that becomes the foundation of future publications and proposals; most are also motivated by a desire to

help their research subjects. Similarly, Malawian collaborators to these expatriate-led projects hope research will help develop and improve living conditions in their country. Further, the material and social capital they accrue as collaborators can help them financially in a national context where funding for academic work and teaching is scant. Fieldworkers view research as a relatively stable source of income and potential social mobility amid high levels of unemployment. Finally, policy makers who consume the data produced by projects such as this one seek better ways to incorporate findings from AIDS research into evidence-based policy.

In this study, I describe and analyze the social organization of international AIDS research projects. How do the exchanges and expectations that comprise these projects influence the production, circulation and consumption of AIDS knowledge? How and when do competing interests in research experience friction and how are differences resolved, at least temporarily? These are the main questions underlying this study. Based on research with four case study international AIDS research projects working across southern and central Malawi, the following pages illustrate how the process of making knowledge about AIDS is influenced by the tactics that people employ to further their own interests within the social field of "policy relevant" AIDS research.

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My interest in this topic arose during my first trip to Malawi in 2005 when I was working with a Malawian researcher to oversee a large-scale, ambitious inventory of the cultural practices that spanned Malawi's three (southern, central, northern) regions. This study was initiated in response to national and international discourse of cultural practices

such as *fisi*[3], *kusosa fumbi*[4] and widow inheritance[5] as risky for the spread of HIV. I was interested in the friction or overlap between local and global perspectives on key drivers of the HIV/AIDS epidemic. However, I soon realized that differing perspectives on HIV were present in more mundane and small-scale contexts.

In August 2005, I overheard a heated discussion between the American principal investigator (PI) for an ongoing survey project in Malawi and her Malawian collaborator. The Malawian co-PI had allegedly hired one of her relatives as a driver and then looked the other way when he used project vehicles and petrol to conduct personal business unaffiliated with research. This followed on an earlier discussion between the same two researchers in which the Malawian co-PI felt strongly that culture was a major risk factor for AIDS while the American collaborator felt differently. Having spent many months among AIDS researchers, interventionists and policy makers in sub-Saharan Africa at that point, the argument I overheard fit a larger pattern of observations: collaborators from different cultures often disagreed and the resolution of these arguments was shaky and fragile. While proposals and official discourse emphasize collaborative and participatory research, fundamentally different ideas about research, sharing of material resources, objectives and the epidemic itself often manifested in arguments or fractures within collaborative projects.

The projects described in these pages operate within a larger, transnational field of knowledge production about global health. They are part of a growing "invasion" of

[3] In English, *fisi* means "hyena;" this cultural practice typically entails the relative or friend of an infertile man coming to his house under the cover of night to sleep with his wife and impregnate her. If a child results from this union, she is considered the child of the woman's husband.
[4] In English, *kusosa fumbi* means "cleaning away the dust;" in practice it entails a girl who has just left the female initiation camp having sexual relations with a male in order to complete her initiation and passage from childhood to adulthood.
[5] Widow inheritance commonly entails a woman who is widowed undergoing sexual cleansing with a man (usually her brother-in-law). In a time of AIDS, this practice has come under scrutiny across sub-Saharan Africa.

Africa by international scholars, medical students, gap-year volunteers and "do-gooders." Scholars suggest that the continent's latest export is information for university-based and other researchers (Janes and Corbett 2009:176). They describe anthropologists' recent interest in this aspect of health this way:

> Global health is an area of research and practice that endeavors to link health, broadly conceived as a dynamic state that is an essential resource for life and well-being, to assemblages of global processes, recognizing that these assemblages are complex, diverse, temporally unstable, contingent, and often contested or resisted at different social scales (2009:169).

Within this field, scholars have focused on local health or social inequalities in relation to the international political economy (Baer 2003, Farmer 2004, Heimer 2007), assumptions that underlie ideologies of health development (Briggs and Mantini Briggs 2003, Rivkin-Fish 2005) and the historical context (Vaughan 1991, Livingston 2005, Fassin 2007). They have critiqued the political-economic relations that influence policymaking and contribute to problems of localizing universal policies (Falk Moore 2001, Biehl 2007, Taylor 2007) and explored the social consequences of the increasing involvement of a wide variety of actors in global health such as NGOs, pharmaceutical companies, bilaterals and public-private partnerships (Pfieffer 2003, Vogel 2006, Kickbusch *et al* 2007, Adams *et al* 2008).

Within the field of anthropology of global health, this research contributes to discussions about the circulation and localization of what science studies scholars have termed postcolonial technoscience (Anderson 2002). As it pertains to health, this includes: technical objects such as medicines, prosthetics and HIV tests; people such as physicians and scientists; and techniques such as surgical procedures, randomized controlled trials (RCT) and viral load counters. Further, postcolonial technoscience is a

cluster of shared epistemologies and definitions of science that are often developed or legitimated in the global North and exported to the global South. The term postcolonial effectively draws attention to the ways in which benefits associated with technoscience are unevenly distributed across spatial geographies enmeshed in particular histories of exploitation, extraction and colonial rule. From this perspective, scholars have explored the "ethical variability" of biomedical research, especially clinical trials, in particular contexts (Petryna 2009, Crane 2010). Employing its prevailing analytic, the anthropology of global health has focused on the free and intensified transnational circulation of health images, bodily tissues, people and medicines. Scholars have shown that representations of sickness and health are often informed by the colonial imagination of the non-Western body (Comaroff 1993, Pigg and Adams 2005) and drawn attention to how existing health and economic inequalities enable global trade in human organs and medical tourism (Scheper-Hughes 2000, Cohen 2005). Still others take interest in the migration of doctors and nurses from impoverished contexts (Lwanda 2005, Wendland 2010). Finally, anthropologists have shown how the circulation of expert knowledge, therapies and policy categories creates new social movements and mechanisms for achieving biological or therapeutic citizenship (Epstein 1996, Petryna 2002, Biehl 2007, Nguyen 2010). Alongside this proliferating panoply of actors, knowledge and objects seeking to improve global health, scholars have also documented parallel occult economies, conspiracy theories and supernatural stories centered, for example, on the extraction or stealing of blood (Kaler 2004, Geissler 2005, Fairhead *et al* 2006).

Despite the recent growth of studies cohering around the anthropology of global health, scholarship in this area disproportionately focuses on technoscientific projects that

produce or rely on *biomedical* knowledge. The circulation of blood, organs, tissue samples, ARVs and generic malaria drugs has rightfully captured the imagination of anthropologists, eclipsing the more mundane objects (surveys, anthropomorphic measurement tools, maps, databases), techniques (random sampling, Chi-squared tests, household surveys) and people that make up *social* scientific postcolonial technoscientific projects in Africa. Further, in studying the production and circulation of expert knowledge about global health, anthropologists have viewed knowledge-making within the framework of single, bounded epistemic communities or settings or relied on analytical categories such as "local" and "global" to designate forms of knowledge and describe their syncretism, interaction or interpenetration. Often, this presumes that "global" science, biomedicine or "ethics," for example, are constructs that need only be localized within specific contexts. As yet, there is no ethnographic study that considers how information about health and illness produced by social scientific research projects operating in postcolonial contexts is an artifact of a particular "social field" with its own social organization, rituals, exchanges, practices and negotiations.

Thus, rather than immerse myself in a single knowledge community, I tried to follow AIDS knowledge itself as it was formulated and circulated through sites of production (the "field"), conversion and manipulation (the office) and consumption (conferences, journal articles or other forums). This approach allowed me to observe the dynamics, relations and social organization within and across groups of actors—expatriate and Malawian researchers and policy makers, fieldworkers, data entry teams, and research participants—and how their positioning within the social field of "AIDS research" informed their stakes in research and the tactics they employed to achieve

them. In particular, I examined how larger political economic, ethical and humanitarian forces bear on these everyday practices. In this way, knowledge claims—in the form of statistics, statements about risky cultural practices or risk group identifications—are not stable or bounded but dynamically shaped by social processes within and outside their site of production.

Conceptual Frame

The increasing penetration of Malawi by global health knowledge-projects must be situated in a long history of the country as a site that intersects multiple forms of knowledge and sorts of expertise about health and illness. Consistent across Malawi (and much of Africa) is a conception of individual health as inseparable from social health. A person can fall ill with malaria or be bewitched; in the case of the latter, the diagnosis brings about moral speculation regarding who "sent" the illness. Healers occupy a precarious place in the moral imagination; their patients and neighbors may accuse them at any time of using their healing powers toward nefarious ends. These moralized accusations were leveled against colonial doctors; the tools of their trade such as injection needles or stethoscopes often took on occult functions in circulating stories that acted as idioms for coming to terms with new knowledge and technologies (White 2000). Accounts of interactions between *asinganga* (traditional healers) and physicians in Africa or between colonial medical officials and villagers tend to rely on a local/global or traditional/modern analytic that views the social groups as autonomous and in possession of unique epistemologies and knowledge that can be fused or selectively and pragmatically combined (Feierman 2000, Luedke and West 2006). In what follows, I

build on these accounts by showing that boundaries and differences are produced only within interactions between actors with multiple and crisscrossing commitments, interests and ideas. Research projects do not just "collect" knowledge from research participants by writing down how many times a person had unprotected sexual intercourse last month or recording her recent illnesses; this knowledge is made, performed and recorded within social relations and in a larger social field of individuals and groups.

I engage my analysis in different social contexts, shifting from documenting features of particular and bounded ways for producing and evaluating knowledge to examining how the politics of making knowledge unfold in a cosmopolitan environment. To loosely draw together the diversity of actors who comprise this environment, I employ Bourdieu's (1977, 1993) idea of the social field as a structure ("a multi-dimensional space") of social relations "felt" by all who belong to it; these relations determine the possible forms of interactions and mobilities available to interactors. I consider "AIDS research" as a social space in which every position depends on those in other positions in the field. My focus on the "AIDS research field" entails viewing negotiations, alliances or friction between actors involved in research as "interested" and situated (cf. Haraway 1991). In this way, researchers cannot be understood except in relation to the research subjects against whom they are defined.

This approach permits me to view knowledge claims made within this social field as interested performances for specific audiences at specific times. Knowledge is the use of evidence to make a claim within a specific set of social relations. While the most obvious kind of claim is spoken or written (such as: "More resources should be directed to interventions that seek to prevent mother-to-child transmission of HIV/AIDS in the

central district of Malawi") or contained in an image (such as a graph of rising HIV prevalence rates), knowledge claims need not be so explicit or visible. They might be embodied, as when a rural Malawian hides from a research team that wishes to survey him or test his blood; they can be tacit, as when a respondent cannot or will not answer in the face of a survey question about the details or meanings of a recent initiation ceremony; or they can be silent, as in the case of the Malawi National AIDS Commission's (NAC) silence about the risks faced by men who have sex with men (MSM).[6] Each of these kinds of claims enlists evidence, performs knowledge, seeks credibility and affects future relations or social action in a social field. A claim is simultaneously new and old; it is recognizable in a long trajectory of other claims about the same matter but departs from them enough to appear novel, different or worth considering. If we presume evidence or knowledge as stable or universal, we miss the spectrum of practices and performances by which knowledge and evidence are made. Finally, this study addresses and questions accounts of the globalization of knowledge and technoscience. While the movement of people and things—often described as knowledge sharing, collaboration or translation—breaks down borders and connects an increasing diversity of actors, I show that knowledge production across national, cultural and social borders sharpens and redefines existing boundaries and produces new ones.

[6] Although MSM were mentioned as early as 2003 in Malawi National AIDS policy ("Persons engaged in same-sex relations"), it is only recently that this risk group has attained public attention; this is likely to channel pooled government funds in their direction (GoM 2003).

Ethnographic Context

Malawi is a small, landlocked sub-Saharan African country with a population of 15.4 million.[7] Its mostly rural population engages in small-scale farming and depends heavily on rain-fed agriculture to grow maize to prepare the staple food dish, *nsima*. The main earner of foreign currency is tobacco, sold annually in bales on the auction floors in the city to eager buyers; however, unpredictable and wildly fluctuating prices make conversion of a food garden into a tobacco garden risky for a rural household. Malawi is also Africa's second largest producer of tea, after Kenya (See Figure 2).[8] Malawi's position as one of the poorest countries in the world, its high rates of unemployment, and the failure of structural adjustment programs (SAPs) instituted by the World Bank and IMF since 1981[9] have likely exacerbated its AIDS epidemic: it is the eighth most infected nation with prevalence rates hovering around 11 percent.[10]

[7] NSO 2008
[8] NSO 2005
[9] Amid global economic recession and the "oil shocks" of the 1970s, Malawi's SAPs had industrial and manufacturing growth at their center (Munthali 2004). As an agrarian economy dependent on external factors like climatic changes and international terms of trade, Malawi faces volatilities in exchange rates, inflation levels, interest rates and GDP growth rates; this made sustainable manufacturing industry growth difficult. Though SAPs did produce some minor growth in the industrial sector, critics argue that the IMF's suggestion that the government not invest in fertilizer subsidies was ill-advised and threatened to thrust almost half of the population into famine. Against expert economic advice and amid US and British skepticism, current president Bingu wa Mutharika has indefinitely reinstated the subsidies (Dugger 2007).
[10] UNAIDS 2009

Figure 2: Tea plantations in Thyolo, Malawi (Photo: Author).

My case study research projects are situated within a massive infrastructure of knowledge production, prevention, treatment and containment that takes AIDS (amid other health and social problems) as a central concern.[11] The common vernacular terms for this disease—*edzi* (a variant on the Anglophone "AIDS") and *matenda a boma* (the government disease)—hint at the widely shared conception of AIDS as an object of interest for foreign and national authorities.[12] My informants widely associated "AIDS" with the Chichewa term for research (*kafukufuku*), pointing to the long history of efforts,

[11] Sridhar and Batniji (2008) provide a critique of global over-investment in AIDS research and prevention at the expense of other diseases.

[12] When AIDS first arrived in Malawi, many considered its symptoms to be manifestations of *tsempho* or *kanyera*, existing sexual diseases caused by the transgressing of boundaries or breaking of taboos. Other initial names for the disease included: *magawagawa* (that which is shared), *chiwerewere* (promiscuity), *kachilombo koyambitsa a matenda a edzi* (a small beast that brings AIDS), or, simply, *kachilombo* (the small beast) (Moto 2004, Lwanda 2005, author's field notes; 2007-2008).

usually by outsiders, to document and contain the HIV virus through collection of information, anthropomorphic data and bodily fluids. Since the first public announcement of the epidemic's presence in Malawi, AIDS attracted money, resources, treatments and experts from within and outside the nation. However, the explosion of AIDS infrastructure in Malawi happened rather late.

Although Malawi's "silent epidemic" probably began before 1980 and the first case was diagnosed in 1985, a strict ban imposed by post-independence "life president" Dr. Kamuzu Banda on discussing family planning (until 1982) or social problems that would challenge his discourse of Malawi as his land of "milk and honey" (Lwanda 2005:39) prevented the topic from becoming a point of public discussion until much later (GoM 2003, Illife 2006). While Banda did establish a short-term plan by mid-1987 and set up the National AIDS Control Programme (NACP) in 1989, the mandate and objectives of this programme [sic] were impeded by political stagnancy on the issue.[13] By 1993, an external review of the AIDS response indicated patchy attendance at infrequent NACP meetings (Wangel 1995:26). Donors and the Global Program on AIDS (GPA) played a central role in pushing the growing epidemic onto the government's agenda; with democratization in 1994 and newly elected president Bakili Muluzi's public prioritization of AIDS,[14] international organizations began unimpeded and intensive work in this arena, complemented by an enhanced governmental response led by National AIDS Commission, established in 2001. In 2006/07, the Global Fund was the largest

[13] During these early days of the epidemic, only one (US-funded) research project on AIDS was permitted to work in Malawi; all others who requested research permission were refused (Wangel 1995:25).
[14] The AIDS response was intensified only slowly under Muluzi. AIDS was recorded as his party's fourth priority and most posts in the AIDS Secretariat were vacant (Wangel 1995:26).

funding agency for the HIV and AIDS program in Malawi, contributing 77 percent of the total budget (Mwapasa and Kadzandira 2009).

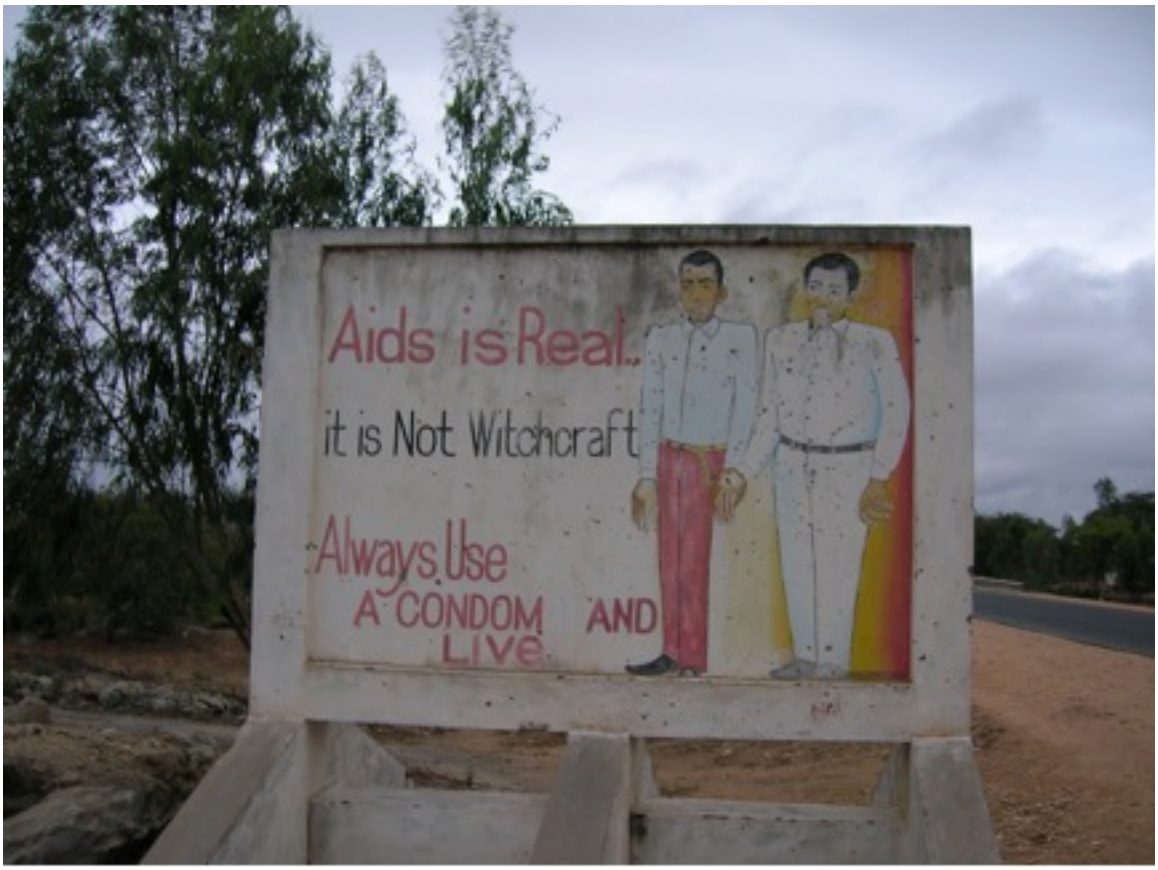

Figure 3: AIDS prevention messages circulate widely in Malawi (Photo: Author).

The social sciences have long played a central role in formulating policy and interventions into the AIDS problem in Malawi. In the early 1990s, research focused on assessing AIDS related beliefs, attitudes and practices, determining the economic effects of HIV on the population, documenting support networks' care strategies for infected individuals, identifying a wide variety of ever-shifting risk groups (adolescent girls, truck drivers, sex workers) and understanding low rates of condom use (McAuliffe 1994,

Bisika and Kakhongwe 1995, Illife 2006). In Malawi and across sub-Saharan Africa, anthropologists, demographers, sociologists, economists and other social scientists have by now become temporary residents in areas of high HIV prevalence. The researchers who led the projects I studied were PhD-holding academics trained in demography, sociology, anthropology, theology, economics and nursing.

Research Design and Methods

While this study is informed by earlier, brief fieldwork periods in both Kenya (2002, 2004) and Malawi (2005), it is based primarily on 16 months of field research in Malawi from August 2007-December 2008. Over the course of this time, I grew familiar with a wide variety of researchers, policy makers and others involved in research and intervention into the AIDS epidemic. My language proficiency in Chichewa allowed me to feel comfortable in environments where both English and Chichewa were spoken. At meetings, interviews or other places where the only language spoken was Chichewa, I worked with a male research assistant who had recently graduated from the University of Malawi and a female research assistant who had finished secondary school.

Ethnographic Research Activities

In Malawi, I spent time with four international AIDS research projects working across the southern and central portions of the country, in Zomba, Blantyre, Rumphi, Salima, Balaka and Mchinji Districts (see Figure 4). Though there were differences between these projects, all of them conducted household-level interviews (and in some cases HIV tests) with samples of about 400 to 6000 participants over two to three months

at a time. While one project had been working in Malawi for ten years, the others were more recent. Fieldwork teams were comprised of American and European researchers (intermittently) and expatriate graduate students, and locally hired Malawian supervisors, interviewers and data entry teams. All four projects studied issues related to the AIDS epidemic: the role of social networks in responses to AIDS in rural Malawi; the influences of religious leaders and doctrine on AIDS-related behaviors; the effect of cash transfers in decreasing vulnerabilities to HIV/AIDS; and HIV risk factors associated with adolescent development and marriage behavior. The projects rented a building that served as a fieldwork office, located near but typically not within the villages or neighborhoods in the study sample. Fieldwork teams left the office early in the mornings (around 6am) to collect data and returned by nightfall. I joined these fieldwork teams and participated in all aspects of fieldwork ranging from: trainings for project staff, survey design meetings, hiring of interviewers, everyday fieldwork practices, evening social events, checking surveys, mapping exercises and data entry. To grasp not only the production of knowledge about the AIDS epidemic, but also its circulation, I attended numerous international and national AIDS conferences, meetings between academic researchers, meetings between policy makers and researchers, meetings between donors and country representatives and NGO events. I conducted interviews with a wide range of people involved in the world of AIDS research including: villagers, researchers, policy makers, government ministers, ethics board members, NGO staff and district officials.

I also conducted over 40 semi-formal, audio-recorded interviews with people living in two research project sample areas, most with rural households who had participated in research within the past week (30) and a some with people outside the

sample (12), as well as informal interviews and exchanges that I recorded in field notes. Interviews usually lasted about two hours; I asked people about their personal life stories, their perspectives on AIDS and AIDS research, their knowledge of research or other projects working in their district and so on.

I also conducted more than 25 semi-structured interviews with academic researchers working on AIDS-related topics across sub-Saharan Africa. While most of these researchers were Americans, Europeans and Malawians working on social scientific and biomedical projects in Malawi, the others were American, British, Canadian, South African, and Kenyan researchers working in other countries with whom I met at an international conference in Tanzania. In these recorded interviews that lasted about one-two hours each, I sought to understand the "research life history" of the interviewee, his/her motivations for conducting research in Africa, attitudes and perceptions of collaborative research and assorted issues around capacity building, relations with research participants/community and outcomes of research.

Figure 4: Map of Malawi indicating districts in which case study projects were conducting fieldwork. Blue arrows indicate districts where I accompanied the fieldwork teams (source: IFPRI 2002).

Survey

After spending three months with my first case study project, I realized I wanted to know more about the demographics, education level and aspirations of the fieldworkers hired by researchers. With the help of fieldworkers themselves, I designed a brief survey with open-ended questions regarding age, education level, work history, future goals, perspectives on research and fieldwork and the names of projects

interviewers worked on. I administered it to over 100 fieldworkers who comprised the field teams of the four projects; interviewers filled the survey out on their own time and handed it back to their supervisors. This allowed me to get a more thorough sense of patterns I had observed among key informants of this group (see Chapter One).

Archival Research

While teaching at the University of Malawi in late 2008, I spent time in the Malawi National Archives (MNA) in Zomba, the colonial capital. Though the collections were extensive, they were poorly organized and many of the documents were damaged by weather, fire or ants. I focused my attention on understanding the nature of health and development interventions, campaigns and research in Nyasaland, mostly from the late 1920s to the early 1950s. I read reports from the colonial development public health committee, memos and minutes from meetings of the Standing Medical Research Committee for East Africa and communication between the health departments of the Rhodesias (present day Zimbabwe and Zambia) and Nyasaland (present day Malawi). I examined the minutes of district council meetings, paying particular attention to discussions of how to encourage local participation in colonial health and development projects (see Chapter Two). In Zomba, I also spent time at the Centre for Social Research's (CSR) documentation unit and in the Malawiana collection housed at the University of Malawi-Chancellor College Library. Here, I looked at research reports, evaluations and papers written and produced under the administration of CSR since the late 1970s. In these documents, I charted the evolution of social scientific research in Malawi—its focal interests, its methods, its techniques and its discourse. I read the

newspapers archive, where I looked for coverage of AIDS and episodic rumors of bloodsuckers (see Chapter Two). In the Malawiana collection, I read unpublished papers given by historians and anthropologists at the University of Malawi in the 1980s and back issues of journals.

Presentation

As mentioned, my methodology in Malawi was to "follow the knowledge." I tried to trace the formation, circulation and validation of knowledge, with particular attention to its social conditions of production. The chapters follow this progression: recruiting and training fieldworkers to collect data; compensating research participants for the information they collect; managing uncertainty that threatens to diminish the quality of data collected from human subjects; and convincing diverse audiences that the evidence underlying claims about the epidemic is sound. The chapters are unified in their shared focus on how actors negotiate competing interests in research or knowledge production informed by their position in a social field. I consider how these negotiations affect the making and evaluation of knowledge or evidence.

Chapter One focuses on the ways in which international AIDS research projects recruit local experts and how they frame research as a collaborative, participatory and relevant effort. The chapter shows that even before knowledge about the epidemic is produced, a particular social infrastructure for its production must be built. International researchers must identify and recruit, first, elite local experts as collaborators and, second, a skilled pool of fieldwork experts who can provide logistical and other local knowledge. Using the figure of the marketplace of expertise to illustrate how experts sell

their "local" knowledge to interested researchers, I argue that the contours of this local knowledge are co-produced in everyday social interactions. The tactics adopted by the local experts significantly influence the kinds of knowledge claims, evidence and data produced by these projects. Therefore, we must ask: What conceptions of local knowledge and definitions of expertise vie with one another for legitimacy? How might the collaborative social infrastructure of knowledge production influence and make knowledge claims authoritative? How has the emergent social infrastructure around the object of "AIDS" allowed some local people to be socially mobile and excluded others?

Chapter Two reckons with another transnationally salient imperative: research must be ethical and fair. I explore the ethical standards that human subjects' decisions to participate in research must not be coerced and must be consensual in order to analyze the friction between ethically collected knowledge and local conceptions of "fair exchange." Research participants are typically given bars of soap in exchange for information. Within this soap-for-information exchange, local participants make many claims: research is exploitative, soap is a meager and insufficient gift, researchers are bloodsuckers. The chapter aims to portray the interests and motivations that precipitate these claims and that inform the everyday relations between research fieldwork teams and research participants. I also challenge the misrepresentation of researchers as "powerful" and the researched as "powerless" by depicting extraction of data and production of knowledge as an ongoing negotiation.

Chapter Three moves into the office setting, centering on how data are made to circulate widely across national, regional, and transnational borders. How do representations simultaneously become detached from their anchoring social context and

retain the potency of that context in a larger marketplace of data? How is "raw" data converted into information for wider consumption? I argue that "seeing" like a researcher is a cultivated and narrowed disposition; these patterns of attention informed by epistemic virtues—such as sample size, precision, replicability and timeliness—also pattern a self (a certain kind of "knower"). Further, the chapter illustrates how seeing also implies not seeing: what remains outside the gaze of researchers or policy makers?

Chapter Four is an ethnographic study of "downstream" sites where knowledge is performed to and consumed by audiences. It is concerned with the ways in which the data collected in Chapter Three take form as the evidence that underlies knowledge claims made in the larger AIDS policy-research arena. In this chapter, a situated study of knowledge claims in action builds on Chapter Three by illustrating what evidence (specifically: good evidence) is and by showing how knowledge claims are deployed. The sites of knowledge—journals, conferences, policy meetings and policy itself—are far from the field. They are a place where patterns of evidence can be determined and empirically studied.

Taking a performative and social constructionist approach to the making of evidence, Chapter Five suggests three main criteria people use in determining whether the evidence to support a claim is legitimate. The final portion of the chapter argues that even as performances of knowledge are validated by consumers, the evidence undergirding them is encapsulated and unaddressed. I consider how an increasing emphasis on the translation of evidence—between actors, between formats and between spaces—masks what is non-knowledge and what might be termed "conventional wisdom." Chapters Four and Five link the previous chapters' interest in the practices of

research with the important and influential ways in which research's products are enlisted into specific kinds of biopolitical projects organized and indexed by transnational and national AIDS priorities, policies and interventions.

Finally, the conclusion revisits the major themes of the work, discussing how postcolonial technoscientific projects provide a fruitful site for reconsidering some of the analytics that anthropologists of knowledge have long concerned themselves with—expertise, exchange, circulation and validation. The study provides insights for reconceptualizing "knowledge" as it is produced in sites of management, measurement and prevention knit together in the "social field" of global health.

Chapter 1

Navigating the Marketplace of Expertise

"…I'm a man without conviction
I'm a man who doesn't know
How to sell a contradiction
You come and go
You come and go…"
-*Karma Chameleon,* Culture Club (1983)

In Malawi, international social science research projects "come and go" intermittently to conduct research about the AIDS epidemic. The production of knowledge by these expatriate-led projects depends fundamentally on their recruitment of local experts to "show the way" through an unfamiliar cultural and geographic landscape. The massive global turn away from top-down practices to bottom-up or collaborative research has assigned the category "local knowledge" a new value. In places like Malawi that are favored sites for research and development projects, a demand for local knowledge has created a vast marketplace where secondary school and college graduates compete with one another to sell increasingly commoditized forms of local expertise to interested outsiders. For these Malawians, convincing international researchers of their legitimate local expertise involves competently performing both authentic "local-ness" and cosmopolitanism. Local fieldworkers in Malawi have mastered these shifts in identity management and performance; they are, to adapt a popular image, "culture chameleons."[15]

This chapter shows how a number of international social science research projects affected the lives of the Malawians who worked for them and vice versa by focusing on

[15] My imagery is inspired by Fredrik Barth's (1969) focus on how individuals and groups opportunistically emphasize or mitigate certain aspects of their identities in and through interactions with others. This is a helpful frame for considering how local experts perform their own identities or expertise across social interactions with individuals from groups ranging from expatriate researchers to rural research participants. Like chameleons, local experts thrive because they are adaptable and responsive within and across multiple backgrounds or contexts.

how the social infrastructure for the production of knowledge about AIDS is built. First, I attend to historical shifts in the definition, evaluation and valorization of local knowledge that preceded the emergence of a marketplace of expertise. Then, I show how a large demand from outsiders for local knowledge in Malawi enables those competing for jobs to employ flexible tactics that simultaneously legitimate their expertise and enhance their own financial and social status. I describe local experts' investment in maintaining their ownership over local knowledge, their performance(s) of the "authentic" expertise that expatriate researchers expect and their ability to accumulate social and other forms of capital in a marketplace structured against their own interests. Finally, I argue that the efforts made by local experts to adapt to the fast-paced and standardized fieldwork context and simultaneously to reconcile their own interests with those of research participants *and* those of their employers may produce data that is "cooked," scripted or misaligned with local realities. Throughout, the chapter makes clear that local knowledge and the identities of local experts are crafted in the processes and practices of research fieldwork in Malawi.

Knowledge Brokers

Fieldworkers capitalize on and broker their ability not only to translate between researchers and their research subjects but also to skillfully maintain boundaries between these social worlds. They thrive in the marketplace of expertise precisely because they mediate between social groups and contexts and fall into a category others have described as cultural brokers, patrons, intermediaries, middle-men, translators or go-betweens (Barnes 1986, Schumaker 2001, Engle Merry 2006, Englund 2006, West and Luedke

2006, Raj 2007, Schaffer *et al* 2009). Recent accounts have described the role of local experts or translators in transnational projects of health, development, human rights or knowledge production. Schaffer and colleagues, for instance, provide case studies of "go-betweens"—"people who articulate relationships between disparate worlds or cultures by being able to translate between them" (2009:xiv)—working in various spheres of imperial knowledge production in the late eighteenth and early nineteenth centuries. This chapter employs the same focus on how local forms of expertise become commodity resources and how the work of intermediaries generates new social roles in a global economy of knowledge (2009:xxx).

Local experts occupy a translational space between the global priorities and agendas of international health researchers and the local interests and complexities of rural Malawian research participants. A niche for these experts evolved out of a post-World War II interest in improving international health that built on foreign involvement in the British colonial contexts in the 1930s-40s.[16] The rising interest of western nations in global public health in the developing world articulated with the rapid spread of AIDS throughout sub-Saharan Africa beginning in the late 1980s (Kalipeni *et al* 2004, Illife 2006). At this time, local experts found new roles as knowledge brokers amid a growing emphasis on collaborative or partnership-based models for knowledge production following critiques of top-down, "blue-print" type projects.[17] Planning interventions and crafting effective policies to mitigate the spread of AIDS necessitates the collection and

[16] A number of medical anthropologists have compiled histories of international health (e.g. Birn 1996, Baer *et al* 1997).
[17] For some exemplary critiques of top-down, colonialist development see: Stiefel and Wolfe 1994, Cooke and Kothari 2001, Cooper and Packard 2005, Edelman & Haugerud 2005. In 2003, much of the nearly five billion dollars that was spent on research and interventions in Africa went to projects that were collaborative or participatory in nature (WHO 2004, Carael 2006). Since the nineties, although there has been a steep rise in the number of internationally co-authored papers, some suggest that the main increase has been in the competition between '"large countries" for partners in the global network (Leydesdorff 2005). The HIV/AIDS Research Strategy for Malawi for 2005-07 opens research agendas and projects to local participation; the strategy calls for enhanced AIDS research infrastructure, more meaningful partnerships and collaborations between international and Malawian researchers and wider dissemination of AIDS findings as correctives to historically donor-driven research (NAC 2005).

analysis of data on sexual behavior, social networks, risk factors, socioeconomic status and local knowledge of the epidemic; across sub-Saharan Africa, international social science research projects maintain an entrenched, if episodic, local presence.

In addition to enlisting Malawian academic researchers into their networks, international survey research projects afford new economic and social mobility to a cohort of young Malawian secondary school leavers and college graduates who find temporary, contractual employment in the world of AIDS research. These individuals are hired as interviewers or data entry clerks by research projects. Many of the college graduates are "contract" workers with the Centre for Social Research or a consulting firm.[18] These organizations hire out not only "experienced fieldworkers" but also vehicles; in this way, much of the pressure of data collection is removed from expatriate staff and shifted to the firm or centre [sic]. Research projects increasingly interface with these organizations to recruit "ready made" teams with extensive field research experience. Other projects rely on word of mouth advice from other expatriate researchers or from Malawian collaborators to "pick and choose" a fieldwork team on their own. Finally, one case study project hired its interviewers locally by holding on-site interviews "in the field." This project recruited employees by posting printed advertisements on trees, walls or at the district offices a short time ahead of its arrival at a field site. On "interview day," hundreds of secondary school graduates from the project's sample areas would turn up.[19] Swidler and Watkins term these secondary school

[18] The manager of one consulting firm said that at any one time his small office was "drowning in [the] CVs" of college graduates looking for contractual "project to project" employment as fieldworkers (field notes; February 28, 2008). Though those who signed on with this firm were not supposed to take research jobs "on the side," this was common practice.

[19] Hiring individuals with only a secondary school certificate is relatively rare. In some cases, after this project finished fieldwork in one region, interviewers who had been part of the team would travel to the region where it would be collecting data next to interview again for a job. Some jobless college graduates were also employed as interviewers and, in a few cases, people who heard the project was interviewing traveled from the city or distant districts to seek a job.

graduates in Malawi "interstitial elites;" in a country where only a small minority achieve this status,[20] they aspire to a bright future. However, because they are not sufficiently educated to be competitive for NGO jobs in the cities, these young people often also find "work" as volunteers in donor implemented programs in the rural areas (2009:7). Research jobs provide a temporary break from farming and petty trading for many of these interstitial elites. Many interviewers on case study projects suggested that after the project left town, they would return home to do farming and "wait for jobs from projects;" all of them also articulated ambitions to go back to school for degrees in fields such as computing or accounting if they saved enough money in the future.

Average age of local experts	**Average number of research projects worked (in lifetime)**	**Percentage secondary school leavers**	**Percentage secondary school + certificate (e.g. VCT, secretarial, accounting)**	**Percentage college graduates**
25.41	3.97	29	61	10

Figure 1.1: Characteristics of fieldworkers. Numbers in this table reflect a survey conducted with n=117 local experts in 2007-08 (Complete responses: 98/117=84%). The respondents comprised HIV counselors, interviewers, supervisors and data entry clerks.

In Malawi and other developing world contexts, the AIDS crisis has permitted free(-er) circulation of knowledge, people and capital across transnational borders[21] and normalized a sort of deregulated or "casualized" labor market that enlists drivers, interviewers, supervisors, scouts, data entry teams, voluntary counseling and testing

[20] Only 2.2 percent of 15-24 year olds successfully passed their exams at the end of secondary school (IFPRI 2002).
[21] The Introduction provides a history of the AIDS epidemic in Malawi.

(VCT) teams and nurses.[22] This casualization results primarily from unpredictable and conditional research and development funding structures. Nonetheless, the advent of AIDS has brought about a wide array of institutional responses that are built on preexisting infrastructures and expertise and redirect energies toward mitigating the epidemic (Cohen and Livingston 2009:39). In contexts of high joblessness, low skill sets and poverty, AIDS has produced new jobs, new modes of social reflection, new kinds of social relations and new categories and performances of expertise.

Within the crucible of the international AIDS research project, local knowledge and expertise are defined, evaluated and valorized in everyday exchanges and interactions. The experts described in this chapter are entrepreneurs of knowledge whose material success often depends on their sustained interaction and familiarity with those to whom they sell their knowledge.[23] Throughout, I focus on the flexibility of fieldworkers as they forge transnational connections that inform future socialities and exchange, simultaneously immerse themselves in many kinds of productive activities and convert diverse kinds of relations and objects into wealth within a social field that appears to be structured against their own interests (Bourdieu 1977:72-87, de Certeau 1984, Bourdieu 1987:97-244). However, this flexibility and adaptability have important repercussions for and influence on the kinds of knowledge produced by international research projects that employ fieldworkers.

[22] Pre-AIDS research projects that focused on fertility, maternal and child health, land management and agriculture also drew on this casualized labor force.

[23] Anthropologist Julia Elyachar draws attention to craftsmen as knowledge entrepreneurs in Cairo; in a city characterized by sustained NGO involvement, local individuals find more value or money in work as "native informants," "putting themselves on display as bearers of authenticity to foreign donors," than in work as producers of commodities (2005:177-179).

A Brief History of the Object of Local Knowledge

The recruitment of local experts to previously top-down research and development projects opens knowledge production to wider and more diverse perspectives, interests and ideas. This shift emerges in concert with a widespread emphasis in international health and development agendas to launch projects that are culturally sensitive, participatory and inclusive of local perspectives and cultural sensibilities. Outsiders have long relied on intermediaries or local experts in Africa; since colonial governments were located in the metropoles of Empire, they enlisted locals into imperial projects in numerous capacities. In colonial Malawi, for example, a policy of indirect rule ascribed much power to village headmen (Vail and White 1991:156-159). However, archival sources such as administrative, research, agricultural or medical reports rarely discuss these intermediaries, rendering their important contributions to such projects invisible. Historian of science Kapil Raj suggests sources that document the production of legal knowledge in eighteenth century Bengal tend to hide indigenous input behind "I-witness" type accounts. However, he also pinpoints some cases where authors explicitly advertise the role of intermediaries, perhaps to discredit the native informants of rival colonizing powers in the region (Raj 2007:104). Similarly, while the imperative of British and other colonial development and research projects had been to solicit culture as data or a resource for development and to "I-witness" from an expert standpoint, contemporary development and research projects have revalued local knowledge as an object laden with the possibility of enhancing efforts and seek to recruit trustworthy and legitimate local experts. This means that local experts have been afforded jobs and other

opportunities while their historical counterparts did similar work for minimal or no renumeration and without recognition.[24]

Archival sources indicate that the orientation of outsiders to Malawi shifted from one of dismissal or disdain toward local knowledge to one of interested inclusiveness. Colonial priorities in the late 1920s and early 1930s centered on maximizing the agricultural production of then-Nyasaland and on reproducing healthy and well-fed migrant laborers to work in imperial mines in nearby South Africa and Southern Rhodesia. In this era, improvement schemes sought to improve native farming practices,[25] to "correct" local conceptions of health and hygiene[26] and to produce positive attitudes toward education; local knowledge was deemed backwards, traditional and stubbornly resistant to change.[27] These dismissive attitudes toward local knowledge were also evident in the famous *Survey of African Peoples* undertaken by Sir Lord Hailey in the mid-1930s.[28] Though the aim of the colonial government's "fact finding mission" was to understand gaps between policies and the "on the ground" situation, there was a glaring lack of interest in local knowledge; the report nowhere mentions "what Africans themselves think or wish…" (as quoted in Cell 1989:504-505). This two year mission that took Hailey from the Union of South Africa to the British territories in east Central and West Africa—he spent just two days in Nyasaland where he consulted district

[24] In a few cases, fieldwork supervisors of my case study projects were invited to be co-authors on academic papers. In almost all cases, supervisors and interviewers were thanked in conference presentations or article acknowledgments. Supervisors for one case study project presented research findings at a national research conference in November 2007.
[25] The British Central Africa Company and the Chief Secretary of Nyasaland corresponded in the 1930s about a scheme to establish native tenants as peasant producers in model villages (BCAC 1939).
[26] In the late 1920s, the colonial office took interest in establishing two hospitals for infectious disease (one in Zomba and one in Blantyre) whose target population was the natives who came to these "urban" centers from rural parts of the country to seek work ("Native hospitals" 1929).
[27] Poor land use was attributed to "apathy, if not actual laziness" and minutes from meetings in district offices in central Malawi in 1938 suggested that natives "must learn to use the land intelligently" (Zomba District Book 1938).
[28] Insights about the Survey gleaned from correspondence about the African Research Survey between the Cambridge School of Social Anthropology, Nyasaland chief secretary, provincial commissioners and directors of medicine education and agriculture (African Survey 1934-1940).

authorities and local leaders on topics ranging from land tenure to methods of education—enlisted the help of intermediaries in planning travel, initiating local introductions and gathering facts; however, the surviving notes and documents elide the specifics of these interactions.

The rising importance of colonial development schemes and projects to the Nyasaland government meant that it was increasingly necessary to recruit trustworthy local experts who could translate the *terra incognita* of both the physical and cultural landscapes. Gradually, colonial research interests came to require greater institutionalization, expanded infrastructure and expatriate presence on the ground in Malawi.[29] With this presence emerged new opportunities for local experts to influence and play a larger role in the production of knowledge about Malawi's social problems. More recently, there has been a shift away from "blueprint" or control-oriented projects and toward collaborative projects that embrace local expertise and that employ local experts.[30]

[29] Beginning in the 1930s, the newly established Tropical Medicine Research Committee and the Standing Medical Research Committee for East Africa sponsored fellowships to recruit young researchers and specialists to work in the tropics and often discussed the effective centralization and coordination of research in the region (CDPHC 1936, SMRC 1936, CBEA 1937, MCCA 1945). Later, the establishment of the Agricultural Research Council, the National Resources Research Committee and, in 1974, the National Research Council indicated a burgeoning post-independence interest in research. The newly minted University of Malawi established an Institute of Social Research, but it ceased to operate due to lack of funds (Ngwira 1986). It was revived in 1977 when UNICEF approached Malawi needing an evaluation team for a project; gradually the institution now called Centre for Social Research (CSR) became a major arm of the university that hosts expatriate social scientists conducting collaborative research in Malawi (Mkwandawire 1986).

[30] The rise of collaborative research paradigms and valorization of local expertise simultaneously masks and depoliticizes a long history of exploitative and extractive knowledge-making practices (Williams 2004).

Figure 1.2: The University of Malawi's Centre for Social Research (CSR) (source: CSR website).

"Elite Experts"

Although local institutionalization of research in Malawi and the global health community's continued interest in mitigating the spread of AIDS in sub-Saharan Africa have normalized collaborative endeavors between expatriate and Malawian researchers and organizations, my informants indicated that neither expatriate nor Malawian partners (elite experts) to these formal collaborations are heavily involved in the everyday social relations, exchanges and processes of research fieldwork;[31] this section shows why. The limiting factors described below give the fieldworkers that are the subject of this chapter a foothold to enter the field of international research as local experts and to play a significant role in everyday knowledge production during fieldwork. The chapter uses the term "elite experts" to refer to expatriate and Malawian co-principal investigators and "local experts" to encompass the secondary school or college graduates who are hired by expatriate-led survey research projects as fieldworkers. The category of "local experts" comprises college educated Malawian supervisors and the interviewers who, in some

[31] Cf. Holland 2006.

cases are college educated, but in others have only finished secondary school. It should be noted that these individuals are also, relatively speaking, "elite;" the vast majority of Malawians do not attend secondary school.

Category	**Education Level**
Expatriate Principal Investigators "Elite Experts"	Post-Graduate Degree
Malawian Co-Principal Investigators "Elite Local Experts"	Post-Graduate Degree
Fieldwork Supervisors "Local Experts"	College Degree
Fieldwork Interviewers "Local Experts"	College Degree or Secondary School Certificate

Figure 1.3: The Structure of a Survey Research Project.

Both the National Health Sciences Research Council (NHSRC) and the College of Medicine Research and Ethics Committee (COMREC) mandate that research proposals submitted by foreign scientists or researchers list a local Malawian co-principal investigator and include a detailed letter of affiliation to a local institution (GRBC 2002). Malawi research guidelines also provide detailed instructions to guide authorship claims, responsibilities in authorship and considerations in determining authorship among collaborators. For example, the Centre for Reproductive Health (CRH) will not recognize "gift authorship," or material whose co-authorship is awarded to a person who will not contribute significantly to the concerned research project (CRH 2009).

This contract for collaboration between (usually two) individuals has a wider sweep; it often bestows benefits or resources upon the institution where the Malawian co-

researcher is based.[32] The acting head of the National Research Council of Malawi (NRCM) explained that the national review boards were increasingly vigilant about ensuring that proposals put in place solid plans for genuine collaboration;[33] one American researcher described how the initial version of her team's proposal was rejected because NHSRC claimed the institutional collaboration posited by the American team with a Malawian university was "not meaningful."[34] She said her research team planned to hire Malawian consultants for some aspects of the project but that NHSRC said they were not permitted to work with a "consulting firm" because it was not a "real, local" institution. Eventually, after the American team secured a contract of collaboration with a Malawian economist and created a memorandum of understanding (MOU) with a Malawian university (which laid out such things as the number of computers that would remain behind in Malawi once the research was finished), the NHSRC approved the project for implementation.[35]

Despite the detailed scripts and guidelines meant to guide and ensure collaboration in Malawi, both parties to collaborative projects agree that, "something must change" about the "unequal" nature of the collaborative relationships. How is it that Malawian researchers are formally recruited as collaborators but may do very little that constitutes collaboration in practice? Most foreign research projects that operate in Malawi follow a certain path that leads to recruitment of a local collaborator. Typically, a project or researcher that has never worked in Malawi will first make contact with a researcher who has experience in the country. This contact familiarizes him or her with

[32] However, benefits do not always flow unidirectionally. An American researcher working in South Africa told me he criticizes his colleagues at his home University of X (a large research university in the US) for "panhandling" at University of Cape Town for sabbatical years (Interview, American demographer; December 15, 2007).
[33] Interview, Acting director of NRC; November 17, 2007.
[34] Interview, American demographer; September 20, 2007.
[35] Ibid.

the ethical review procedures and other bureaucracies one must navigate before setting foot on Malawian soil as a researcher. The first contact between a research project who is looking to recruit a person whose name they can print in the blank space left for "Malawian Co-PI" on the cascade of forms that will be submitted to NHSRC and a potential Malawian collaborator happens through e-mail or phone conversations arranged by other foreign researchers or through a formal "introduction," perhaps at a research conference.[36] Barring any real objections to the plans of the research project as outlined in the already prepared proposal, the Malawian potential co-PI will usually sign on to the study. Acquiring this signature is a platform upon which field research is built; a Malawian collaborator on many projects across southeast Africa said it well:[37]

> 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!

In most cases, the co-PI becomes a collaborator long after the research study itself has been conceived and proposed and sometimes after it has already been funded.[38] After the study proposal passes the national ethical review board, the foreign researcher(s) go to Malawi for some period of time to facilitate the initial set up of fieldwork. During these brief (usually three days-one week) visits, the expatriate researcher meets his/her Malawian collaborator face to face and/or recruits his help in pre-fieldwork tasks such as survey tweaking, translation or choice of research sites. In many of the MOUs or contracts drawn up between foreign and Malawian collaborators, the latter are formally granted payment in return for a few days of "on the ground" observation of or

[36] Research notes, correspondence with Malawian and expatriate principal investigators; 2007-2008. This mode of introduction is how I, too, came to begin my research in Malawi.
[37] Interview, Malawian researcher; December 14, 2007.
[38] It should be noted, however, that many international bodies that fund overseas research are now requiring evidence of local collaboration. This is usually in the form of a letter of affiliation on the institutional letterhead of the collaborating institution.

participation in fieldwork activities. In many cases, Malawian researchers rarely spent time "on the ground," even though they received their payments. The expatriate researchers did not view this as out of the ordinary until they were asked in post-fieldwork interviews to comment on how much their Malawian counterparts had contributed to fieldwork.

Malawian collaborators were overextended, overworked and often out of the country. Most of the collaborators to these social science research projects were academics, typically professors in the Sociology, Demography or Economics departments at the University of Malawi, Chancellor College. Another common site from which Malawian collaborators were recruited was the Centre for Social Research (CSR), an institutional arm of the university with its own budget whose main function is to house rotating faculty from the university who oversee the collection of data for research projects that are in the national development interest (interpreted loosely). For example, in 2007-08, the kinds of projects with which Malawian researchers affiliated with the CSR worked on included: a study of the context of adolescent risk behaviors across numerous African countries, an evaluation of UNICEF funded community based child care centers, an FAO study on rural aging and livelihood in one district of Malawi and an assessment of how Malawian farmers experienced the 2006-2007 input subsidies.

Because of the small size of the country, the small number of people holding MA or PhD level degrees in Malawi[39] and the high density of research networks through which collaborations were forged, it was the case that one professor at the University or

[39] In 2006, 106 on staff faculty at *all six* of the constituent colleges of the University of Malawi held a PhD degree. Notably, for the most recent date prior to 2006 (2001) that such statistics are available, 155 individuals held a PhD. At Chancellor College, the main site from which the social science collaborative projects I studied recruited collaborators, 52 individuals held a PhD in 2006, 101 individuals a Masters, and 47 an "other" degree (EMIS 2006).

research fellow at CSR could be listed as a collaborator to upwards of ten projects at one time (this was true for three of the social scientists at CSR in 2007). Furthermore, Malawian historian P.T. Zeleza's self-description as an "academic nomad in distant lands" applies to many Malawian intellectuals who have either sought greener pastures than the cash strapped University of Malawi or who spend much of their time traveling for consultancies or conferences (McCracken 2002; Zeleza 2002). Malawians and other sub-Saharan Africans who enter into academic careers anticipate this life of academic nomadism and travel. They become skilled at identifying those opportunities (conferences, consultancies, workshops) that will be most worth their time. During a dinner at an international conference sponsored by a Norwegian African Studies Institute, African academics complained that the sponsoring institution had not provided them with "pocket money or *per diems*." A young Zimbabwean academic gave voice to the larger reasons behind this shared complaint:

> We live off *per diems*, you see! 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. Let me tell you, 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 [laughing]. That man makes money, let me tell you.[40]

This man's account of a "character" familiar to many others at the dinner indicates that research worlds are tightly knit and small and hints at the central importance of *per diems* as a supplemental source of income.[41] A Ugandan man shared a story about his wife who received a £200 *per diem* at a training session in Edinburgh,

[40] Field notes; November 29, 2007.
[41] A supervisor for one case study project reported earning an allowance of 17000 *kwacha* (121 USD) per day for attending a conference at a hotel on Lake Malawi; this was on top of the sponsoring organization paying accommodation costs. Field notes; June 28, 2008.

where she and her African colleagues found that rather than paying £10 for a meal inside the hotel, they could eat a similar meal for £3 just a short walk from the hotel.[42] The opportunity to travel outside Malawi and be paid draws many who are meant to be "on the ground" collaborators on international projects to spend their time elsewhere.[43] A Malawian academic working for a UN agency in Malawi described another benefit to attending conferences or workshops: "It is nice to have a respite from people knocking on my office door constantly and some time when I can just read my emails in peace!"[44]

Malawian academics repeatedly discussed the ways in which they were "spread too thin." A senior researcher and faculty member at the University of Malawi described the multiple demands:

> 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 abandon "moonlighting" on consultancies… We cannot compete for research money at a global level…Proposals for the consultancies I've mentioned, on the other hand, are not as comprehensive…If you submit [a proposal] in-country, you hear in two weeks, get the money and life goes on. The research may not be intellectually stimulating but it pays.[45]

"Moonlighting" on consultancies becomes less a "distraction" than a norm and individuals are left with no time to work on or develop their own research interests. These sentiments were echoed by the growing tension between CSR and the larger university; since 2005, the university administration has pressured faculty to "improve"

[42] Field notes; November 28-December 1, 2007.
[43] Others have documented the importance of *per diems* as income supplements in sub-Saharan Africa (Lwanda 2005, Heimer 2007).
[44] Interview; June 17, 2008.
[45] Interview, Malawian social scientist; December 1, 2008. Senior Malawian academics earn a salary of around $500-$600 per month; consultancies pay hundreds of dollars per day. Cf. Holland 2009.

their teaching and to focus more intensely on their courses instead of on research.[46] In Malawi, purely academic research is devalued amid a de-institutionalization of the social sciences.[47] To make ends meet, many academics have become savvy at marketing themselves in other capacities that draw on their skills or status as "elite experts." However, the entrepreneurial success of faculty members involved in consulting may be inversely related to their academic status (Holland 2006, 2009).

The main way that elite experts supplement their income is through working as consultants on government or non-government research contracts. This, too, draws them away from meaningful supervision of or participation in data collection or fieldwork with international projects. The average length of such consultancies ranges from less than one to four months, but the most common involves "thirty man-days." This means that the actual period that a consultant has from the start point of his work to submission of a final product could be much longer than thirty days, but that the organization calling for the consultancy agrees to pay for thirty days of full time work, and no more. Since everyone hired as an in-country consultant has other things to do, this means it takes longer than thirty consecutive days to complete a consultancy. Informants who worked as consultants suggested that many consultancies come about when the end of the fiscal year or some other deadline is drawing near, when organizations need to submit a report to donors and know they will not finish it on their own. Consultants are hired to "bridge the

[46] Research notes, conversations with faculty members in the University of Malawi's Economics, Sociology and Population Studies Departments; late 2008. When I taught as a guest lecturer in the Sociology Department at the University of Malawi in 2008, colleagues often told me they felt they were "selling out" and expressed frustration at not being better mentors to the next generation of scholars. My students were surprised by my "perfect attendance" to my scheduled lectures; often, they said, they turned up in the morning to be greeted by a hand-written sign on the classroom door stating that their lecturer was "away on business" or "out of the country, suddenly" (Field notes; late 2008).

[47] Though faculty members at the University of Malawi are underpaid when compared with western academics, they do lead a comfortable life locally. Their salaries, however, fail them in terms of the costs and commodities associated with professional life and when one considers their obligations to support rural or less wealthy kin (Interview with Malawian sociologist; June 19, 2008). Holland describes how deinstitutionalization of the social sciences partially results from the creation of "Centers" for the study of poverty or education that are aligned with donor interests and draw academic social scientists away from the university (2006:128).

gap" for these organizations. The ease with which Malawian academics acquire consultancies is partially a result of the growing reluctance of contracting organizations to deal with institutions, which often complain about "short notice" or are perceived to be "bogged down in bureaucracy."[48] Often, contracts involve travel abroad, such that a person who garners just a few per year might find him/herself abroad twice a month; one Malawian social scientist said:

> This year has been crazy, my worst one yet. I was never in Malawi! I have worked on, I think, not less than six assignments. Let's see if I can remember: February in Mozambique, April in Cape Town, May in Pretoria, June in Johannesburg, in July I was in London, August… hmm… Norway… September, a trip to Uganda, at the end of October Geneva, and then in November I was in Kampala… all for meeting other collaborators and conferences. Two or three trips had to do with a project I'm on with World Health Organization (WHO).[49]

It was precisely his respectable and reliable "local knowledge" that enabled this professor-consultant-researcher to expand his experience in the wider world via serial trips outside the country.

Malawian partners in survey research projects were not the only actors whose involvement in everyday fieldwork was limited. The participation of expatriate researchers in fieldwork and data collection was limited by a number of factors. The international marketplace of expertise privileges efficiency and the collection of "timely data" and devalues long-term stays in the field for expatriates. Expatriate principal investigators on collaborative projects based in Malawi or other sub-Saharan African countries were not expected to do "fieldwork." A Canadian demographer, for example, highlighted what she termed the "difference between research and scholarship." She suggested that the disciplinary norms of demography do not allow for her to be "in the

[48] Field notes, frequent conversations; 2007-2008.
[49] Interview; December 1, 2008.

field" for long periods of time, despite her desire to spend more time in sub-Saharan Africa and her belief that logging local time would improve her research findings and conclusions: "The point of fieldwork is not understood at all in my field. People [in demography] really view it as a vacation!"[50] An American demographer believed her past decisions to remain in Malawi for longer than a few weeks at a time had significantly hindered her ability to find an academic job in population studies. "When I went for [job] interviews, people tended to ask me, 'What were you doing there for *three months*?' as if that had been a complete waste of time, time I could have been using to write papers, analyze data, write another proposal…"[51] Clearly, the role of expatriate researchers involved in the survey research this study focuses on is not to make inroads into the rural areas; the demands of research, teaching, publishing and stringent disciplinary norms mean that short visits to the field (usually three days to one week) are commonplace. At the time of my research, the adjective "World Banky" had entered local vocabularies to describe foreign researchers or interventionists who spend "a few days in Malawi now and then…" or "parachut[e] in and out of countries."[52]

Malawian collaborators and expatriate researchers come together during the initial implementation stages of the research project. Foreign researchers usually traveled to Malawi for a few days or a week to set up the research project, oversee the hiring of supervisors and interviewers/data collectors, and meet important local people in the research world. This "face time" in the country involved late nights around food and drink designing survey tools or engaging in translation. These interactions served two main purposes. First, they were a way for foreign researchers to hear from Malawians

[50] Interview; December 14, 2007.
[51] Interview; January 19, 2008.
[52] Interview, American demographer; August 23, 2007, field notes, frequent conversations; 2007-2008.

whether or not they thought the survey questions or research plans they had already designed were appropriate. Second, they were a performance of partnership and collaboration that allowed local collaborators a minimal level of participation in a pre-conceived, solidified agenda. The contributions made by Malawians to the survey tools, for example, were almost wholly in the arena of translation. I describe the process of translation and survey design in more depth in Chapter Three.

In short, although expatriate researchers must enlist a Malawian collaborator to any research project they plan to carry out in Malawi, in reality, neither Malawian collaborators nor expatriate researchers are instrumental to the everyday practices of research (namely, data collection, fieldwork logistics and data entry).[53] Instead, young Malawians who are hired by research projects to supervise, manage and conduct the "fieldwork" portion of research step up as local experts, translators or intermediaries who significantly impact the course of and findings derived from research.

Brokering Local Knowledge in the Field

"As a fieldworker, the [HIV] counselor should be able to know that culture has been there for ages and your plan is new to them [the villagers who are participating in research] and it might also take another generation to change the culture…"[54]

This excerpt from a training manual distributed to fieldwork teams by one case study project implicitly solidifies boundaries even as it attempts to make them permeable. First, it places a boundary between the HIV counselors and their subjects, rural Malawians, by confining culture to the villages and associating the power to change

[53] It is important to note that some expatriate researchers are significantly involved in "on the ground" fieldwork. In the case of two case study projects, principal investigators spent weeks in the field at a time and were involved in overseeing everyday operations.
[54] Source: Training manual distributed to the HIV voluntary counseling and testing (VCT) team for a large social science research project; May 2008.

culture with the counselors. Likewise, in its objective to train or teach the counselors, it implies a boundary between the project and its employees. This section, in addition to capturing the interests, everyday practices and aspirations of fieldworkers, illustrates the importance that maintaining boundaries plays in research fieldwork. Solidifying and emphasizing boundaries between themselves and their employers and between themselves and rural research participants enables fieldwork supervisors and interviewers to preserve ownership over local knowledge. It ensures that their skills and expertise remain highly valued in the marketplace of expertise.

The discussion is divided into three parts. Part one, "Boundary Work: Training Local/Experts," shows how fieldwork training sessions and everyday fieldwork discourse produce and solidify a role for the fieldworkers that relies on the maintenance of boundaries between locals and experts. Part two, "Always on the Clock, Clocking Time," shows how the expectations that researchers have of fieldworkers overestimates the authenticity and stability of local knowledge, which is a product of local experts' anticipation of the needs of researchers. Finally, part three, "Accumulating Social Capital," illustrates how local experts capitalize on their employment to forge new social relations, engage in multiple kinds of exchanges and build monetary capital. The influence of the local experts' interest in boundary maintenance, performances of local knowledge and everyday self-interested tactics on the data collected by case study research projects will become clear in the chapter's conclusion.

Boundary Work: Training Local/Experts

Training sessions held by my case study projects prior to the start of fieldwork and data collection helped to produce and reinforce boundaries between research culture and local village culture. These sessions also transformed trainees into field experts and cultivated their own investments in preserving boundaries between themselves and, first, the local culture of the villages and, second, the global culture of the projects.

Case study projects held extensive training sessions for their fieldworkers during the first week or two of a fieldwork season. These trainings were held in rented facilities (such as a teacher's college or a hotel conference room) or at the guesthouse where fieldwork teams stayed for the duration of data collection. The purpose of training sessions was to encourage bonding and cohesiveness among the field teams, to determine before fieldwork began which fieldworkers should be "let go," to familiarize fieldworkers with the survey or other instruments to be implemented and to standardize and harmonize data collection procedures as much as possible. These trainings also implicitly drew a boundary between local people and local experts.[55] Specifically, the project and its new employees co-constructed a sort of ideal type villager or research subject to facilitate their work in the field. First, engagement with this ideal villager necessitated some preparations and forethought as to proper comportment, behavior and dress code on the part of the fieldwork teams. On day two of a joint training session for interviewers and HIV-counselors, a supervisor provided some guidelines:

> "How do we dress for the field? We put on a *chitenje* [cloth wrap worn by women]. We can't wear what we wear in the city. You have to suit the environment. Strong perfume can make the respondent uncomfortable and manners affect everything... Don't whisper or appear to be gossiping in front of

[55] Because the trainings were primarily led by trusted Malawian supervisors, often who were familiar with the project from past years, the sessions also illustrated the ways in which supervisors drew boundaries between themselves and the interviewers they trained.

> villagers." The supervisor closed this session with a performance of a commonly known piece of "village culture" in Malawi; he clasped his hands together and thanked the trainees for their attention: *Zikomo!* [Thank you!] [56]

The gesture—*Zikomo*—was further explained for the benefit of those who may have been unfamiliar: "Always do this if you pass someone in the village or if you wish to enter someone's compound…" Instructions such as these belied an assumption on the part of the research project's Malawian *supervisors* that fieldworkers must be familiarized with or encultured into "the field." As they are trained to embody these roles, they are taught that they are fundamentally *different*—more urbane, more familiar with international branding, more sophisticated, more open-minded—than the villagers they will be interviewing.[57] However, these instructions also point to the supervisors' interest in maintaining a boundary between themselves and their trainees. Even if these trainees are already familiar with local customs (as was the case for the project that hired interviewers locally), becoming a local expert—a competent fieldworker—necessitates *training* as a mode of professionalization into the world of survey research. Though individuals have probably already encountered rural Malawians and village customs, they must be taught to embody a new role as a local/expert and to see villages as "the field." Instead of initiating them into "local culture," these trainings initiate fieldworkers into "research culture."

[56] Field notes, training session; May 21, 2008.

[57] Though this is mostly accurate, it depended on the project's hiring practices. While some projects hired interviewers who were urban, more cosmopolitan and generally well-educated, one project in particular made it a point to hire fieldworkers from local sample areas to bring some financial benefit to the surrounding communities. There was much discussion as to whether this model was better or worse than one that brings in "strangers" to conduct intimate interviews. Nonetheless, the fieldworkers hired locally tended to be very similar to the people they were interviewing; in some cases, their relatives (or even, in one case, the actual individual) were in the research sample. Across the projects, however, this discourse of difference during the training sessions was consistent.

Figure 1.4: A teacher training college (TTC) building rented by a case study project. A ten-day training was held here in June 2008 (Photo: Author).

The guidelines for dress and comportment were, in general, meticulously observed by fieldworkers and monitored by fieldwork supervisors on a daily basis. In June 2008, I attended the training sessions for interviewers who would be administering a thirty-page survey to villagers in the coming weeks. As they prepared to enter "the field" for the first time to pilot the survey, a supervisor singled out a fashionably dressed male interviewer who was sporting a Kangol brand cap to drive home a lesson: "We can't be putting on hats like this one *ku mudzi* [in the village]!"[58] This reprimand mirrored statements I heard again and again that served to oppose the city and the village: "Blantyre is Blantyre, but Mchinji [a rural fieldwork site]… *ndi ena!*" [The city is one

[58] Field notes, training sessions; June 2008.

thing, but the rural areas are another thing altogether!] While I was participating in fieldwork with another project a few months later, an interviewer was sent home to change his trousers before work. His supervisor asked him: "What were you thinking coming to work with those jeans with 50 CENT [the American hip hop artist] written on them in big letters?"

In their effort to "blend in" with villagers, fieldworkers employed costumes and accessories. Each morning, I would join a mini-bus with about ten other fieldworkers. As the early morning fog lifted and the quiet silence of morning dissipated, we drew closer to the villages where we would be working that day. At the halfway point between the field office and the field, the women in the van tied headscarves or bandanas around their heads and knotted colorful *chitenje* fabric around their waists (usually over trousers or a skirt). At the end of the day, the women sighed with relief, unwrapped their heads and removed the now dusty *chitenje*. This ritual was mirrored by male fieldworkers' tendency to call older or less fashionable sneakers "fieldwork shoes" and to replace them with their regular shoes at the end of the day before heading into town for dinner. The distance between the fieldworkers and the villagers was re-established as the mini-van hurried back to the field office for the day.

In July 2008, these rituals of dress were at the center of a discussion between an American researcher who was in Malawi for two weeks and a local expert, the supervisor for the project's data collection that summer. The researcher wanted to know why women wore headscarves while in the field. The supervisor explained that it was to foster closeness to their respondents by hiding things like expensive extensions or elaborate hairstyles village women do not have access to. "To not wear the scarf would be fostering

social distance, would be saying 'I have a lot of money and I'm not from around here and I care too much about my hair.'"[59] Wearing scarves and *zitenje* simultaneously worked to decrease and to reproduce the social distance between interviewers and research subjects. Villagers could tell if a fieldworker had her hair done in extensions even if she wore a headscarf. However, attempting to "blend in" allowed the interviewer to maintain a foothold in both the local and expert worlds that she straddled. Like chameleons, interviewers gradually became skilled at using cultural diacritics to competently blend into the cultural landscape. Even if they are not "fooling" anyone, dressing and undressing indicates their simultaneous interest in being deemed local and expert—in "knowing" the local.

[59] Field notes; June 24, 2008.

Figure 1.5: A team of fieldworkers and the author in the field office in February 2008. The women wear *zitenje* over their trousers or skirts for fieldwork (Photo: G. Shapira).

Fieldworkers, above all, must be flexible and maintain composure, even when met with traditions, beliefs or practices that surprise them. Training sessions produce expectations and stereotypes about village culture meant to guide the actions and interactions of fieldworkers on the job. Trainings often employed a "cultural sensitivity" approach based in predictions of behaviors or scenarios one is likely to face when interacting with, for example, someone from a different ethnic group or gender than one's own. During a training session for HIV counselors who would be deployed to villages to test and counsel patients, a supervisor said: "In Rumphi, you might find that a man can

have seven wives, in Balaka, there [they also have multiple wives] too."[60] Other assumptions included the idea that men in village households do not cook or carry water and that women do not build houses. The training manuals that accompanied these lessons in cultural sensitivity presented a number of scenarios likely to happen in the field (a place described as "never short of drama, dilemma, laughter or even tears" by the veteran supervisors who authored the manual). The scenarios are followed by formulaic or suggested responses to guide the counselors. Throughout, the manual and the training sessions produced "culture" as an object that is a stumbling block to the progress of research in the field:

> Everyone is molded by culture and jealous[l]y defends his culture and it is not easy to change one's culture just by comparing to some culture practiced by some people somewhere… us as counselors are not supposed to advise but rather just give information, have a small mouth [hold one's tongue] and avoid developing anger [creating bad feelings with] in the people you are working with.[61]

Interviewers at another training session were encouraged "to try not to change whatever they [villagers] might believe… or tell them it is wrong to believe in *afiti* [witches]."[62] By relegating "culture" to the realm of the traditional, old fashioned, rural and backwards, the training sessions produced a distance between the experts who are, ostensibly, naked of culture, and the villagers who are imagined to be mired in culture. Trainings facilitated the imagination of a new national and cultural topography characterized by stagnant pockets of culture and mobile paths of newly minted "local experts" trained to sidestep village culture. The trainings ask interviewers to "black box" culture in order to render it incapable of complicating or slowing down fieldwork.

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[60] Field notes, training sessions; May 2008. Incidentally, this local knowledge is inaccurate, according to the statistics collected by a case study project; the data indicate that there is a single man in the project sample in these districts with seven wives (Email correspondence with project principal investigator; March 19, 2011).
[61] Chimbuto *et al* 2007.
[62] Field notes, training sessions; July 2008.

The lessons imparted during these training sessions were made manifest once fieldwork began in earnest. On an everyday basis, being a local expert entailed (while on the job) de-emphasizing the local moniker and foregrounding the "expert" designation. Lyn Schumaker (2001) observes that fieldworkers associated with the Rhodes Livingstone Institute in its heyday gradually began explicitly to view themselves not as mere research assistants but as researchers themselves; the same was true in Malawi, especially among the supervisors who often called themselves researchers. They genuinely felt they had accumulated the legitimate expertise to identify in this way. Identifying as an expert meant drawing attention to the distance between expert and local worlds. Circulating jokes and what I term "villager stories" play a crucial role in this performance. These took diverse forms, but stories or jokes served to articulate a general theme of backwardness or stubbornness to change: villagers are short sighted when they carry maize to a nearby trading center or *boma* to sell it;[63] or villagers believe in bloodsuckers, for example (see Chapter Two). The conclusion of one of these stories was met with a generalized agreement in the van that "villagers believe the craziest things!" This story telling helped create a narrativized foil to the "experts" charged with researching villagers.

[63] Although Malawian small-scale farmers tend to produce enough maize to feed their household for the year, the need for cash to buy items such as soap, sugar, relish, salt or washing powder often motivates villagers to sell their maize for cash to government or private "middle men" buyers in the *boma*, or local town center. In most cases, this meant that the same household would have to buy back maize later in the season when it ran out, and at a higher price than they sold for. The government recently passed legislation to restrict who can buy maize from villagers and to regulate prices nationwide by pushing private buyers ("middle men") out of the game. Though framed by government as beneficial to poor villagers, many critics interpret this legislation as a sneaky move to sanction ADMARC (the government buyer and storer of maize and fertilizer) as the sole possible buyer of maize.

Figure 1.6: A rural household's recently harvested *chimanga* (maize). The maize will be stored in the *nkhokwe* (silo) to the right to be ground at the nearby maize mill for consumption or sold for cash (Photo: Author).

Thus far, discussion has shown how local experts maintain a boundary between themselves and the rural Malawians who are their interviewees in social science fieldwork. This boundary serves to reinforce differences between locals and "experts" and produce new ones. However, although fieldworkers consistently performed a social, cultural and geographic distance from rural Malawians, their role also necessitated the performance of a certain distance and difference between them and expatriate researchers or project staff. Specifically, fieldworkers staked out a claim on authentic local knowledge that only they posses or have access to. This entailed the maintenance of boundaries between local and global experts in order to sequester and sacralize the

knowledge possessed by the former. This involved identifying oneself in opposition to those who occupy different social positions in what Bourdieu (1977) would call social space. Well into the fieldwork season of one project, the expatriate researchers decided to change the daily fieldwork structure. Along with the expatriate researchers, this project enlisted a staff of numerous American and British graduate and undergraduate students; as the students themselves framed it, they did the "grunt work" for the project.[64] A few students were engaged in small projects of their own, while others were described as "lazy" by researchers; however, many found that they had too much idle time when fieldwork teams were out in the vans. After some deliberation about this matter, researchers assigned the students a new role as "checkers" who would travel to the field a minimum number of times each week of their stay in Malawi. This meant that a student accompanied a team of about ten fieldworkers in a van and helped the supervisors to check the questionnaires for completeness and errors as the interviewers submitted them during the day. This "checking" process, generally accomplished by the supervisors alone served to mitigate time wastage and the need for follow up trips to "fill in the blanks" left by negligent interviewers.[65] If errors or omissions were discovered while a team was still near a household, the interviewer could be sent back the same day to correct them. If they were discovered later, the team would have to make a special trip and lose valuable time and petrol in the process.

When the project directors introduced this new plan to the supervisors over a late dinner one night, the response was quite unexpected: the supervisors were not excited. The fieldwork director explained that the supervisors should not view the addition of the

[64] This included: photocopying surveys, buying soap in town, supervising data entry, coding qualitative data, crunching numbers, organizing data bases or preparing for fieldwork the next day.
[65] While I was in the field with my case study projects, I spent much of my time helping the supervisors check surveys.

azungu checkers to the vans as the project "checking up on" them; they were only there to help. That evening, the supervisors launched into a litany of complaints that anticipated the nuisance student checkers would be. They claimed that the checkers would "slow us down" and "be dead weight." In the course of the next few weeks, their fears were made manifest (in their eyes).[66] Checkers tended to question things that the supervisors were confident should not be questioned on the completed surveys. The supervisors said this was because of the checkers' lack of local knowledge or village norms and life ways. Each time an error or incongruence was flagged on a survey by a checker, the team had to deal with "callbacks" to the household in question. For instance, checkers would often flag questions on the survey where an interviewer had filled in the age of a child in Standard 4 as 14 years or had written 30,000 *kwacha* for the amount a rural household had saved last year. Supervisors explained that one must be Malawian in order to know basic things, to check the figures and information filled into the questionnaires. They suggested that a Malawian would know that it is not unheard of for a 14 year old in a rural area to be enrolled in Standard 4, even though most pupils of that age would be 9 years old. Similarly, they said, although 30,000 *kwacha* ($214) is a large sum for a rural family to save up, some families run maize mills or enjoy bumper tobacco crops. The supervisors grew frustrated when the student checkers sent interviewers back to "needlessly" double check an entry on the survey. They stated specific kinds of local knowledge the survey sought that the American students were unlikely to be able to gauge for accuracy: how much cash crops like tobacco or groundnuts had fetched per kilogram harvested during the prior year, how much money a family saved or loaned in a

[66] Field notes; May 25, 2008.

year or how many times a respondent reported having sex. In the words of a long-time research supervisor:

> That's the problem with having someone check questionnaires, like the *azungu* they are sending as checkers to us… someone from somewhere else doesn't know the area. They are not familiar with what is happening on the ground… You can have the *azungu* working in the field which is proved through simple calculations but if you are trying to study something which is… sort of a local thing, something unknown to them, you have to have people who know what is happening on the ground, so that your data can't be questionable. These guys don't know enough about the context, about Malawi to be able to check a questionnaire and to correct the interviewer's work. These people just here for a few weeks just can't do that kind of work![67]

This characterization of the American and British checkers as lacking local knowledge needed to properly check and preserve the quality of research data articulates a solid boundary between these two categories of "expert," reserving certain tasks, translations and contexts as the sole purview of the Malawian fieldworkers. In these words, we witness a local expert protecting "local knowledge" as possessed only by native Malawians, or by those who have assimilated to the local culture. We might interpret this as an instance of what Steven Epstein (1996) terms "credibility struggles." The kinds of knowledge that are "second nature" to Malawian local experts but alien to expatriate checkers have the potential to enhance data quality. Local/experts are neither "here nor there;" they competently display authentic localness and foreignness as congruent with their shifting contexts and interests.

[67] Interview; July 30, 2008. It should be noted, however, that even as supervisors complained about the *azungu* checkers, they also felt overburdened by the imperative to submit checked surveys at the end of each workday. Many had to "check" over dinner or before bed; their fatigue likely compromised their ability to check accurately and comprehensively. In discussions with the supervisors about *azungu* checkers, their perceptions of my own competence as a checker were higher; they explained that because I spent every day with the teams and checked hundreds of surveys, I had "picked up some basic facts from them."

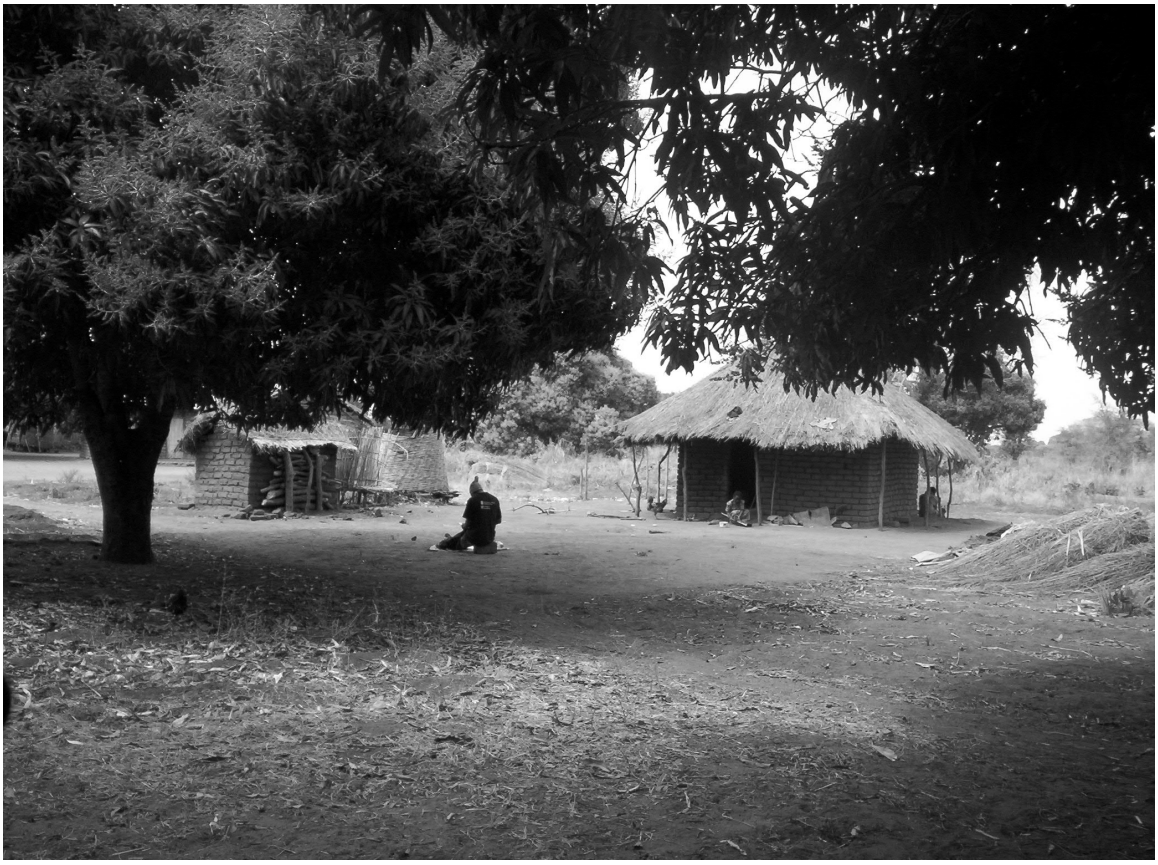

Figure 1.7: An interviewer double-checks his survey before submitting it to his supervisor (Photo: Author).

Always on the Clock, Clocking Time

For fieldworkers, being flexible entails anticipating and meeting the expectations of researchers who seek to enlist local knowledge, logistical advice or perspectives into global protocols for data collection. Local experts are employed by foreign researchers because they possess a store of local knowledge and experience. However, the stability, purity and content of the category of local knowledge cannot be assumed. This section illustrates how researchers' expectations or assumptions of the knowledge possessed by the fieldworkers was often misplaced.

Researchers, especially those who were new to Malawi, recognized the importance of assembling a fieldwork team comprised of professional, trustworthy and

flexible fieldworkers. They, and the fieldworkers themselves, saw a direct correlation between a professional, committed team and high quality data. Increasingly, this means that expatriate researchers draw on knowledge from other peers in their research network who are working in Malawi. A principal investigator for one of my case study projects explained how her original plan upon beginning fieldwork in Malawi had been "naïve." She had had planned to go to the University of Malawi and hire research assistants there. However, in discussions with other researchers, she began to understand how important it would be to have experienced fieldworkers on her team. It was through another American researcher that she was introduced to an experienced fieldwork supervisor—upon meeting him, she was so impressed that she handed over the task of hiring interviewers to him. In this case, the stamp of approval from a fellow expatriate researcher in the same network was enough to convince her to hire him and delegate to him the authority to determine the composition of the field teams.[68]

When recruiting and retaining fieldwork teams, researchers sought out people they could trust. This central focus on trust between fieldworkers and researchers parallels other scholarly framings of the relationship between interpersonal trust and the production of good knowledge; Steven Shapin (1994) shows, for example, how the codes and conventions of gentlemanly conduct in seventeenth century England also determined which people (and by extension, which knowledge claims) were credible, reliable or trustworthy. Trust, however, is not something inherent to an individual or character; rather, it is built over time and within unfolding social relations. Although the researcher above trusted the supervisor she was referred to enough to allocate him significant

[68] Interview, American researcher; May 30, 2008.

(hiring) power in pre-fieldwork planning, he would also have to perform and reaffirm trust for the duration of fieldwork. This trust between researchers and fieldworkers was established within a distinct culture of research as it mapped on to an underlying social field in which the actual asymmetries between these actors was mitigated by cultural norms that continually reinforced and affirmed professionalism, respect and equality. This research culture encompasses norms for social interaction, expectations of sharing (of everything from blankets to food to workload to music files to stories) and guidelines for behavior (How much gossip is appropriate? How should a woman dress?). Inevitably, interactions and impressions that occur or are formed outside of the bounded "workday" or field context inform not only how fieldworkers interact with one another, but also how much or how little expatriates come to trust individual supervisors or interviewers. Trust informed researchers' evaluations of the data collected by a certain supervisor's team of interviewers, how much independence a specific fieldwork team was granted, whether a researcher allowed an interviewer to borrow his computer or whether a graduate student loaned a supervisor 100 *kwacha* for dinner. Because trust must be continually and consistently performed and negotiated, becoming trustworthy—effectively recruiting a new person into one's network—is a full time job. Whether distant from the eyes of their bosses or sitting next to them at dinner, local experts maintained an interest in performing an identity as a good fieldworker.

The cultural norms of research by which trust is built up are rooted in a certain "disinterested interestedness" on the part of both researchers and fieldworkers. This disinterested interestedness upholds the shared misrecognition of the large economic and educational gaps between researchers and, especially, supervisors. These actors infallibly

treated one another with respect and explicitly conceived of themselves as equal partners. Within this context, disagreements or conflicts between supervisors and researchers were relatively rare, even if behind the scenes talk indicated friction. Though supervisors complained about doing a full day of fieldwork on a Saturday, they arrived eagerly and on time on Saturday morning. Even if researchers bemoaned the slow progress in the field, they graciously congratulated the teams for another good day. Both parties were disinterested in conflict that could threaten their mutually beneficial relationship to one another: to oversimplify, researchers wished to collect data as efficiently as possible and supervisors wished to run an operation that was stress free. The two sides to this performance, however, were by no means unaware of what was really going on. Below I discuss ways in which fieldworkers capitalized on their distance from the "eyes and ears" of researchers; however, researchers were not naïve but merely turned a blind eye:

> When you're working with a big project like this one, you can't have all the control. People have told me, you know other researchers, that they think I don't supervise fieldworkers enough. They say "Your supervisor is a free agent!" And, well, it's true. My supervisor is not here every minute, even on days when we are doing data entry. Like yesterday afternoon he was off in the car scouting [scheduling interviews for the next day with local leaders]. And I know when he's out that he's taking care of his own personal business, but the thing is, overall, he is available to us 12 hours a day. He gets his job done.[69]

This researcher makes clear that she knows her supervisor often conducts his own business or errands "on the clock," even though he does not explicitly inform her of this. However, this does not break the trust between them—trust is a give and take. The researcher gives up some time and money in exchange for assurance that the job will get done. Indeed, the supervisor explained that he preferred working for this project over others because "they [this project's researchers] are not constantly looking over my

[69] Interview, American researcher; June 1, 2008.

shoulder."[70] In this way, a mutual disinterest in conflict or confrontation that might have created bad feelings and negatively influenced fieldwork ensured that both parties achieved their interests.

In addition to being trustworthy, fieldworkers were expected to possess and explain local knowledge useful to outside researchers. Lyn Schumaker observes that research assistants to anthropologists at the Rhodes Livingston Institute were "expected to act as a kind of superinformant, being asked for explanations of local behavior, in addition to the meanings of local words" (2001:198). In the words of one long-time research supervisor in Malawi: "Researchers don't just want a tour guide! They want a renaissance man."[71] Local experts marketed themselves by fitting seamlessly into the professional and hectic cultural worlds of researchers and anticipating their expectations. Being a good fieldworker depended on amassing local knowledge and on convincingly exhibiting a simultaneously local and cosmopolitan identity.

In discussions with supervisors about why they thought they made a good supervisor or why the research project hired them over other possible individuals, they consistently mentioned trust and their possession of local knowledge as major factors. I quote one supervisor, speaking at length on this issue, to illustrate the kinds of knowledge that the local experts themselves think researchers are seeking:

> Most of the time it's like when people from outside come here to do their research, the main advice they ask from us is that of the processes they have to pass through for them to do their research in a proper way. So maybe you go to a site, which people should we meet first so that our job should go smoothly? So we tell them "these are the authorities we have to meet first so that things go well." Aside from that, like, cultures in local areas… we have to explain, to say, okay we are in this area and this is what we are expected to do in this area and we should behave like this… for example, the Yaos mostly don't drink because they are

[70] Field notes; June 4, 2008.
[71] Field notes; May 7, 2008 and July 3, 2008.

> Muslim and on Fridays they go to mosque so we tell the researchers to do interviews in non-Yao areas on Fridays so we don't disturb them in mosque… we may even have to tell these kinds of things to interviewers, as well. Like one time an interviewer offended a Yao man who had been cooking us lunch by bringing in one of his mice for lunch. The Chewas do prefer [enjoy eating] to eat mice, but the Yaos… it's taboo for them, you know?[72]

First, this supervisor's comments on local knowledge show that fieldworkers have become familiar with the expectations, demands and needs of foreign researchers. With sequential interactions and interfaces with larger numbers of projects and researchers, they possess an increasingly convincing and commoditized form of local knowledge, scripted to match the anticipations of expatriates. Second, the examples of local knowledge stated here either deal with logistics or with cultural stereotypes. They exemplify the unstable, shifting and constructed nature of local knowledge as it fits into the marketplace of expertise.

Though international projects may take for granted their need for local knowledge, the content and meanings of the category itself often go unremarked. In many projects and contexts, expatriate researchers solicited "local" or "cultural" knowledge from their Malawian supervisors or interviewers. They asked, for example, about the specific differences between types of traditional healers in the rural areas,[73] about the details of initiation ceremonies,[74] about the availability of ARVs at local hospitals, about local perceptions of female condoms or about widow inheritance.[75] Researchers often

[72] Interview; September 22, 2008.
[73] Field notes; July 9, 2008.
[74] Ibid.
[75] Field notes and interview; August 14, 2008.

assumed the responses given by experts to be experiential, authentically local or "from the horse's mouth."[76]

Researchers generally overestimated the amount of logistical local knowledge possessed by their employees. For the "culture chameleons," it was useful to appear familiar with the research area in question, even if it was *terra incognita*. "Now, you know your way around this area, correct?" the researchers asked in the morning. Supervisors would never admit non-familiarity. Once in the vans for the day, distant from the eyes of the researchers, the team's peripatetic meanderings betrayed their non-knowledge of the local context. The fieldworkers, however, maintained flexibility and nonchalance, cobbling together directions from young children or women carrying water on their heads (often giving them rides in exchange for directions to a chief's house, for example) and/or asking "door to door" to learn the location of a certain village, household or headman. Many times, teams were lost amid dense grasses or stuck on the wrong side of a bridge felled by mudslides in the rainy season. However, so long as logistical knowledge ("Yes, I know this area a bit…") enabled the progress of the team that day ("Your team was a bit slow today, but…"), the fieldworkers maintained their credibility.

In the cases of both cultural and logistical sorts of local information, it was perhaps ironic that the fieldworkers attributed "beneath the surface" knowledge to their past work on research projects. In a conversation about whether young girls in rural areas fall in with "sugar daddies" who give them money or gifts in exchange for sex, for example, a supervisor prefaced his response with, "When I was with the adolescent

[76] Field notes; July 23, 2008. Here, it is important to note that while some expatriate researchers relied heavily and uncritically on local experts, others did not. One researcher's impression of "local knowledge" could differ drastically from another researcher's. I found that researchers who were more skeptical supervised their field staff more intensively.

intervention pilot study, we found that…"[77] The range of research studies these culture chameleons have participated in, then, enjoy a new citational life distant from the world of JSTOR, PUBMED or other academic journal storehouses. In other instances, a supervisor would thoughtfully consider the countless interactions (scripted and not) he had engaged in with rural villagers to respond to questions about AIDS-related issues. Local knowledge emerged from mobility through and exposure to the national landscape *through* research project employment.

During 2007-2008 I distributed an open-ended survey to over one hundred interviewers and supervisors working in many international AIDS research projects. One question asked respondents what they most enjoyed about fieldwork. The written responses complement many interviews I had in Malawi where project staff imagined the field as a different, almost magical place that was unfamiliar and new. I coded the responses to this question and found that they formed two clusters: traveling and learning more about Malawi (77/98, or 79 percent of respondents to the survey mentioned these as the main benefits of fieldwork jobs).[78]

[77] Field notes; June 5, 2008.
[78] One case study project hired local secondary school graduates as interviewers. Though these fieldworkers were working in contexts familiar to them, they provided similar responses to the survey questions (and in discussions) about "fieldwork."

Figure 1.8: A supervisor (right) pounds maize (*adadzodzoda chimanga*) with the daughters of a respondent who is being interviewed out of sight (Photo: Author).

Traveling is a major benefit to the young people hired to work for research projects. Most viewed fieldwork as an adventure or a chance to get out of familiar settings and explore new ones. Survey respondents described fieldwork as "a chance to discover the world" and liked that it provided opportunities to make business or other

connections, to see family in other parts of Malawi or to try new foods (eating fish near Mangochi was especially appreciated). While teaching at the university from September-December, I socialized often with research supervisors, many of whom were tired of the "down time" between projects since most international fieldwork happens in the American summers. People said they "longed to be on the move again." Some projects took all or some of the fieldworkers on short leisure trips (usually subsidized) to places like Luanda National Park in neighboring Zambia, to wildlife reserves near research sites or on other special outings. The employees enjoyed these aspects of the job and often considered them when deciding to work for one project versus another. Finally, fieldworkers suggested that, due to the small number of people in the research world and to the long hours spent with one's fieldwork team, they had a fieldwork family: "It's a nice time to hang out in resthouses with my friends- these guys are my family!"[79] Fieldwork and the field offer the same opportunities for adventure, novelty and leisure to Malawian staff that it does to expatriate graduate students who look forward to summer research in Africa.

Fieldworkers also appreciated the chance to get to know Malawi during fieldwork. They liked learning what rural Malawians do, being exposed to the cultural beliefs of rural people, learning about Chewa culture, playing *bao* (a traditional game of skill) with young men in trading centers and listening to elders' stories in the villages.[80] Fieldworkers generally enjoyed interacting with people of different backgrounds, cultures and beliefs, appreciating, for example, the chance to understand "the real life of the people and their culture and to see what it means to be Malawian" or to see "remote"

[79] Response to open-ended question about the benefits of fieldwork.
[80] A selection of the responses coded under "benefits of fieldwork," field notes; 2008.

parts of Malawi.[81] For the local experts, then, as for the foreign researchers, the households they visited and the villagers they met stood in for an imagined "real Malawi" different from what they were used to. Interviewers who were working in their own districts or villages (in the case of one case study project) likely emphasized this "difference" in order to lend credibility to their new role as "expert" interviewers and "researchers." This role and associated symbols (project T-shirt, clipboard, membership in a "professional community," canvas bag for holding soap and surveys) conferred these locals significant status among their peers, who were often in the research sample. Through their initiation into "research culture," these individuals, too, learned to see research participants as "different."

[81] Ibid.

Figure 1.9: Market day at Mkanda Trading Center. Mkanda (Mchinji District) is a research sample site for one case study project (Photo: Author).

Local experts cultivate an ability to display the very kinds of expertise and competence that researchers are willing to fund as they clock time working with research projects. Researchers recognize the value of continuity and cultivated expertise; an American research supervisor told his audience at a training session, "The more time you spend with us, the more valuable you are to us."[82] He asked that fieldworkers sign a contract in which they promised to stay with the project for the "full time." Later, this same individual explained it had been difficult to find interviewers because "there are so many other projects going on right now." He said the project was competing with the

[82] Field notes; May 15, 2008.

national census, which paid much better than his research project for similar work. The value of "sticking with" a project for the duration of fieldwork and over the course of many years is weighed pragmatically by fieldworkers; each project is a platform for expanding social connections and increasing the probability of future financial gain. One man who had begun working as a driver for one project in 2005 described how this initial "contact" allowed him to find regular business with projects and with people who were referred to him through this channel and to invest in "more and more" vehicles.[83] Over time, he asked for higher fees than those he initially received—in 2009, drivers working for projects expected to be given an allowance on top of their explicit salary. A supervisor explained why he had "deserted" a project that originally hired him many years earlier to work for another one: "They didn't bid high enough for me!"[84] Another informant elaborated on the dynamics of the marketplace of expertise: "Research is getting much more expensive… even I am getting more expensive myself. Now I can negotiate, say things like, "They [another project] are giving me this and that…"[85] Working for the same employers year after year also allowed supervisors more room to negotiate for raises and better living conditions. Clocking more time in the research world and learning the ins and outs of the marketplace of expertise enables local experts to more effectively broker local knowledge to possible employers, to increase their negotiating power, to access resources and to earn more trust from their international counterparts.

[83] Field notes; November 19, 2007.
[84] Field notes; June 26, 2008.
[85] Field notes; August 7, 2008.

Accumulating Social Capital

The previous two sections have shown how fieldworkers maintain boundaries between local and expert worlds to maintain ownership over "local knowledge" and how the expectations of researchers regarding the content of local knowledge possessed by these experts may misalign with reality. I now move from the recent discussion of how work with a research project may serve to *produce* or *refine* local knowledge to a focus on how research work can serve as a strategy for accumulating diverse kinds of capital that can translate into upward mobility for the young fieldworkers. Although employees frequently voiced complaints about meager salaries they were paid and their grueling work schedules, they were better off than most people in their peer group simply because they had a temporary, but guaranteed salary. It is essential to consider the non-financial benefits of joining a research project's network. Even if financial renumeration for work on research projects was low, the research project offered diversified social connections and social capital, defined by Bourdieu as "an aggregate of the actual or potential resources which are linked to possession of a durable network of more or less institutionalized relationships of mutual acquaintance or recognition" (Bourdieu 1986:248). International research projects are crossroads of social and informational capital that can be converted into economic capital. Transfers and exchanges of this sort occurred every day during fieldwork.

Fieldworkers accumulated many kinds of objects during fieldwork. First, objects regularly changed hands between American and Malawian project staff. At first glance, the transfer of "second hand" objects from foreign project staff to local staff at the close of the typically short fieldwork periods might seem insignificant. However, such objects

were often reinvented or revalued as they passed hands not only from the staff member to his local counterpart but from the counterpart to his family or friends in the future. Clothing or running shoes were sometimes kept for personal use, but also served as highly valued gifts to kin living in rural areas who may be entitled to monetary or in-kind gifts. As rural and urban dwellers in Malawi often outfitted themselves in *zovala* (second hand clothes for sale at local and city markets), the *zovala* given to project staff members was usually of better quality and newer than the *zovala* available at weekly markets (Figure 1.10).[86] In some cases, objects transferred were much more highly valued. Very frequently, friends to American staff would find themselves with a mint condition cell phone at the conclusion of fieldwork, an item that could be used personally or sold for a large sum. American staff members were compelled to give things away at the close of fieldwork and frequently referenced the poverty and difficulty of finding electronics in Malawi as motivations for, in some cases, bestowing a research colleague with an iPod, digital camera or USB key (flash drive). Such gifts were likely to be kept and not sold, due to the high status they would give to their owner in a context where access to western popular culture is coveted.

[86] Though urban Malawians with disposable income increasingly buy fashionable (usually "knock off") brand name clothing in the cities, during fieldwork they shopped frequently at weekly markets. For the duration of data collection, fieldworkers often competed to see who could find the nicest clothing in the piles. Middle class Malawians living in Blantyre and Lilongwe, too, seek out clothing (especially sneakers) at second hand clothing markets in and around the cities.

Figure 1.10: Second hand clothing (*zovala)* sellers at a weekly market in Mkanda trading center. Fieldworkers (right center) often took advantage of lunch breaks or down time in the field to shop in 2008 (Photo: Author).

Though the material utility of these objects is apparent, it should also be noted that they often played a key role in the ability of individuals to market themselves to future researchers seeking local experts. Namely, researchers preferred to hire research staff members who are "well versed in English and understand what we as [foreigners] are looking for."[87] Often, the expatriates who are charged with the task of hiring the

[87] Fluency in English was a bottom line requirement for employment by research projects (As mentioned by researchers for biomedical and social scientific projects in Malawi in interviews).

"local experts" who will collect fieldwork data are relatively young (either graduate students or recent PhDs) and, therefore, likely to find common ground with a young Malawian. As often as American research team members shared their music with Malawian counterparts, they also exhibited a hunger for Malawian or Zambian music they could share with friends back home. USB keys became a future oriented object for their owners. The owners of these "keys" could use them to store resumes or cover letters to potential employers and access these documents quickly at Internet cafes. USB keys often enjoyed wide circulation among groups of close friends; upon inserting one into your computer you were likely to, first, contract a virus, and, second, to observe files named for multiple people. In more than a few cases, project staff members would give or sell at an affordable price laptop computers to Malawian staff members. Obviously, this object's potential for enhancing future career and social prospects is significant. It should be noted that familiarity with and a clear ability to use technology significantly enhances one's chances of being hired at a higher level on a research project. Working as a supervisor or interviewer, for example, often requires an ability to work with digital recorders (to record interviews with research subjects), iPods (used by some projects as transcription devices), cameras (to photograph research subjects) and laptops (if one is on the data entry team or a typist of interviews).

However, local experts did not only accumulate material objects. Social capital was as, if not more important, than highly valued technological gifts. First, the friendships that form between foreign and Malawian research staff became a resource to be tapped into later, when the former returns to Malawi for another round of fieldwork or to start up another project. Expatriate research staff members told me that before

returning to Malawi for "another fieldwork season," they would e-mail or SMS friends in Malawi to inquire as to whether there was anything they needed. Most suggested that being a courier for gifts was "the least they could do" since their friends in Malawi had very little access to the goods of consumer society. Furthermore, it was often the case that project staff would furnish "loans"[88] or monetary gifts (via one of the many Western Union outlets in Malawi) to help their Malawian colleagues "[go] on in school" or "[start] a business;" loans were disbursed in person or with the help of email or Skype after expatriate project staff members returned to the States. Thus, an open line of communication to a friend across the ocean became another node of support in already existing networks of kin and acquaintances. One supervisor who worked on numerous research projects told me, "many of us tend to each have our own *azungu*,"[89] a person from abroad who was most intimate with him or her. Especially in cases of emergency or tragedy, such nodes could be easily activated.

Another common way in which this kind of social capital was often converted into financial capital was through recommendations for employees passed from people who had spent time in Malawi and people who were anticipating arrival in Malawi; a long-time supervisor explained, "These researchers employ people they know, who they have worked with…. They know someone they are familiar with already will do a good job."[90] In more tragic cases, too, the friendship networks born of the crucible of the research project were immensely important to Malawians. In mid 2009, members of a social science research project received news that a Malawian supervisor had passed

[88] I place the word in quotations to index its differential interpretation by Malawians and expatriates. In all cases known to me (including some personal ones), loans were not repaid. Though expatriates who had provided the loans often viewed this non-repayment as an affront, loans between Malawians, too, were not *directly* or *equivalently* repaid. The practice of "loaning" must be interpreted in its own local and moral context. In Malawi, it is more accurate to interpret loans as gifts in the Maussian sense.
[89] Field notes; August 12, 2008.
[90] Interview; December 2, 2008.

away; news from another project reported that an elderly woman who had worked as a cook for the project had been raped during a forcible break in to the project's housing compound. In both cases, these digital connections mobilized massive financial and other resources from Americans affiliated with the project directly to the family of the deceased and the affected individual, respectively. In this way, transnational social networks were efficiently activated.

Working in the field, distant from the eyes and ears of expatriate research staff sometimes permits local experts to accumulate resources through "siphoning" them from the project. Various forms of siphoning such as conducting personal business on project time (as described above) remained "hidden" and did not necessarily threaten researchers' authority or project protocols; were the fieldworkers to make explicit these actions, however, they would lose credibility and trust. In some cases, research project supervisors used their own cars for some work-related tasks, necessitating reimbursement for fuel used on "company time." However, local experts could often take advantage of the non-knowledge of their bosses of, for example, the price of fuel to fill their gas tank for the next week. Another benefit commonly siphoned from projects was mobile phone airtime. Projects provided airtime cards to fieldwork supervisors so that they could check in with their interviewers about their progress or locate them if they were lost. In the field, supervisors almost never phoned interviewers (air time depletes very quickly if one uses it to make a phone call); if absolutely necessary, they would send an SMS, which cost significantly less *kwacha*. Supervisors used their "siphoned" airtime for personal calls to friends or family and viewed these "*maunits*" [airtime units] as a perk of the job. If supervisors knew that the boss providing them with the airtime had little knowledge of

how long units last, they might try to negotiate for more by claiming they had depleted their units making phone calls in the field that day. In some cases, project staff who stayed in the office failed to realize that many of the rural fieldwork sites lacked reliable cell phone coverage, making both phoning and SMSing difficult.

John,[91] an experienced supervisor, managed to draw on and activate social capital with great acumen. When we first met in 2005 he was working as an interviewer; in 2008, he was the head supervisor of a case study project.[92] Since 2005, he married, had a child, started a minibus business and traveled widely. He dressed well, often wearing a tie to work on days when we stayed in the field office. In the years since 2005, he visited numerous international cities, often staying with researchers or graduate students affiliated with the research projects he had worked for. In addition to his role as a head supervisor, John also runs a business in a suburb of Blantyre, Malawi's commercial capital. John is exemplary (though not representative by any means) of the kind of social mobility this chapter is interested in depicting. With each serial job for research projects, he gained increments of credibility, status, expertise and authority that subsequently permitted him to expect more money, resources, trips and benefits. Even his personal laptop computer and mobile phone were acquired through his work with research projects. At times between 2005-2008, John capitalized on the distance between himself and his employers to take on work from more than one research project simultaneously,[93] an impressive feat made easier because one employer attempted to oversee John's work from abroad via Skype.[94]

[91] Pseudonym.
[92] Interview; May 7, 2008. John is now working on a Master's degree abroad.
[93] Field notes; July 28, 2008.
[94] Field notes; August 2008.

Many projects have begun to put in place contracts that state that an employee may only work for a single project at a time. In June 2008, the recruitment and training for a case study project happened to overlap in time and physical space with the recruitment and training for National Statistics Office (NSO) census enumerators. NSO posted a list of local people who had won positions as enumerators on the bulletin board at the front of the building where the project was holding its training sessions. A supervisor noticed the name of one of the project interviewers on this list; although this interviewer had already been selected as an enumerator for the Census a week earlier, he attended two days of training for the research project. This "eating from both sides" was deemed underhanded and the interviewer was not paid for the trainings he attended.[95]

Although some Malawians working for research projects were duplicitous with their employers, it makes sense to view all such strategies to maximize social position and financial gain in terms of "flexibility." Again and again, research supervisors told me that being flexible is essential in this kind of work. The descriptor flexible was a fitting one, for many reasons, not least of which involved the efforts of these individuals to diversify their social and financial capital networks. Their strategies were diverse, but, in almost all cases, work on a research project became a platform for forging profitable relations and practices. One 29-year-old male who worked as a research supervisor for ten years explained that he grows tobacco alongside his research work by reinvesting the money he earns doing the latter "to do farming." From these earnings, he employs six men who monitor and harvest the tobacco each year. Last year, he supplemented his income by selling 4000 kilograms of tobacco. This supplementary livelihood strategy is

[95] Field notes; June 5, 2008.

an example of his flexibility; he can go to his home in northern Malawi three times a years to "check on the tobacco" and still earn money as a research supervisor. For some individuals, then, knowledge work has become a contemporary form of migrant labor that enhances rural accumulation in a village home.

Living "Project to Project"

Working as an interviewer or supervisor for international research projects requires many things, but, most importantly, it requires the flexibility or ability to respond opportunistically and rapidly to changing conditions. Although the increased presence of international research projects in Malawi provides a wide array of employment possibilities, it would be remiss to suggest that the flexibility is entirely liberating or forward looking. Interviewers and supervisors are perpetually poised to learn of better opportunities, higher pay and rumors of new projects coming to work in Malawi. Research world "gossip networks" were efficacious in spreading invaluable information: who is working for which project, how much a project is paying[96] and the paths and trajectories of in-country *azungu*.[97] However, it is important to note that even opportunities to move upward within the research world were tempered by close analysis of the social and economic benefits; John was invited by a group of Americans to be one of the Malawian trustees of a new organization but declined this offer when he discovered that a national law prohibits trustees of such organizations from working for

[96]This is an especially important piece of information for applicants because salaries for supervisors, for example, can range from 2000 *kwacha* ($14) per day to 10000 *kwacha* ($71) per day with an average of about 4500 *kwacha* ($31) per day.

[97]It should be noted that gossip was a major avenue for finding out information and staying informed across all levels and categories of person in the research world. People often framed the periodic meetings of the Malawian national ethical review board meetings as "gossip sessions," where information about which foreign researchers were collaborating with which local institutions, or how much a project was paying its Malawian staff was exchanged.

the same organization.[98] Furthermore, fieldworkers rely on a larger structure they have little knowledge of or access to. For example, in late 2007, one large research project received word that their proposal had not passed ethical review and, therefore, could not be immediately implemented. However, anticipating approval, the project had already begun training its staff, including nurses who would act as HIV counselors for the project. When the researchers received the news, they had to pass it on to a cadre of well-qualified nurses who had ostensibly anticipated months of steady employment, but were left suddenly unemployed. Similarly, fieldworkers who were part of "ready made" field teams contracted out to research projects often complained that their salaries were not paid on time by the consulting firm or centre [sic] they worked for: "They will just call us and say, 'You'll get the money in two weeks.' And, well, we have no choice but to wait for it."[99]

Because most of the interviewers and supervisors were typically in their twenties or early thirties, they harbored career aspirations; males and females alike complained about the "instability" of this kind of research work, where they were forced to live "project to project." They described how they became "stuck" in the work of research; "This kind of work just doesn't propel me forward at all. I've just been getting some money but I am starting to think I need to make a next step. I am just, like, stuck."[100] Peter,[101] a long time supervisor on research projects, and his wife, a data entry clerk, wanted to study for an MBA and Master's in development studies, respectively, "so that we can stop this working constantly for other people and just have our own

[98]Field notes; December 14, 2007 and July 28, 2008.
[99] Field notes; July 8, 2008.
[100]Field notes; November 1, 2008 and commonly heard.
[101] Pseudonym.

organization."[102] Especially in the case of supervisors, people tended to internalize feelings of great failure if they "were just staying, sitting idly" while "others are working."[103] Many fieldwork supervisors were graduates of the liberal arts college of the University of Malawi and were greatly shamed if they failed to secure employment for even a short amount of time. Nonetheless, research jobs were scarce which meant college educated young people stayed for some portion of the year in the village (or, most likely, the town) they were from. Whereas expatriate project staff members assumed that fieldworkers were happy to go home at the end of a long fieldwork contract, they dreaded returning home where they would no longer be earning money. A supervisor said: "You know, in the old days it was very easy for anyone who went to college to find a job because graduates were so scarce and there were lots of new companies coming in. But now there are just so many of us and jobs want five years of experience and, well, if I don't know someone, I won't get a job anyway."[104]

"Living project to project" simultaneously provides opportunities for and blocks to social mobility. A person's position in the social field of a research project correlates with his/her chances of achieving financial or career success. Though public discourse on the part of the project members celebrated the equality of all team participants, status distinctions and hierarchies within a project were often preserved and maintained through

[102] Field notes; November 1, 2008. Peter tried to diversify his income by investing money he had made working on research projects in a minibus. He was thrilled at this prospect and his business plan exhibited much foresight in its desire to market the minibus to all the projects he works with (projects tended to pay about 8000 *kwacha* ($57-65 at the time) per day to rent a minibus and driver to conduct fieldwork). However, his plan came to a tragic end when he "went in" with a colleague who promised to buy the bus while in South Africa for a business trip. Peter fronted as much of the price of the minibus as he could afford and waited eagerly for his bus to arrive. When it did, his friend handed him the sum he had fronted and proclaimed that he had decided to "go it alone." Peter accepted the news ambivalently: "I'm sad but he just had more capital than me. He has worked longer than I have in research and he had the financial means to double cross me."
[103] Fieldnotes; November 16, 2008.
[104] Field notes; July-August, 2008.

talk and practices. A supervisor on one project described how interviewers (who had finished secondary school) saw their superiors:

> [They] tend to think we think we are too good for them. You know we went to college and had this shared experience and they didn't. And also, you can see on the project how this pans out; while we [supervisors] get the nicer chalets [at the resthouse where fieldwork was based] as accommodation, they complain about how they are there in the public, crappier rooms.[105]

Similarly, when supervisors went for drinks in the evenings, they would often restrict invitations to other supervisors or expatriate staff and frame the exclusion of interviewers as "professional;" [i.e. "we cannot drink with those who work for us"] in the case of the two projects that also hired interviewers who had finished college, the interviewers and supervisors socialized much more freely. Because they had less contact with those who had hiring and firing power on research projects, interviewers were least likely to move up in the project. Thus, although being flexible often led to upward mobility or increased capital for fieldworkers, interviewers and supervisors lead a precarious existence characterized by differential levels of ambivalent stagnancy based on their role on the project and specific social connections and intimacies.

The Implications of Flexible Expertise for Knowledge Production

The "marketplace of expertise" comprises diverse spheres of interaction and exchange that emerge around and within the international AIDS research project in Malawi. The marketplace is not rooted in physical or geographic space; rather, its contours stretch from the US to Malawi to South Africa to India and into cyberspace.

[105]Interview; July 31, 2008. Supervisors frequently expressed "pity" among themselves for interviewers (who were now jobless) at the end of a fieldwork period (field notes; September 22, 2008). They tended to reach out to especially impressive interviewers by phoning them to let them know about potential jobs. Further, supervisors said the most difficult thing about their job was firing people and, in some cases, they asked expatriate staff members to do the "firing" for them.

Encompassing objects as diverse as USB keys, soap and clipboards and actors as diverse as traditional chiefs and renowned academic researchers, the logics and practices of this marketplace are oriented toward the need to collect "timely data" about the AIDS epidemic in Malawi. The labor done by local experts was eclectic—answering the questions of expatriate researchers and villagers, making proper introductions to traditional authorities, appeasing frustrated villagers left out of a study, checking questionnaires for accuracy, translating concepts, recording a household roster, or waiting for respondents drunk on *kachasu* (local brew) to sober up so they can answer survey questions—all these tasks and more made up the labor of the "culture chameleons." Fieldworkers, however, have a certain level of flexibility precisely because the "product" of their labor is not a fully alienable commodity but, rather, a shape shifting and manipulable one: local knowledge.

As they navigated this marketplace of expertise, the local experts (like other kinds of intermediaries) were "instrumental in defining, objectifying and maintaining boundaries between cultures [and] in influencing the power dynamics at play" (Schaffer *et al* 2009:xv). The tactics of these local experts also influenced data collection procedures in the field. Since this data is used to make statistical and other authoritative knowledge claims about the AIDS epidemic in Malawi, it is important to understand the people, exchanges, assumptions and interests that comprise everyday research worlds. The following section briefly describes three ways that fieldworkers' flexible tactics may influence knowledge produced about the AIDS epidemic in Malawi: the collection of "cooked" data, the black boxing of culture, and the (re-) validation of surface knowledge.

Collection of "Cooked Data"

During my time in Malawi, researchers and fieldworkers were aware of the meaning of "cooked [bad] data." Supervisors and interviewers alternated the Chichewa phrase *kuphika madata* with its English equivalent (to cook data) to describe the practice of making up, or cooking, data to save time or avoid asking questions of a respondent. Fieldworkers often teased one another about cooking data. Speedy interviewers who always finished their interviews before their fellow team members were half-jokingly accused of "cooking data;" their colleagues claimed that it was impossible for someone to be done with his interviews so quickly. If an interviewer was observed off on his own, rifling through his finished survey, those around him would speculate that he was "cooking in the kitchen."[106] Cooking data was a jocular way for the interviewers to critique the time constraints imposed on them from above.

Though the practice of "cooking data" was, in my experience, more a mythology than a reality, researchers explicitly warned fieldworkers that cooking data would mean termination of employment with the project. The trainings described in the "Local/experts" section above illustrate the research projects' preoccupation with the collection of data that is as pure and clean as possible. Trainings attempted to produce local experts that would harmoniously and mechanistically collect data; local experts were taught everything from how to dress, how long to spend on an interview encounter, how to write legible numerals, how to answer villagers' questions about the project and whether or not to indicate a mark on the survey with a line or a check. They were also discouraged from ever leaving "blank space" on the surveys; "probing" [vernacularized

[106] Fieldnotes 2007-08; "Cooking data" is a widely known expression among research fieldworkers.

as *maprobing* by the fieldworkers] was a valuable skill. However, this emphasis on collecting complete, perfect and accurate data distracts attention away from the "cooking" that happens even in interview encounters closely guided by project scripts and protocols.

Perhaps it is precisely in producing a "good fieldworker" that the research project collects mediocre data. As fieldwork progressed, fieldworkers gradually came to assert less and less interest in the research and ownership over data they collected. Where a completed survey early on had been a product of pride and satisfaction, it later became much more alienable. Where the first week of fieldwork saw interviewers excited and interested to chat with respondents, the last week of fieldwork saw them going through the motions and disinvested in their work. Though fieldwork was always framed as a collaborative and participatory endeavor, it was clear that suggestions (for how to better ask survey questions, about changing the order of the sections on a survey and so on) from supervisors often fell on deaf ears. Though researchers often conceded that such suggestions were good or legitimate, emphasis on "timely data" and standardization weeded out or muted innovation or change. Although "good interpersonal skills" were valued when a project was hiring interviewers or supervisors, the hectic schedule and culture of fieldwork depersonalized fieldworkers by scripting, standardizing, monitoring and predicting the kinds of interactions they would have. Time does not allow for meaningful discussions between villagers and interviewers about political issues of the day, the rains or the local effects of the AIDS epidemic. In cases where a respondent does begin to touch on these issues, he is usually cut off by the interviewer who is worried about time; in other cases, a long winded response is necessarily "miniaturized" into a

number on the survey, its complexity lost forever to the researchers (see Chapter Three). Thus, while research culture values uncooked data, it may in reality produce data that is, might we say, "over-cooked" and flavorless. The spice of social spontaneity is watered down by a recipe-type approach to research which privileges form (piles of completed surveys) over content (the unscripted conversations and exchanges left behind in the field and undocumented).

Blackboxing Culture

Despite their preoccupation with the object of culture, research team practices inevitably served to "black box" culture. For researchers, policy makers and activists alike, culture has been a centerpiece of the global AIDS epidemic, particularly when it comes to Africa. Like many other cases, the presence of a health crisis in developing regions has brought renewed attention to beliefs, practices and rituals suspected of facilitating the spread of AIDS (see Chapter Four). Across sub-Saharan Africa, national AIDS policies and research proposals aim to unlock relationships between cultural practices such as male circumcision, sexual cleansing or initiation rituals and the risk of AIDS transmission. Culture also influences the planning and administration of the kinds of data collection procedures discussed here. Researchers were universally concerned with ensuring that their fieldwork teams treated research participants with respect and kindness; supervisors imparted this concern to interviewers by training them to be culturally respectful and professional. However, as this chapter has shown, the training sessions that local experts attended encouraged them to engage with "culture" in the field only long enough to neutralize its potentially confounding effects on data collection. In

fact, local experts themselves emerged from trainings newly "naked" of culture that must be imagined as confined to the village and as backwards in order for data to be collected in the first place. An orientation toward villagers as fundamentally "different" or "mired in culture" mixed with an emphasis on "timely data" to disallow the very interactions across difference that a more complexly defined culture necessitates. Local experts often critiqued the mismatch between project time and village time they were forced to negotiate. They mentioned that they felt badly taking up so much of the villagers' productive time and that they felt rude blatantly rushing through greetings or other social conventions known to all Malawians.

While the local experts were viewed as translators by both the researchers and the villagers, they often exacerbated, rather than collapsed, social distance. First, in their role between research and rural social worlds, they served to uncritically reproduce a simple, scripted and stereotyped idea of "Malawian culture." As described above, trainings and project discourse produced local experts who necessarily conceptualize themselves as *different* than their respondents. This self-actualization worked as a volatile reactant with their time spent in the villages during fieldwork to legitimate pre-existing images and representations of rural villagers and local culture. Although fieldworkers may spend months (or, in the case of one project, be from) in rural areas of Malawi, the scripted and fast-paced nature of research and their need to preserve what are sometimes thin boundaries between themselves and their respondents prevents them from meaningfully engaging with village culture. For example, although some survey questions asked respondents whether they had been through traditional initiation ceremonies, respondents' sense that such ceremonies are coded as "bad" by not only national

discourse but, also, by their interviewers, likely contributed to their ambivalent responses. Further, a "yes" response distilled the complicated decision processes that many families discussed on separate occasions.[107] In some cases, especially in accompanying qualitative interviews with survey respondents, culture did come up; however, descriptions of a cultural practice often took the form of scripted responses well familiar to the national imagination (as represented on the radio or in newspapers). Fieldworkers, then, tended to find what they were looking for in the field. They found that village culture was exactly how they imagined it and their future scripts joined a long parade of others that conceptualize culture as backwards, stubborn, in the village and fundamentally different. Training sessions, the need to maintain boundaries and national discourses of "bad" culture help research projects to black box culture in its pre-existing frame. As is the case in development and public health projects, as well, research produces culture as a conundrum or as unknowable.

Reproduction of Implicit Knowledge

Finally, the flexible accumulation strategies described above serve to prevent innovation and to reproduce the same surface knowledge again and again. To succeed in the marketplace of expertise, a local expert must perch himself on the edge of many precipices simultaneously and must be ready to detach from any singular context at the drop of a hat. Although culture chameleons' flexible strategies rely on mobility, this

[107] Many families were well aware of the supposed risks for HIV that male circumcision posed for their boys (the actual risk is negligible since so few young boys are infected with HIV); traditional circumcisers (*ngalibas)* have made efforts to reduce this risk by sanitizing their knives or using a separate knife for each boy. Further, I found that families that had not put their boy(s) through circumcision made this decision not out of health consciousness or fear of HIV but out of sheer lack of funds to pay the circumciser and to sponsor a celebratory party for friends and family. These individual narratives rarely surfaced unless specifically solicited.

mobility is stuck, and experienced as "running in place."[108] Local experts only have time to pack their bags before moving on to another project that is likely to be researching the same sorts of questions and expecting the same kinds of local knowledge from them. Though the case of John described above suggests that upward mobility is possible, this should not be taken as the norm.[109] For most local experts, life is "project to project," and while this mobility *exposes* them to a wide variety of people, research problems and places, they have little time to invest seriously in any one. This tends to legitimate, rather than challenge or alter, the implicit knowledge (or stereotyped facts) described above. Further, supervisors and interviewers complained that the world of research offered no possibilities for professional or intellectual development. All of the projects had streamlined some level of capacity building into the proposals they submitted to national research review boards. However, the constraints of fieldwork permitted only nominal investments in capacity building. Though projects have, in some cases, put supervisors through master's programs or helped them seek out other graduate school opportunities, everyday capacity building holds less promise. One project encouraged Malawian staff members to stay after hours with expatriate staff who taught them word processing fundamentals; another held workshops on preparing an impressive resume in the early (5am or 6am) hour before fieldwork began. These efforts were diluted by time constraints, disinterest or fatigue. One research supervisor wished to advance his interests in nutrition and health since he had worked in some capacity on about sixteen projects the prior year. Yet he felt he had no real skills: "I have no real skills. I don't know how to

[108] Fieldnotes; August 10, 2008. A supervisor used this phrase to capture his frustration with his "stagnant career" in research.
[109] There are other examples of supervisors moving up in research or academic worlds. Since 2004, two supervisors who worked with one case study project have gone for advanced degrees, one has become a practicing lawyer and one has a full time job for a large biomedical research project in the city (Email correspondence, American researcher; March 19, 2011).

analyze data or evaluate projects or develop measures for monitoring or formulating research questions. I only know how to do fieldwork."[110] He learned to write a resume, but had only "projects" to list on it. He explained what he and others expected from the research projects they spent many hours working with:

> All of us would rather learn things to do with how to analyze data, what to do with the data we collect in the field. And if we can be empowered with such skills, that can be better for us. And those are our expectations, but yet if you are doing this research, you have little time to do such things. You are most of the time work, work, work. Then you come back from the field, you are tired and no one can teach you anything. Then you come back from the research, you haven't acquired anything. But maybe if these organizations could be teaching people how to come up with the research questions, how to do proposal writing, how to analyze data, how to use the data we have collected in the field. That can be more useful to them… rather than just [teaching us how to] write our CVs, our resumes.[111]

Though capacity building efforts made by research projects are meant to be beneficial, in the eyes of most fieldworkers they provide only cosmetic or surface level skills. Though this ensures projects have a ready and flexible labor pool, it also makes the labor pool stagnant and works to reproduce the same knowledge. Namely, the same individuals remain in the same positions and offer the same "local knowledge" to projects whose focus on standardization, ambitious work schedules and high quality data minimizes the possibility of the injection of potentially innovative local expertise into data collection processes.[112]

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[110] Interview; September 22, 2008.
[111] Ibid.
[112] Here, it is important to note that knowledge claims or "suggestions" are always products of the intersecting interests of those who occupy a social field. Fieldworkers often made suggestions about what should *not* be done. In one case, a project wished to implement an auxiliary survey called a "social autopsy" that asked rural villagers to recall and provide detailed accounts of the illnesses of deceased relatives. This "autopsy" sought to determine whether past deaths had been due to AIDS, even if not officially recorded as such (Field notes; June 2008). Supervisors told expatriate researchers that these discussions about death would be "too sensitive" and that villagers would refuse. In another example, when a researcher proposed checking the penises of Malawian men to determine whether they were circumcised, supervisors insisted this was not culturally appropriate (Research notes; March 20, 2011). In both cases, the suggestions that methods were inappropriate for "cultural" reasons proved to be overblown. However, the claims were interested; for example, in the first case, fieldworkers did not want to increase their workload in the field by implementing another data collection tool.

Conclusion

This chapter has shown that local lives, identities, mobility and aspirations are affected by the presence of transnational projects in places like Malawi. "Becoming" a local expert entails continual renewal and renegotiation of social roles, relations and exchanges that unfold in an uneven social field (cf. Hall 2002). How might these negotiations influence the data produced by these transnational projects?

Although local knowledge and expertise are highly valued by global institutions, the content of the descriptor "local" is often presumed. Has the knowledge produced about AIDS and other global health issues been improved by the incorporation of a diversity of actors and perspectives? I suggest that viewing the category of local knowledge not as stable or always already present in places like Malawi but, rather, as produced in the everyday social interactions of research can enhance awareness about the contours and limitations of "knowledge." Further, a deeper understanding of the lifeworlds and practices of the actors who are central to knowledge production in the everyday and on the ground can better contextualize the data and knowledge claims we often take at face value. Like the local experts themselves, knowledge is flexible—promiscuously changing its shape and content for its diverse audiences. I have shown that the *mutability* of the actors implicated in collecting data very much affects the form and content of immutably mobile knowledge claims circulated by international research projects. As Lekgoathi (2009) illustrates in his study of the construction of knowledge about the Transvaal Ndebele of South Africa that fed influentially into apartheid policy, African researchers and informants play a central role in making African societies accessible (logistically and culturally) to outsiders. The expatriate researchers of this

chapter reinterpret Malawian ideas, traditions, customs, behaviors and contexts through the prism of their training in a certain discipline and their scripted impressions of "Malawi"—most influentially, however, they complement these perceptions with what local experts tell them or where they take them.

Certainly, knowledge of the "local" is necessary to the production of authoritative knowledge about the AIDS epidemic by social science research projects in Malawi. I have illustrated how international research projects recruit, produce, standardize and evaluate local expertise within their global knowledge making projects' social infrastructure—the first step in producing knowledge about the AIDS epidemic. The next chapter, continuing this focus on *production* of knowledge analyzes the "fair exchanges" between local research participants and research workers. By focusing in on the exchange of a gift (soap) for information-data, Chapter Two illustrates what happens when different claims to information and competing definitions of fair exchange encounter one another. What kinds of evidence do the actors involved in these exchanges enlist to legitimate their claims and further their competing interests? How do these uneasy alignments and potentially asymmetrical exchanges influence the kinds of knowledge about the epidemic produced by social scientific research projects?

Chapter 2

The Exchange of Soap for Information

"Whatever the person gives me, I will receive; a gift is never small (*mphatso sichepa*)."[113]

Following my consideration of how the social infrastructure of research is constructed, this chapter examines how this infrastructure temporarily anchors itself to local field sites by engendering social interactions centered on exchanges—especially the exchange of soap for information. The sentence above captures the words of an elderly woman who was a participant in a large-scale international research project on perceptions of HIV risk in rural Malawi. In exchange for her responses to a three-hour survey, she received two bars of soap. A week after this exchange, I asked her what kind of compensation she and others living in her village might expect in return for answering survey questions; variations of "a gift is always good" were common answers when rural Malawian research participants considered the exchanges they engaged in with global health research projects.[114] Although researchers and research participants in Malawi called the soap a gift, the meanings and interpretations of the term were significantly different. Researchers viewed the two bars of soap that respondents were given in exchange for information as part of international research ethics protocols—a fair but not coercive mode of compensation. Because bars of soap were standardized and neutral, they were an ethical and efficient way to close the researchers' relationship to their respondents. Research participants, meanwhile, viewed the soap as a token that symbolized the obligations of the researchers to return and address problems they

[113] *Mphatso*=gift, prize; *-chepa*=small, worthless, or useless (author's translation).Research participants used both English (gift) and Chichewa (*mphatso*) to describe soap.
[114] Other common responses were "You don't choose a gift" or "There is no limit to a gift."

uncovered; soap was a way for the researched to make claims on researchers as moral actors.[115]

In analyzing the exchange of gifts for information, this chapter makes visible the social and power relations that comprise international research in Malawi. First, I describe and analyze the everyday exchanges and social relations between research projects' fieldworkers[116] and research participants, focusing on how these social interactions have become ritualized and scripted. I suggest that each exchange is nested in a larger international research "market" that assigns significant value to data collected in local sites and, within this frame, pinpoint the main differences between researchers' and research participants' interpretations of the soap for information exchange. This discussion contextualizes my analysis of three tactics utilized by research subjects to "resist" research: complaining about the nature of the soap-gift, hiding from researchers and circulating rumors that cast foreign researchers as *opopa magazi* (bloodsuckers).[117] I interpret this resistance as a discourse through which research subjects critique exchanges they consider improper or exploitative; these tactics emerge from a pool of evidence of exploitation and extraction located in the local historical imagination. I suggest that such critiques indicate that the gift for information exchange validated by international ethics covers over a long history of complicated power relations between insiders and outsiders in Malawi. Finally, I use Derrida's figure of the impossible gift to show how "gifts" of soap reproduce the inequalities that underlie global health and other well-intentioned

[115] In his historical account of the scientific collection of body tissues and fluids from the Fore during the 1940s-1950s *kuru* epidemic in Papua New Guinea, Anderson suggests that, "to engage in these exchanges was to put oneself on the line, to expose needs and desires, to expect recognition and *reciprocity"*(2008:95). In *The Gift*, Mauss, following Durkheim, wrote: "for it is groups, not individuals, which carry on exchange, make contracts, and are bound by obligations; the persons represented in the contracts are *moral persons*…" (1924/1967:3).

[116] See Chapter One.

[117] In rural Malawi, many kinds of outsiders were accused of being bloodsuckers. Bloodsuckers are mythical beings that come in the night to suck the blood of victims, making them feel weak or ill the next morning. The Chichewa *opopa magazi* combines the verb *kupopa* (to pump or suck out) with *magazi* (blood).

transnational projects. Engagement with these issues suggests ways to achieve more equitable research and global health policy that takes seriously the needs, quality of life and expectations of the populations it targets.

Soap as "Gift"

Anthropologists have long been concerned with the logic, social relations and conditions of gifts across cultural contexts. Mauss (1924/1967) is credited with ushering in modern contemplation of the gift; he shows that although gifts may appear free and disinterested, they are always interested, laden with the power to bind or fragment social groups. Drawing insights and patterns from the comparative study of gifting practices in "archaic" societies, Mauss describes the quiet, but well-defined social rules that govern gift giving. The gift obliges repayment; once the receiver repays the gift, the original recipient assumes a certain power over the initial giver in the form of a force that compels the initial giver to give again. Maussian gift exchange rests on a major distinction of the gift economy: the inalienability of a gift from its giver that compels the receiver to give back—"the thing given is not inert" (1924/1967:10). Referencing its potential to reconfigure social relations, roles and structures in all spheres of human life, Mauss calls the gift a total social fact. The soap described in this chapter cannot be alienated. It, too, reconfigures and recruits a wide range of emotions, institutions and relations. Conversely, in a commodity economy, objects are alienated from their owners when they are sold: "Things are in themselves external to man, and therefore alienable" (Marx 1867/1977:182).[118] As the chapter progresses, the differences between gifts and

[118]Hyde writes, "In commodity exchange, it's as if the buyer and seller were both in plastic bags. There is none of the contact of gift exchange. There is neither motion nor emotion" (1983:10).

commodities is borne out in my argument that the information-for-soap exchange enlists moral, market and ethical imperatives and produces value, knowledge and social groups (such as "researcher" and "researched"). Engaging Derrida's figure of the impossible object[119] and collapsing the dichotomy between gift and commodity, I pay significant attention to my informants' surface level classification of soap as a "gift" but show how this verbal classification of soap oversimplifies its circulation through numerous overlapping value regimes and markets.

Exchanges and ideas of reciprocity in rural Malawi are infused with global depictions of ethics, modern notions of progress and development and emerge out of a long history of negotiation with outside actors who entered local contexts momentarily to engage in forms of exchange. Despite their efforts to efficiently but ethically sever their moral and affective relation to research participants at the close of the interview or the conclusion of their time "in the field," researchers are entangled into local contexts and struggle with the claims that rural Malawians make on researchers as "moral persons." In his discussion of Kabre gift exchange in Togo, Charles Piot shifts attention from the object(s) transacted to the emotional and affective transactions that accompany exchange. He writes, "[the] differing and unequal needs of the transactors creates an unequal exchange of equal products" (1999:65). This analysis exposes a problem that is found in the supposed equal or "proper" exchange of soap for information. A gift obtains dramatically different interpretations depending on a giver or receiver's perspective.

[119] The impossibility of the gift, for Derrida, lies in its lack of a present moment to exist in. That is, as soon as a gift is given and recognized as such, the actors involved are drawn into the contract of exchange and debt that exist in the realm of the economic. For a gift to be a gift, it must *not* be recognized as such and must *not* draw its giver and receiver into cyclical relations of exchange and reciprocity. He writes, "A gift cannot be what it was except on the condition of not being what it was" (1992:35).

International AIDS research projects working in Malawi provide a unique site where the conditions, objects, relations and contexts of exchange are constituted and complicated by the ostensibly benevolent intentions of researchers who are gathering "life saving" information from villagers living in a part of the world with high AIDS prevalence. This context is fertile ground in which to rethink Mauss' three axes of obligation: giving, receiving and repaying (1924/1967:37-41). Specifically, the exchanges of international research, precisely because they extend across transnational borders, open up and complicate the space and time of gifting practice. We might hypothesize, then, that the spatial and temporal distance between the givers and receivers of gifts in the international research context serves to efficiently mask or legitimate the assymmetrical power relations it relies on and reproduces. A gift must be individually selected and chosen based on an individual social relationship. In the case of soap, the gift's standardization and bureaucratic rationalization detaches it from its social moorings. Recalling Marx, the social production of the gift is clouded by its commodification. Soap does not emphasize or pronounce, but whitewashes the messy social relationship between the researcher and the researched.

Exchange and Stranger Intimacies in the Field

In focusing on the exchange of soap for information, I look mainly at encounters between young Malawian interviewers and rural villager interviewees.[120] My interest in soap and information lies in the paths they travel along, the meanings they accumulate,

[120] In 2007-2008, I observed exchanges between interviewers and urban or peri-urban research participants. One case study project's sample areas encompassed religious organizations in neighborhoods of Blantyre City. These exchanges exhibited some different features than the ones I observed in rural areas. Whereas rural participants graciously accepted the soap, wealthier urban dwellers often refused it, suggesting the project instead donate it to churches or "people who really need it" (Field notes; June 28, 2008).

the way they divide and bring people together and the meanings attached to them by different social actors. Along with piles of blank paper surveys, boxes containing hundreds of bars of individually packaged Lifebuoy and Sunlight soaps were loaded into the fieldwork vans when they departed from the field office in the morning. These bars of soap, in their distinctive bright red and bright yellow packaging, respectively, would, by dusk, find their way into the hands of respondents from sampled households who had satisfactorily completed a survey-interview.[121] When a research project was in town, villages experienced not only an influx of soap but also an influx of guests. Wandering around with canvas bags bearing a prestigious foreign university's insignia and holding clipboards, these guests collected information from villagers by administering a survey or an HIV test and giving the soap to respondents. The answers given by research participants to the interviewers will be converted from data into knowledge claims and, eventually, find their way into peer reviewed journals or conference papers. The soap-gift will be consumed by the respondent when he or she bathes or does laundry.

Figure 2.1: A bar of Lifebuoy soap.

[121] For the duration of my time in Malawi, soap was the standard gift for respondents. One project provided one bar of Lifebuoy soap while the others provided one bar each of Lifebuoy and Sunlight.

Typically, the interview took place at the home of the respondent. An interviewer (either alone or accompanied by a scout)[122] called *Hodi!*[123] at the entrance to the household or compound of a known respondent. *Lowani!*[124] came the response from inside. The interviewer and respondent met, exchanged pleasantries and began the interview. The successful completion of the interview resulted in the research project's extraction of useful information from the respondent and the respondent's receipt of the soap as a token of the project's appreciation. The encounter, however, incorporated many other modes of exchange. The interviewer would sometimes eat lunch with the family of the respondent, the interviewer would brief the household on current events or happenings in the cities and interviewers would return to the minibus at the end of the day with their canvas bags laden with gifts bestowed on them by generous villagers: groundnuts, sugar cane, small fruits or roasted maize. Often, respondents grew fatigued during the interview and needed a break; none of these unscripted exchanges were recorded.

By the end of a three-month research project, each interviewer had completed hundreds of individual interviews; the presence of multiple research projects in the same sample villages and the need for "follow up" sessions to check initial survey responses meant that both interviewers and interviewees knew what to expect of an interview session. Over time, the interview encounter became ritualized in two senses. First, it incorporated local, traditional norms of hospitality, and second, it incorporated the rituals of international research protocol. A successful interview, to some degree, depends on the

[122] Projects whose supervisors or interviewers were unfamiliar with the local terrain or sample villages often hired a "scout," a local person who was paid around 500 *kwacha* ($3.50) for the day to help the project locate respondents in his/her village or neighboring villages.
[123] This is a greeting used in Malawi to announce one's presence at the gate or door of a private dwelling, meaning "I am here, may I enter?"
[124] "I am here, you are welcome!"

removal of a person from his/her usual milieu and his/her isolation from the distractions, work and people of everyday life. In this phase of the interview encounter, the interviewee is transformed from a villager into a research participant. Though this sequestering of respondents mandated by international ethics regulations and the rubric of confidentiality has become ritualized in Malawi, many rural Malawians protested; parents or older siblings suggested that they did not like the idea of the research teams taking their girl somewhere quiet to discuss what they deemed "private matters."[125] Certainly, Africanists have long questioned the meaning of "private" knowledge; they have documented knowledge across eastern and southern Africa as socially composed or distributed among specialists (cf. McNaughton 1993, Feierman 2000). Frequently, chiefs and villagers alike were baffled that a research project only wanted to speak to a select few of the people in a village: "Why do you want to speak to him? He doesn't know anything about this village; he's a drunkard and a fool." On other occasions, villagers suggested that the research teams speak to a group of people, "so we can all remember and discuss things together. It will be better that way; you'll get a more complete picture of this village."[126]

Ritualized serial encounters produced certain expectations in the study population. Many people said things like: "We always know when [the project] is here. We see the minibuses and we say, 'They have come again to meet with us, to find out how we are staying.'"[127] When I asked an older man what he thought of when he hears the word *kafukufuku* [research], he told me, "I just think the question people are coming

[125] Field notes, Discussion with parents of a respondent in a case study project's sample; February 26, 2008.
[126] Field notes, project introduction to a village head man in Mchinji District; June 3, 2008.
[127] Interview, Madala. N. (Balaka District); August 23, 2008.

again…and I get prepared to answer their questions again."[128] It is at this juncture where the research project meets its participants that we can best examine interpretations of giving, of reciprocity and proper exchange.

The exchange of soap-for-information serves to create the social groups involved in this and the exchanges to follow (researched/researcher). However, this seemingly closed, temporally bounded and reciprocal encounter is infused with meanings, moral interpretations, history and expectations. In fact, it is at the point where the gift is exchanged between the interviewer (representing the research project) and the respondent that we can examine different answers to the questions: What is information worth? What constitutes a "proper exchange"? In the calculus of the interview encounter, what debt(s) remain?

[128] Interview, Wilson M. (Balaka District); August 28, 2008.

Figure 2.2: Interviewers set off in search of respondents (Photo: Author).

The Push and Pull of Timely Data

Each year in Malawi, thousands of citizens participate in international AIDS research projects. This is in addition to participation in other kinds of research: operations research by NGOs, feasibility studies by development organizations or the census, for example.[129] At any one moment throughout the year, a large number of researchers are collecting data across the small nation, but the palpable presence of a "culture of research" is also evident in the chronological layers and (re-) production of research studies and documents. Research conducted in the diverse local settings that comprise

[129] One expatriate researcher said his project had to relocate their sample from the district they had planned on working in to another one because "a project was already working there and we were stepping on each other's toes" (Field notes; September 20, 2007). Case study project fieldwork teams would occasionally run into other research teams walking around the same village on the same day.

global health produces data that has a transnational value. Each localized individual encounter fits into a larger context of nested exchanges.[130]

After its extraction during the interview encounter, how is information commodified into data? I met with Collette Shaw,[131] an academic researcher (based at a British university) who led research projects in multiple African countries, to discuss larger motives surrounding research in sub-Saharan Africa. I asked her what she thought drives research across sub-Saharan Africa; I quote her response at length because her discussion of DevInfo, a centralized database of data collected in sub-Saharan Africa, illustrates the pushes and pulls of thousands of information exchanges across transnational contexts:

> What's really interesting is who is driving the demand for data: You've got the MDG [Millennium Development Goals] and… a demand for up to date reporting coming from the MDG hub and it's being pushed backwards into individual countries. Donors in those countries are suddenly going, "Oh! We need to tie our funding to results and [we've got to] read annual reporting." It puts pressure on people in the country to produce timely data…and individual countries have an obligation to feed those 48 indicators into the MDG indicator database called DevInfo. Tanzania [her main research site] is feeding in via a hub at national level drawing together all of the data from: DHS, Household Budget Survey, Labor Force Survey, [the] Census. It's all very harmonized. [On] the Devinfo homepage there is a way of showing how good or how bad… how much—not quality, that doesn't matter—data is coming in from each individual country. And if you look at the names of the countries, the bigger and bolder the font is, the more data they have collected."[132]

Collette's characterization of the demand for data and results as being "pushed" into individual countries by an apparatus that encompasses them all indicates that information collected across individual research encounters joins larger markets, processes and flows. As it flows, local information is translated into global data; the

[130] By nested exchanges, I mean the integrated social systems that connect the production (extraction) of information, its incorporation into a larger set of information (databases, e.g.), and its consumption by researchers and policy makers (who write journal articles and present findings at conferences or other forums distant from the local collection point).
[131] Pseudonym.
[132] Interview; December 12, 2007.

publically accessible DevInfo data are used to track progress toward MDGs, to draw comparisons across countries or regions and to formulate evidence-based policies (See Figure 2.3).[133] Collette suggests that this drives research projects such as her own to "produce timely data" that can be usefully applied to pressing social problems. This is what motivates the busy research activities and sometimes over-ambitious fieldwork schedules of research teams. The *modus operandi* of researchers and their research teams is to collect as much data as efficiently and ethically as possible; in short, they strive to get in and out of the field in record time. The larger transnational exchanges into which locally sited research projects fit value efficiency and productivity (the more data the better, as Colette's discussion of quantity over quality suggests). The individualized exchanges of gifts and information described here produced a commodity that is in high demand and destined for consumption in distant places. The familiar framing of the AIDS pandemic as a global health crisis (particularly in sub-Saharan Africa)[134] amid globally shared goals such as the MDGs gives new urgency to the wide circulation of data-commodities.

[133] UNDG 2010.
[134] Though Malawi's infection rates seem to be declining, there were 1.2 million new infections among sub-Saharan African adults in 2009, compared with 70,000 new infections in North America (UNAIDS 2010).

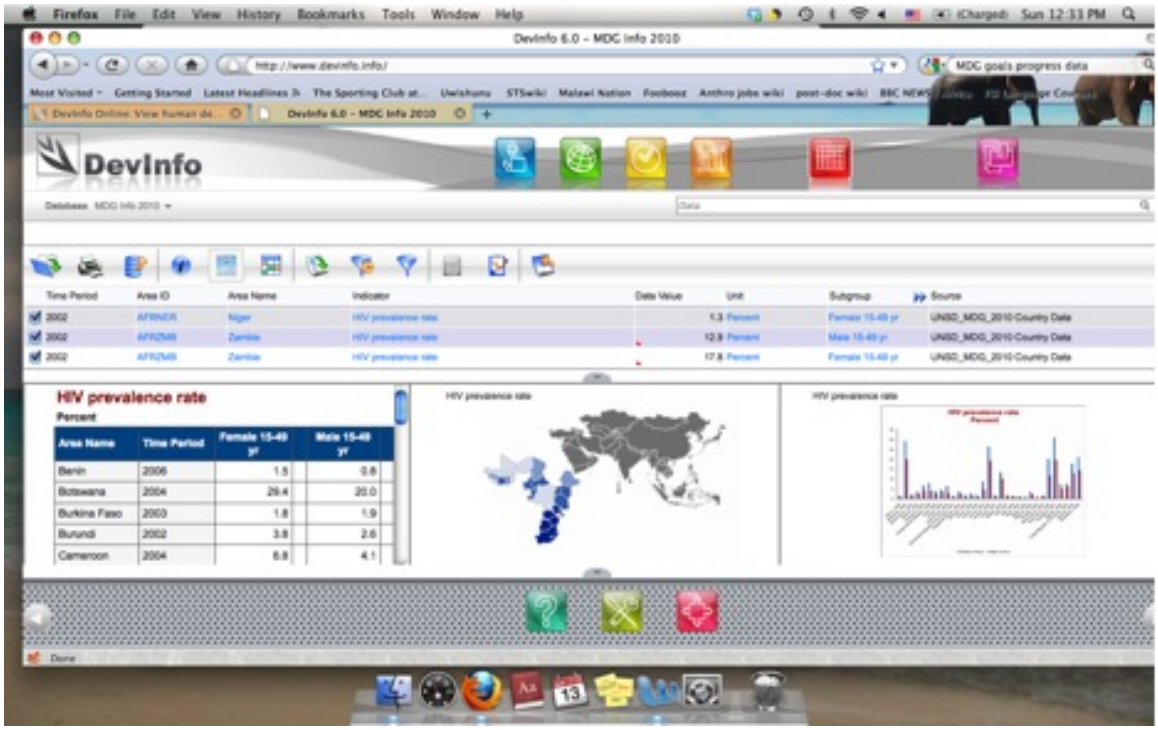

Figure 2.3: A snapshot of the UN DevInfo website. It displays the "HIV prevalence rate" indicator for African countries (Source: UNDG 2010).

Extractive Exchanges: Data as Commodity

The production of the data-commodity and its subsequent entry into networks of circulation (as described in Chapter Three) relies on the extraction of good information within countless individual research encounters like the ones described above. Rural Malawians were certainly aware of the value that their answers to survey questions or their blood or cheek swab samples commanded once they entered transnational circulation. In casual conversations, people claimed the *azungu* [white people or Europeans] would use the things they found out from Malawian villagers to write papers and to "get even more money." Rural Malawians told stories about research project *azungu* "doing business abroad" with their blood or information.[135] In light of these comments, it is interesting to note that in a recent article published in the local *Malawi*

[135] Field notes, Balaka, Salima and Blantyre districts; 2008

Medical Journal, a Malawian bioethicist and his colleagues argue that research participants from limited resource settings should be compensated equally to those in industrialized nations because "they equally contribute towards the study by contributing the same product-data" (Ndebele *et al* 2008:42). The authors' choice of the word "product" to describe data collected from villagers and their suggestion that data from Malawi are equally valuable to data from more developed countries points to the way in which data are nested within and gains value within a larger global research network. When I asked a woman living in rural central Malawi why she thought so many researchers wanted to interview Malawians, she told me, "*Afuna kudziwa kuti zikukhala bwanj"* (They want to know how things here are going) and informed me that researchers want "to know how we live so they can establish why we contract HIV."[136] Her words indicate that rural Malawians understand the value that their body fluid samples and survey responses have in a larger context. Almost all of the rural research participants I interviewed mentioned AIDS in conjunction with research.

Inevitably, as the different levels of nested exchanges intersect with one another, frictions result. Specifically, the extraction of information from participants by research projects is the site of many tensions regarding the definition of proper exchange. Research participants utilize three tactics to slow down, challenge or stop altogether the exchange of gifts for information. Following my discussion of these tactics as they operate in everyday practices, I argue that they function as idioms through which the researched people voice criticisms of the researchers' notions of proper exchange. These

[136] Interview, Mwai J., Chopi (Balaka District); August 25, 2008.

tactics mobilize a storehouse of evidence that soap-for-information is an unfair and non-reciprocal exchange.

Complaining About The Gift

"We have given you enough information."—Research participants in southern Malawi

Researchers return again and again to distant fieldwork sites to extract information to convert into data. "We have given you enough information," however, indicates that this cycle is bound to run into some roadblocks; the word "enough" points to a stoppage or limit. This common sentiment was articulated when respondents were dissatisfied with the alleged closure of the relationship between a research participant and a research project by the compensation of the respondent with the requisite two bars of soap. I grew interested in the performances associated with the exchange of soap as I observed more and more exchanges. In almost every case, the respondent feigned surprise and expressed extreme gratitude for the gift. However, it was clear that everyone in the village knew exactly what a person would receive if he/she was in the study sample.[137] One interviewer, upon reaching the compound of his respondent, was greeted: "I already know what you have in there [in your bag] for me and it's just soap! I don't want that as it's only fifty or sixty kwacha [each bar]- bring me some *zovala* [second hand clothes] and then you come back."[138] These words point to the higher value that a gift of second hand clothes obtains in comparison to soap, according to research participants.

[137] Bars of soap were given to respondents with little regard for who might see. The gifts bestowed on chiefs for their "cooperation" were always wrapped in plastic bags or handed over in a cardboard box, so as not to offend the status of the Traditional Authority by making known the value of his cooperation to his subjects (chiefs were typically given five-six bars of soap).
[138] Field notes; June 3, 2008.

The choice to give soap as a gift was not an arbitrary one. First, soap has become a corollary to research itself. People who had not been sampled for a research project often complained about how lucky those sampled were to "receive some soap and get visitors." Even those who were in the project sample suggested that projects sampled the wrong people and missed out on speaking with people who were really interested in the research.[139] When I asked people what they first thought when they heard that a research project was around, they often said they thought immediately of the soap they would receive. Soap has become, then, a standardized gift. Projects often communicated with one another so as to keep the "exchange value" assigned to a one hour interview constant; research teams and government employees complained about how NGOs have caused people to ask for too much ("Information comes at a price now," they told me) by inflating compensation rates for participation in development projects or surveys. This narrative suggested that NGOs are wealthier than research projects or government and, therefore, cause problems for everyone by giving villagers "too much." Many people also connected well-rehearsed stories of former President Bakili Muluzi's handouts of fertilizer or *kwacha* for votes to the "inflation" of the price of information in the villages. An American Principal Investigator (PI) on a case study project said that the main issue that the ethical review board (the National Health Sciences Research Council) had with her initial proposal when it was reviewed in 2006 centered on reimbursement: "[I]n the US you always have to talk about reimbursement on a consent form and they [NHSRC] didn't want me to talk about reimbursement because they didn't want—it wasn't the issue of coercion—it was more the issue of making people in the future less likely to

[139] Field notes; August 28, 2008.

participate in research if they're not going to be paid."[140] Furthermore, international human subjects research ethical standards discourage the exchange of money for body excreta or information. At a recent conference where I presented a paper about the soap-for-information exchange, I met a former member of the Malawian ethical review board (now a professor at a university in the northeast US) who said my paper had "dismayed" him as "We [the ethics board] worked so hard to make the exchange into a gift exchange, and now they [the villagers] seem to be viewing it again in terms of money."[141] His comments point to the investment of ethics boards and international human subjects researchers in maintaining the gift as an object untainted by money. The head researcher on a large biomedical research project focusing on malaria prevention in Blantyre described things clearly:

> In general, most [of our] projects will offer participants money for their travel. We're quite careful not to offer large financial inducements and, in fact, COMREC is very keen to avoid that, as are all ethics boards. We don't provide food or anything like that. For some of our projects we provide bed nets as part of what we do because what we're trying to do is prevent malaria. But in general the inducements are modest.[142]

Nonetheless, local people were able to historically document the specific value and quantities of gifts they had been given as far back as five years ago (this in contrast to their inability to recall the names of the various research projects or NGOs which had worked in their midst and given them these gifts). Some even saved yellowing consent forms in their homes, bringing them out to show me when I asked about their participation in research. These forms remained long after the gift of soap (or, in some cases, sugar or cooking oil) had been consumed. People would tell me *exactly* what they

[140] Interview, American researcher; July 9, 2008.
[141] Field notes, African Studies Association Meeting (New Orleans, LA); November 20, 2009.
[142] Interview, expatriate biomedical researcher; April 1, 2008.

had received from projects and compare the offerings of one project with another, even if they had forgotten the names of the projects. For example, many participants in a ten-year longitudinal research study spoke nostalgically of the days when they were compensated with a gift of a one-kilogram bag of white sugar, which they valued more highly than two bars of soap. One woman who finished school told me that she felt she lost out because she read the form the project gave her and noticed that it listed the gift to be given as soap *and* sugar; she had only received the former. She explained that the word sugar had been "blacked out with a pen by someone; I just assumed they knew what they [were] doing."[143] When people suggested to me what they thought research projects should provide the main suggestions were always *ndalama* [money] and *zovala* [second hand clothes].

Research projects were nonchalant or ambivalent regarding the villagers' complaints. In their view, soap was an easy, convenient, standard and neutral object to give to respondents. Researchers mentioned that giving bags of sugar meant "a lot of waste" and "harder work for our staff," since the bags tended to burst on bumpy van rides (making for sticky floors) and bags weighed one kilogram each and had to be carried by interviewers for long distances in the hot sun.[144] Furthermore, researchers tended to be against giving money, believing this would promote a "hand-outs" culture,[145] and opposed to giving away second hand clothes because "it's not standardized if you just

[143] Interview, Linda S., Matukuta (Balaka District); August 24, 2008. The soap/sugar comparison was common among my informants. Many people living in Balaka District suggested they had heard that the researchers stopped giving sugar as a gift because research participants accused them of putting contraceptives in it—a widely circulating rumor. "They got tired of tasting the sugar before being able to give it as a gift," one man said (interview, research participant; September 2, 2008). Linda had received a consent form from an older batch. On this occasion, fieldwork teams ran out of the current forms and decided to black out "sugar" on the older ones and distribute them.

[144] Field notes, research team meetings; February and May 2008.

[145] Giving money as compensation for research participation is viewed as coercive (and therefore, not a gift at all). Conversations with members of the local ethics board suggest that they draw a sharp distinction between a "gift" and money, which was assigned a place in a completely different "commodity" register.

have people picking what they wish." Interviewers also told me stories of times when they had given respondents sugar or cooking oil that if someone in the respondent's family fell ill soon after ingesting the oil or sugar, the project would be blamed for the sickness.[146] In short, soap was quick, small, necessary to local families, standardized and easy.

Soap is a commodity long tied up in the colonial production of modern, consumerist and hygienic African subjects; today, Malawian families have come to identify it as a fundamental need. Researchers' perceptions of soap as clean, neutral and standard should be contextualized in the crisscrossing web of the international political economic forces and underlying inequalities between imperial centers and peripheral territories that produced both this neat, tidy commodity and capitalist desires. South African and Southern Rhodesian (now Zimbabwe) legislation such as the Standardization of Soap Act favored manufacturers of soap who had superior access to capital and a foothold in the world system and, thereby, put their local and regional competitors of "local soap" out of business (Burke 1996:97-118). In the same way that imperial soap production produced exclusions and smuggled in larger governing projects under the sign of health and hygiene, soap-for-information exchanges necessarily exclude Malawians who fall outside research samples and extract information for use in humanitarian, health and other interventions on African bodies.[147]

Even this seemingly minor, highly standardized and neutral gift generates debate and demarcates insiders and outsiders. Many people told me that some people in their village appreciated the soap and others thought it was too small a gift, but people who fell

[146]Interviewers tended not to give empty water bottles to young children who begged for them, claiming that if the child should fall ill, the project staff would be blamed and relations between the villagers and the project would be soured.
[147] Doumani (1995) shows how the rise of soap production in Palestine played a central role in solidifying class hierarchies.

outside a project's sample (the "soapless") commonly mocked or exhibited jealousy of those inside the sample. In addition to the uneven social terrain created by sampling, the social landscape that a project entered into was itself the product of past kinds of uneven exchange. In framing discussions of soap as a gift, people often drew parallels with the exclusions produced by the Malawi government's annual distribution of fertilizer coupons to the rural poor. A limited number of coupons are distributed by local chiefs who, according to rural Malawians, may "fail to give them to the right people," causing some families to go hungry because of the prohibitive cost of unsubsidized fertilizer.[148] Other rural Malawians recalled research projects or government schemes they had encountered in the past that gave money only to those who were lucky enough to be selected to participate. Linda S. mobilized a pithy aphorism to express her critique of the pitfalls of this random selection process: *Chimalora opanda mano*! ("Maize [luck] always goes to those who don't have teeth," i.e. good things are wasted if given to the wrong people).[149] Still other villagers often advocated for a fairer distribution of gifts, suggesting that *all* people should have a chance to be interviewed and receive soap or a gift.[150]

Regardless of what gifts people preferred to receive in exchange for sitting down with an interviewer for anywhere from thirty minutes to three hours, rural Malawians value their time. They have come to see the interview as a form of *work* or *labor*.

[148] In 2010, Malawi's Anti-Corruption Board (ACB) began an investigation into the specific problems faced by the 2005/06 fertilizer subsidy program, marked by inadequate coupons, favoritism in distribution of coupons and shortages of fertilizers in most districts. Goodall Gondwe, an accomplished economist and finance minister of Malawi from 2004-2009 is a major subject of this investigation. When the coupons were being distributed, I heard stories of wealthy people buying coupons from villagers who had received them for free, leaving villagers without fertilizer likely to experience a poor maize yield and hunger (*njala*).
[149] Interview, Linda. S, Matukuta (Balaka District); August 24, 2008.
[150] Field notes, Salima, Balaka and Zomba districts; January-September 2008.

Rebecca C., an elderly woman living in southern Malawi, considered participating in research a "job:"

> I expect more than soap because it [the soap] is not equivalent to the job I do as a respondent…it's a very big job; they [interviewers] can ask you so many questions on so many topics and sometimes you just reach a point where you run out of answers and just look at the interviewer.[151]

Also reflecting on the "lost time" of research participation, an 18-year-old ironsmith described how those living in sample villages discourage others [from participating] by saying, "Instead of working on something, you just sit there for hours for nothing."[152] Another respondent said that his friends who are not in the project sample criticize him by laughing at him and saying, "You have stopped working just to stay there and be asked useless questions."[153] A supervisor working on a project in Salima district agreed: "We are increasingly finding these cases where people, household heads especially, own businesses and must be taken away from their work and earnings to answer our questions."[154] In the van on the way back to the field office one day, three of the interviewers on the field research team were talking about one respondent who insisted that she wanted money, even in the form of just fifty *kwacha* (about $0.35) for participating in the interview. The interviewers were laughing: "Look at this woman, thinking she is working, when we are just asking her for some information!" Their laughter is the clear status distinction between the interviewers who were working temporarily for the research project and the woman who, in their opinion, was not "working" at all.[155] Many of the villagers in research samples expressed a similar

[151] Interview, Rebecca C., Chipapa (Balaka District); July 26, 2008.
[152] Field notes; February 18, 2008.
[153] Interview, Humphreys M., Kawanga (Balaka District); August 25, 2008.
[154] Field notes; February 19, 2008. This was a common problem faced by field teams who would often have to search for male respondents working in their fields or doing business or meeting friends at the local trading center.
[155] Field notes; July 30, 2008.

ambivalence for the people interviewing them, often accusing them of "eating our money;" in Rebecca C.'s words, "They come here and instead of fetching food for the children we sit here wasting time [talking] and they go home and eat good food, rice, meat… they leave me hungry and make money as they do so."[156]

In May 2008, a group of young Malawian research supervisors debated whether the unpaid internships starting to be offered by academic research projects to Malawian college graduates were worthwhile or not. The supervisors agreed that any kind of unpaid job is not worthwhile because it marks the volunteer as "worthless." To drive home their point, the group told me about a project at a nearby orphanage where students at the University of Malawi are invited to work as "unpaid volunteers"; the orphanage, in the opinion of the people involved in the discussion was labeling these volunteers as *munthu chabe,* or worthless (or cheap) people, by virtue of not paying them for their "work." "Why would someone ever do that, work for no money?" they asked with great incredulity.[157] Indeed, the increasing reliance on volunteers (for example, as local health care workers) at the local level prompted representatives from UN and other transnational organizations at a meeting between funders and national representatives in Malawi in 2007 to observe that changes in this volunteer-heavy structure were needed: "These expert patients really should be compensated…they are not employees of the state…it is not a priority for them to give up other work and income to do these tasks. Yet we rely completely on them."[158]

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[156] Interview, Rebcecca C., Chipapa (Balaka District); July 26, 2008.
[157] Field notes; May 20, 2008.
[158] Field notes; October 1, 2007. Comments made by a representative from the Global Fund during the question and answer session following the Annual Review of the National HIV and AIDS Response in Malawi in Lilongwe. Swidler and Watkins (2009) suggest that volunteering for development or other projects becomes an important livelihood strategy and a marker of status in impoverished contexts. Conversations with Catherine van deRuit, a sociologist working in South Africa, indicate this is also the case for orphan programming in KwaZulu Natal Province.

District officials, researchers and chiefs expressed nostalgia for the kinds of simple, unquestioned exchanges of information and knowledge in the past; they complained that villagers no longer participate like they used to. The District Commissioner (DC) of a lakeside district in central Malawi told me that the main problem faced by his office was getting villagers to participate in development projects that promised to be beneficial to them in the long run but may provide little immediate benefit to them in the here and now.[159] Similar concerns troubled colonial officials involved in development schemes. In 1930, when the colonial administration began to consider the profit and benefit in training Africans in basic medical knowledge and procedures, a series of letters from missionaries working in Nyasaland to the director of medical and sanitation services indicates that similar questions around voluntary versus compensated participation in such training programs arose. A missionary based at the Livingstonia Mission in northern Malawi in 1930 urged the colonial government to subsidize the missions to pay the native trained hospital assistants who "after being trained, tend to find their way to neighboring territories where they are engaged with pay, unlike here…" ("Letters" 1930). At the 1938 Nyambadwe District Commissioners' Conference, one of the questions put to the room was how the native authority system could deal with what was termed the erosion of "the communal system of village life." In discussing the matter, the district commissioners suggested that "[i]t is now accepted as a general principle that whenever possible payment should be made for service rendered; times are changing and the commercial view point bulks ever large in the native mind" ("District Commisioners" 1938). It was with great reluctance that the British colonial

[159] Field notes, NGO event; October 24, 2007.

office began to contemplate compensation; community development enthusiasts even ten years later saw individualism and competition as threats to African societies and development schemes of the time aimed to protect and bolster local moral and religious institutions, family life and arts and crafts (Vaughan 1982). It seems that in 1938 and in 2008, seventy years later, the same issues arose around fair compensation for kinds of labor deemed menial but beneficial to those engaged in it at some nebulous future point.

One expression of how research participants have come to value their time in relation to labor lost or as a "job" is excerpted from an interview with a supervisor on numerous large-scale research projects:

> Crystal: Almost everyone wants to receive money as their gift especially in Balaka. So how do you feel as a supervisor who's worked on projects and knows the context, how do you feel about giving people money? Or what do you think is a suitable gift for participating in an interview?
> Andrews:[160] I don't know but I will give you another example. It's like maybe sometimes giving people money, some people, some researchers have said "no"…like in Malawi I think we have these rules that in research you don't have to [cannot] give people money. But, you know, things are changing. Times are changing.
> Crystal: The price of maize…
> Andrews: Yeah. Nowadays for you to get anything you need, you need money. So if somebody else comes to your house and then tells you let's sit down [and] we should chat, that means you have lost that time. That could have been productive time but yet you spent that time chatting with somebody. So what I'm observing now in the villages, it's like people…are really starting to value their time. So [when] someone gives them something and they can look at it and value it and say okay from that job I've got this thing. I should give you one example. I was doing this…project with World Bank and these people they said in their budget [that] we should be giving to the respondents 300 *kwacha*. At that time, 300 *kwacha* was a lot of money, 2005.
> Crystal: Even now it would be a nice gift.
> Andrews: So we went somewhere in Mangochi and I was sent to do a life history with a certain lady and when I went there to book my interview for the next day she told me, "I don't have a husband as I'm a widow. I have children so each and every morning I go up the mountain get some firewood and go to town to sell it so I get some food for my children." She said so because of that I can't do an

[160]Pseudonym.

> interview with you tomorrow. She said… I'm a busy person. It's like in the morning when I go up the mountain get some firewood, what I do is I come back here, cook for my children, they eat and that afternoon maybe I go back to the mountain again to get some firewood for tomorrow so that the next morning I have enough so I can carry some to the market. I said, okay, how much do you make per day when you get your firewood? She said I make 150-200 and with that I have enough to buy something for lunch, supper and even a little breakfast. So I said tomorrow I will bring something for you. I will bring you 300 *kwacha.* In fact, I should just give you 300 *kwacha* now. She said, ah, are you telling me the truth? I said Yes! I gave that lady 300 *kwacha.* She was very happy and said tomorrow you come and we will chat. I'm giving the whole morning to you. We will chat here and then you will have your job done. Then the next morning, I went there and I found the lady. She was ready, she had laid her mat [out for me]. We chatted and I had a very good life history.[161]

In this excerpt, phrases like "productive time" and "they have learned to value their time" emphasize the valuation of time. Also important is that the supervisor draws a direct link between the 300 *kwacha* he paid the woman and the "very good life history" he collected. The exchange shows how this supervisor managed to negotiate between the research project's interests in collecting "timely data" and this respondent's concerns about leaving her productive work for too long. Although he did not diverge from the project protocols by giving this woman more than he should have, he did "give" the money in a different way: he formed a contract with her that elongated the exchange (by giving her the money a day prior to the actual interview, he generated a sense of obligation between himself and this woman). Thus, in his own words, he managed to collect quality data by slightly altering the terms and expectations of exchange between himself and one of the project's respondents. In July 2008, another supervisor discussed the large number of refusals that had slowed down research the previous day. I asked him why he thought so many people were refusing to participate. He said:

[161] Interview; September 22, 2008.

> It was better in 2004, when we came here with this same project but we camped in the villages. There, right there in that field [pointing to a big open field near the tea room]. That was better because if people had questions they could come ask us and we managed to eat and drink with them [the villagers who comprised the research sample]. We also brought money to them in the form of hiring local guards for the campsite, cooks, or buying our goats and other foods from them. For long term projects like this one, that is a must. Not this simple coming and going. [162]

It is clear that this young Malawian working for the project defines proper exchange as more than simple compensation. He views the contributions to the local economy and the spontaneous social interactions that happened when the project was sited among the people, as opposed to at a resthouse nearby, as instances that legitimated the presence of the research project and demystified its objectives.[163] Complaints about the soap-gift, centered on critiques of both its form (the nature of the exchange itself) and its content (the value or worth of the soap and its equation with the extracted information).

"Hiding" From The Research Teams

Another tactic that research participants utilized to express their displeasure with the nature of the gift was hiding from research projects. This took the form of literal self-concealment, refusing to participate and pretending to be someone else. Like the other two tactics described in this section, hiding can, at first glance, be interpreted as a form of resistance to research. It has a long history and is colored by the interactions of researchers with local populations through countless foreign research projects, censuses, tax collection activities and health interventions. During colonial health surveys and

[162] Interview; July 5, 2008.

[163] Project staff members often did business with local purveyors of goods like honey, beef or chicken. This supervisor's memory of that field season works nicely in this discussion. However, it should be noted that this perspective may be a nostalgic one that overemphasizes the proximity of the field and office and overlooks the fact that, in the case of two out of four of my case study projects, field offices were located close enough to the "field" to permit interaction with villagers in the sample and to allow the research teams some daily exposure to the context from which data were collected.

medical research in Nyasaland, researchers faced resistance or found themselves having to answer to the concerns of their research subjects. Regarding an upcoming lakeside health survey in 1935, the district commissioner of Chikwawa District wrote to the Director of Medical and Social Services:

> In regard to obtaining samples of blood, dejecta, etc… of the natives for the forthcoming medical survey… on account of the superstitious nature of the people, he [the doctor at the local hospital] anticipates difficulties in obtaining the samples in the way required by your instructions… natives believe that if their enemies obtain possession of such articles as constitute the samples required, acts of bewitching and sorcery would be liable to ensure…"[164]

Though it is likely that these comments by members of the colonial health service partly ensued from unfounded stereotypes and presumptions that circulated at the time, Nyasaland medical reports point to some of the practical difficulties faced by smallpox vaccination teams in 1950: "There is evidence that a large section of the population is still unvaccinated and native vaccinators complain that on arrival in the villages, a number of children depart to the bush to avoid being inspected or vaccinated."[165] Medical teams described their work as a "battle of wits to prevent concealment." Anthropologists, too, faced these suspicions. In the early pages of an ethnographic account, anthropologist J. Clyde Mitchell describes how "villagers slipped away into the bush when they knew [he] was coming" (1956:5). A 1952 Stool Survey conducted to determine the prevalence of parasites in Zomba District faced "resistance and lack of cooperation which can be traced back to the violation of customs and superstition" as they tried to collect adequate stool samples. Eventually, to quell fears over specimens being used to make medicine, survey team members examined specimens "in the open" and then disposed of them by

[164] "Medical Surveys" 1934.
[165] NMD 1939-1952.

"public burial."[166] Whether or not the colonial representations of superstitious villagers are entirely true, they speak to the fact that the fundamental terms of exchange implicated in research of any sort have been an object of consideration and contestation for researchers and their subjects over a long historical time.

During fieldwork, some respondents who knew they were to be interviewed or noticed that a research project was in their midst went into hiding. Sometimes, this entailed concealing oneself by, for example, hiding in the latrine or telling a child to tell the researchers that his or her parent or sibling was not home. Occasionally, projects ran into "fake" respondents posing as "real" respondents (see Chapter Three) either because they wished to acquire soap or because they were more interested in participating than the real respondent. This posing was made even more complicated by the sheer number of people in one village with the same last name (Banda, for example, is the "Smith" of Malawi) or the incomplete information a project held about a respondent in a longitudinal sample. Though the projects had extensive technologies to circumvent these instances of "hiding," some respondents nevertheless managed to slow down, "trick" or avoid research projects in this manner.

[166]Ibid.

Figure 2.4: A household in rural Malawi. Latrine is to the right (Photo: Author).

While some reluctant respondents resisted participation by hiding, others simply refused to participate. Most often, this entailed an interviewer turning up at the respondent's home and being turned away. Certainly, a number of people who refused to participate tended to blame it on the too-small value of the intended compensation or gift (the two bars of soap). However, when I inquired with people who refused, the reasons turned out to be more complicated. In the case of one project, many refusals could be attributed to respondents' dissatisfaction with kinds of research exchange or encounters they had been involved with in past years. One man who had recently refused to participate in the survey explained his "research fatigue:" although he accepted people coming to his village to teach about AIDS, he refused to participate in research projects

because, for him, it makes no sense to "keep asking villagers about AIDS."[167] Another respondent who refused to be interviewed described his sentiments on the matter:

> I won't answer those silly questions; people already came here [a few months back] and some of my friends chose some bottle caps with *kwacha* on them and, me, I chose a cap and it had nothing on it. If they are coming here to fool us again, just tell them don't even come![168]

This man was referring to a cash incentives project that had passed through the same villages a few months earlier. As this project was "using" the same sample as the ten-year longitudinal study, the interviewers donned the same T-shirts with the same insignia; this marked the interviewers who arrived months later as "one and the same" as the ones who had come before them to the villagers. The cash incentives study was testing the hypothesis that people provided with cash incentives would be more likely to remain HIV negative. This experimental design involved study participants randomly receiving an experimental set sum of money (different across individual villages) per year, such that the degree of the incentive could be correlated with ultimate HIV serostatus at the close of the study. Some people in the sample villages that I spoke with about this study interpreted those who had chosen bottle caps with amounts of *kwacha* printed on them as "lucky people." In Malawi and other developing contexts, cash incentives projects are increasingly popular and considered an effective way to improve rural quality of life and decrease risk of HIV infection (UNGASS 2010:105). Despite aggressive efforts on the part of research project staff members to "sensitize" rural participants in such projects about experimental design and "randomness," villagers interpreted the distribution of incentives in their own way.

[167] Field notes; August 19, 2008.
[168] Field notes; August 5, 2008.

In September 2008, I accompanied the field teams for one such cash incentives project to the monthly cash distribution point. This project was similar in design to the HIV incentives project, but was interested in how much money rural households would need to be given to ensure that girls within the household would remain in school as opposed to working in the fields or around the house. The most significant aspect of the study design was that half the households chosen for the sample were deemed "non-conditional" and half were deemed "conditional."[169] Households in the first arm were given money every month whether or not their girl(s) attended school regularly. Conversely, those in the second arm were only given their money if the girl(s) attended school a certain amount of days per month. At the distribution point, a withered but lively *agoga* (grandmother) was told by the fieldwork supervisors she would not be receiving her household's money this month because her granddaughter had not attended the minimally required number of days at school. She was angered at this news and, first, insisted that her granddaughter had, in fact, been in school. The project managers went to fetch the headmaster and the keeper of the attendance books, to verify school attendance. As originally suggested, the girl had indeed failed to attend the required number of days. At this point, her grandmother reframed her arguments. She pointed to another old woman, standing under a tree nearby and clutching her envelope filled with money. "But…" she began, "her granddaughter didn't attend school all month! I know she didn't even go to class at all, even once! How come she received [money] and I didn't?"[170]

[169]The project included a "lottery" component where girls in the sample chose a coin from among a clutch of coins in a bag. The amount printed on the coin varied, and was paid directly to the girl on a monthly basis as "pocket money." The girls often complained that a friend had received more money.
[170] Field notes; December 4, 2007.

Flustered, the project managers tried their best to calm the woman by explaining that the other household was in the "non-conditional" group and, thus, their girl was not required by the terms of her contract with the project to attend school. These examples help us to understand why, then, a chief referred to researchers as *anganya* (something like: "little thieves"). The example of the misinterpreted "conditional" and "non-conditional" arms of a study and many others indicate that research participants weigh past and future benefits when considering whether or not to participate in current research projects.

Circulating Bloodsucker (*Opopa Magazi*) Rumors

Finally, research participants critiqued the exchanges that they entered into with international research projects by exoticizing foreign researchers, linking them to macabre mythological stories about bloodsuckers. These stories are part of a larger, transnational and transhistorical genre of accounts that demonize unwanted or dangerous "others."[171] I argue that the accusations of bloodsucking against researchers implicate them in a long history that, though temporally distant, colors research participants' expectations of contemporary exchange. The stories deploy and organize evidence that the soap-for-information exchange is extractive or non-reciprocal. The presence of a research project serves to activate these stories from where they lie latent in the public imagination.[172] In this section, I first describe the wide if uneven circulation of the

[171] Many have documented these rumors across historical and geographic contexts in Africa (White 2000, Fairhead *et al* 2006, Fassin 2007). These scholars suggest that such stories can be read as condensations of historical social and power relations. Geissler writes that rumors accusing staff of a clinical trial in Kenya of being *kachinga*, or butchers, "intertwine local patterns of relatedness and wider global connections evoked by the research situation" (2005:175).

[172] We might think of the collection of information from villagers as the uncovering of an open wound and rural research participants often suggested that they put their problems "on display" to no avail. "Help never arrives…" In cases where villagers had their blood tested for HIV, they often framed this "knowledge of my AIDS status" as a major *benefit* to participating in research. This challenges the stereotype of Africans as superstitious about blood; many villagers had no issue with surrendering blood to discover their status.

bloodsucker stories and present the normative plot and narratives of these stories. By incorporating an array of comments on the stories and their meanings, I show how they are activated by extractive research exchanges and function to critique the "vampiric" research practices by now so familiar to Malawians.

First, the bloodsucker rumors enlisted plots common to stories that circulate widely in Malawian society. Soon after I arrived in Malawi in September 2007, I began to hear stories about the bloodsuckers. While people from all walks of life—taxi drivers, people I met in restaurants, villagers, district health officials and researchers—were familiar with these beings, the repercussions of their existence in stories were different across social categories; villagers felt terrorized by and afraid of bloodsuckers, others simply dismissed them as silly villagers' stories and researchers grew frustrated with the circulating rumors of bloodsuckers that delayed their collection of timely data.

These stories actively influenced social relations between Malawian research participants and research teams. One research project, in the early stage of piloting its project in a district notorious for bloodsuckers, was, according to a graduate student hired to oversee fieldwork in Malawi, "literally chased from the villages" when the project's SUV was pelted with stones by villagers who claimed the vehicle carried bloodsuckers.[173] Ultimately, the project had to relocate to another field site. In a neighboring district, just six weeks later, health surveillance assistants newly assigned to a rural health post were chased from the post by villagers who vandalized the clinic overnight and threatened them with violence.[174] Soon, the national newspapers were rife

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The terms of this exchange were simplified by the technology of rapid blood testing; villagers could give blood and know their status almost immediately (via a finger prick test).

[173]Field notes; November 2007.

[174]Interview with Deputy District Health Officer (DDHO); November 21, 2007.

with dramatic headlines about bloodsuckers. A late September 2007 edition of the *Malawi News* proclaimed "Bloodsuckers terrorize Chiradzulu!" (Mmana 2007)[175] and juxtaposed villagers' "hysterical response" with the denial of the local police force that bloodsuckers exist. In December 2007, a local party official in Balaka was beaten up on allegations that he was linked to bloodsucking (Muwamba 2007) and a local magazine ran a feature story with glossy photos of a "young victim" of bloodsuckers and a narration of his ordeal (Malikwa 2007).

Intrigued by the popularity of these bloodsucker stories, I documented as many as possible. In addition to interviewing "victims" of bloodsuckers, I sought to place these stories in historical context by examining documents in the Malawi National Archives (MNA) and newspaper archives at the Centre for Social Research's (CSR) Documentation Unit. There were a large number of references to the *opopa magazi* in 2002-03. In December 2002, Malawian President Bakili Muluzi made public statements to disassociate his government from "stories" that it was sucking people's blood in exchange for maize donations from foreign governments. He attributed the source of the rumors to the opposition party, accusing its members of spreading malicious lies to destabilize the government in the eyes of villagers (e.g. Munthali 2002; McFerran 2003).

Informants indicated that the stories surfaced periodically in times of uncertainty; a Traditional Authority (TA) in rural Zomba district told me that these rumors have circulated for many years, and "come back again and again." As for the rumors that were circulating at the time we spoke, he traced their rise to "politics." He explained:

[175]International newspapers seek out these "outbreaks" of bloodsucker rumors and report them in an exoticizing and sensationalist manner (e.g. Reuters 2003).

> This area is a UDF [political party] stronghold; the MCP [political party] has put the rumors of the *magazi* in people's heads here to hold back development [and research] so that when people chase the projects from here they move to other places, ones which are supportive of the ruling party.[176]

Although he attributed the source or origin of the rumors to "politics" (broadly speaking), the consequences of the rumors directly related to the distribution of development projects and the resources they bring to local communities. In the same conversation, he suggested that the rumors reveal the problems with exchange. Although his villagers are generally eager for projects to "come to help us," he also suggested that people do not believe it is possible to get "something for nothing" and, thus, assume that any project claiming to be helping or donating things must be expecting something in the future from them.[177]

The bloodsuckers stories circulate within and draw on the public imagination. Stories had the same general plot line, style, tone and roles[178] although the details and "props" employed by storytellers varied. Some accounts incorporated a torch (flashlight) whose light had the power to weaken humans, while others employed a needle used to extract blood and still others mentioned pipes inserted through holes between the bricks of village houses to "suck blood." Moreover, the bloodsuckers themselves were not a permanent social category that served to identify the same person or group as thieves or wrongdoers; the category shifted to encompass various social actors (such as health surveillance assistants, physicians, nurses, politicians and researchers).

Different interlocutors had their own theories about why villagers spread rumors. A taxi driver said the rumors are "seasonal" and tend to emerge during September and

[176] Interview, Traditional Authority, Zomba District; December 4, 2007.
[177] This "danger" of the gift is emphasized by Derrida in his suggestion that for a gift to be a gift, both giver and receiver must undergo radical forgetting: "For the symbol [the gift] immediately engages one in restitution" (1992:23).
[178] Cf. Priscilla Wald (2008) who"plots" the narratives about emerging epidemics.

October. Because of the extreme dry heat this time of year, he said, villagers find it difficult to sleep or get less sleep. They wake up unrested and then walk around all day in the hot sun, feeling weak or tired, and this makes them believe they have had their blood sucked in the night, "weakening" them.[179] While media coverage showed many outbreaks occurred between the hot summer months of September-December, rumors also surfaced in the much cooler months of May and April. A sociologist at the University of Malawi observed that the bloodsuckers stories were not "like maize or the rains" which come every year: they don't come every year and you never know when they will surface."[180] He recounted his experience with a project that resettles people living in crowded urban areas to less congested rural places. There was resistance in the form of rumors that the government was placing the urban poor in "carrels" in the rural areas to fatten them up and then take their blood. The rumors grew so powerful, he explained, that autocthonous rural villagers migrated away from the proposed resettlement sites to "avoid the crossfire" or fallout from this tense situation.

Amid stories of *opopa magazi*, questions about who should protect their victims also surfaced. When villagers claimed they had no one to protect them, the legitimacy of the postcolonial state came into question in much the same way it did during colonial era witchcraft cases in British southern Africa, where, "colonial law did not take cognisance [sic] of the efficacy of witchcraft, and, much to the distress of the victims and complainants, did not treat purported killing by witchcraft" properly (Chanock 1998:82). Today, as in the past, there is a deferral of authority in cases of bloodsucking that lie outside the realm of the state court system (Jul-Larsen and Mvula 2009) and tensions

[179] Field notes; November 26, 2007.
[180] Field notes; February 8, 2008.

arise between overlapping and sometimes disparate customary and constitutional law in post-colonial contexts (Widlok 2008). In fact, the only official response of the Malawi state to the bloodsucking rumors occurred in October 2007 with a declaration that anyone caught spreading such rumors would be arrested. Rural informants also suggested that government health institutions could not protect them against bloodsuckers: "When someone has been [blood]sucked, they sometimes go to the hospital but they just say they don't have the medicine for that, so the person ends up walking home."[181]

It is precisely because the accusations contained in bloodsucker stories are leveled against individuals who are clearly *outside* the control of the state that the state is compelled to respond. If the state sits silent in the face of citizens' complaints that foreign researchers are extracting information, its control over matters of national concern is eroded and its imagination as a weak state is solidified. While a colonial government could "look the other way" when faced with witchcraft cases or other instances of so-called "supernatural activity" because the state was itself an *outsider* to these practices, the contemporary Malawian state is called to intervene or acknowledge these same sets of security issues precisely because it risks being illegitimate if it does not respond. This analytic of insider/outsider is also central to the operation of the *opopa magazi* stories.

Anthropologists have shown witchcraft accusations result when improper accumulation happens (usually by a so called "selfish" individual) at the expense of kin relations. By contrast, the *opopa magazi* stories typically cast *strangers* as villains who accumulate or extract wealth or knowledge of some sort. These extractions rely on outsiders gaining access to people that are unfamiliar, unknown or geographically distant

[181] Interview, Linda.S, Matukuta (Balaka District); August 24, 2008.

from them. Many victims or purveyors of the bloodsuckers stories blamed their chief for allowing the *opopa magazi* to access their community for "a few *kwacha*" given to him/her by research projects. Obviously, the key themes of wealth, redistribution and exchange characterize both witchcraft stories and bloodsuckers stories, but it is important to note the distinction: while improper sharing or distribution happens within and between *socially intimate* kin groups, improper exchange in bloodsucker stories relies on *social distance* between researchers and the researched.

Though previous studies of vampire stories as they emerge around technoscientific research in Africa have focused on biomedical projects that center on "stealing" blood or body tissues (Geissler 2005, Fairhead *et al* 2006), bloodsuckers stories in Malawi also circulated around research and other projects that were not extracting blood. In many cases, people viewed those who came to collect *information* from them as bloodsuckers. Like blood, then, the responses that locals give to interviewers belongs to them. Information is a scarce resource that is owned by the person who provides it. When it is entered into boxes on a survey, it only appears to be alienated from them. A woman who had recently participated in a survey said: "I do think research is important. The findings can help improve our lives. But I do ask… Why are they [researchers] stealing my voice?"[182]

Unmasking the Gift, Unwrapping the Soap

The countless exchanges of information for soap in the context of international AIDS research are central to the production of knowledge by research projects. These

[182] Interview, Tiwonge L., Nkumba (Balaka District); Augusr 25, 2008.

exchanges make possible the extraction of valuable information and its subsequent enlistment into transnational circulation *precisely because they are coded as gift exchanges.* International research ethics preclude the possibility of monetary compensation (or payment), assigning to money a coercive power. Researchers and policy makers were committed to preserving the purity of the gift (consider the frustration of the ethics board member who was upset that villagers were viewing soap not as a gift but as "payment" for participation in research). The neutral, innocuous and healthy object of soap makes it an ideal gift, ensconced as it is in a hygienic wrapper that protects it from the "dirty" connotations that research ethics assign to money. However, the "gift" also serves some more poisonous functions.[183] First, the gift in international research hides a long history of power relations and exploitation that continues to frame local interpretations and expectations of exchange. Second, naming soap a "gift" effectively removes research participants from the commodity economy that their information and body samples enter into, relying on and reproducing asymmetry between researchers from the global North and research participants in the global South. Finally, the gift of soap calls attention to the exchanges in global health and other projects and questions their "benevolent" intentions.

The Long Dureé of Exchange

Popular and academic accounts of global health research often reduce the actors involved to "powerful" researchers and "powerless" research participants. The tactics utilized by the latter to critique the extractive practices of the former, however, have

[183] Mauss traces the etymology of the word *gift* to ancient Germanic languages and suggests there was a dual meaning: gift and poison (1924/1967:62).

shown that things are more complicated. Further, this chapter shows that transnationally validated ethical standards are a sort of straitjacket for researchers, many of whom recognize the problems with a soap-for-information exchange. Often, accounts that paint researchers as villains and villagers as romantic resisters fail to reckon with the larger structural standards and regulation that do not make room for non-bureaucratic or personalized negotiations. As the AIDS epidemic continues to plague sub-Saharan Africa, AIDS research has taken on increasingly crucial importance; data collected from Africans can provide important clues or directives for policy makers or clinicians interested in mitigating the spread and social trauma of the disease. In this way, the state of exception that is AIDS blends with the assumed "social good" that research can do to divert attention from the everyday exchanges within which information is collected and transformed into data. Resistance to participation in research projects is often attributed to the ignorance or "backward stubbornness" of villagers. These explanations, however, misrecognize the long history that has cultivated such responses.

In Malawi and other post-colonial contexts, information has always been a valuable commodity. Researchers and "the researched" have long bartered and negotiated with one another. In an ethnographic exploration of the history of the engagements of scientists and the Fore of New Guinea during the *kuru* epidemic, Warwick Anderson shows how the drive to collect Fore brains and body parts in the 1950s and 1960s led to particularly peculiar kinds of exchange; "…brains for blankets, urine for knives, blood for tinned fish…" (2008:2). Today, bioethics and human subjects research guidelines foreclose the possibility of these kinds of haphazard, unscripted and potentially coercive exchanges. By the 1990s, blood samples collected without controversy by earlier

generations were seen by global ethics bodies as deeply embedded in power relations and subject to the constraints of ethical disclosure, consent and sensitivity to cultural context (Lindee 2003). Globally validated assumptions about fair exchange rub up against local conceptions of proper exchange.

The tactics employed by rural Malawians to resist research and exchange practices that they saw violating "proper exchange" are not wholly "moral" or wholly "local." Instead, local moral economies draw on or co-opt global symbols and elements to critique research practices. For example, the criticisms leveled against research projects' small gifts of soap often were accompanied by suggestions that participants should instead receive money as compensation or that it was participants' "human right" to receive health care if a project found them ill. Similarly, research participants' expectations were informed by their past participation in other projects. After one of my case study projects had departed, I interviewed both respondents and people who had not been included in the project sample. I asked these individuals (and others, more casually) what they thought "research" meant. People assumed that if a research project was working in their villages, they could expect some sort of help or benefit in the near future. Despite the fact that researchers worked very hard to be sure that they presented their plans to research participants and traditional authorities, villagers tended to link the stated research projects to the anticipated benefits that would inevitably come to them in the future. While academic researchers emphasized the exploratory or academic nature of their work, villagers made assumptions about the kinds of "returns" that research would bring to them. A young bicycle taxi driver said: "So many projects are coming here and not giving us anything and breaking their promises, unfulfilled promises. It's time for

them to stop asking us so many questions and start doing something for us."[184] Other people said they expected research projects who found them ill or poor to provide medication to cure them or *kwacha* to help them buy necessities. One man summed up a widespread sentiment: "They [the researchers] use the survey responses to shed light on the kinds of problems villagers are facing… then they come back and do necessary things or erect projects that will help people change their lives."[185] Finally, an elderly woman told me that she thought researchers collected answers from people so they can "make sure everything works alright, and if it doesn't, after *amasankha bwinobwino* [they analyze/count the answers well] they come back to us *kuti tigwirizane nawo* [to bridge the gap]."[186] Neither this project nor any of the others I observed had plans to provide their research participants with medications or money or to start projects that would improve material life conditions for these villagers. In proposals submitted to funding organizations, conference papers delivered, or proposals to Malawian ethical review boards, research projects tended to cast their "helping" role[187] in terms of potential contributions to AIDS policy, a position that one Malawian research supervisor referred to in this way: "These researchers are just telling people, oh, policy making… people are so tired with this policy making! They don't care!"[188]

People tend to ask why rural Malawians continue to expect things from projects that repeatedly bear no fruit, or why they do not learn from past experience and abandon expectations of benefit. People in Malawi are used to waiting; one might say that the

[184] Field notes, Salima District; February 19, 2008.
[185] Field notes, Balaka District (July-August 2008), Salima District (January-March 2008), and Zomba District (May 2008).
[186] Interview, Agness A., Chopi, (Balaka District); August 18, 2008.
[187] The research market in Malawi insists on research that can be convincingly cast as "practical" or "relevant;" this mandate, however defines its terms loosely. The manner in which researchers grapple with this but still manage to do academic research is by using the language of "policy relevant" and "local capacity building" to frame their work as "useful" or "beneficial" to Malawi.
[188] Field notes; July 30, 2008.

primary genre of social action in Malawi is a "politics of waiting." People wait for fertilizer subsidy coupons, for deliveries of food aid from World Food Program (WFP) during the 'hunger months,' for technology or DVDs they have heard about from relatives living in South Africa, for the annual rains that sustain small scale farmers in the country and for remittances sent home from family members working abroad. In short, Malawi's peripheral position in the global capitalist system has conditioned its people to waiting. Many objects, inputs or material benefits that Malawian citizens wait for are delivered to them through unreliable, unpredictable, largely unaccountable and mostly opaque systems.[189] In this way, it makes good sense that the respondents in research project samples, though disillusioned with the lack of direct benefits from their participation in various projects, still anticipate future possible benefits.

Contrary to interpretations of resistance to research as spontaneous, backwards or silly, the long history of research and other interventionist projects in rural Malawi where research projects are currently collecting their data constructs and informs the lens through which rural dwellers apprehend the present. While experts or researchers tend to imagine their projects or interventions as building upon a clean slate, the physical and social remnants of past encounters between outsiders and villagers very much informs the terms, expectations and criticisms of present day research practices. Even if the historical interactions of villagers with other outsiders remains opaque to researchers, the researched are generally able to clearly recall their personal research histories, e.g., "Like for this year, I've had three interviews. Every year we have interviews from 1990 up to

[189]The opacity of these systems is gradually being eroded by the large number of rural dwellers who own mobile phones. There were a few stories about villagers who had phones using the phone numbers of research project principal investigators listed on consent forms to ask when they would be back or when they would inform the village of their findings.

now…"[190] Thus, while rural Malawians certainly draw on traditional norms of giving, reciprocity, and exchange to critique or delegitimize research practices deemed extractive, they also very much draw on universal notions of rights and ethical practice that they have become familiarized with *through* their interactions with researchers and research projects. We might say that rural Malawians have become "research-conscious subjects" whose very familiarity with research informs their efforts to make the most of projects whose aims and stated rationales may, at first glance, seem to be of little interest or relevance to them. A Malawian sociologist scoffed at the aim of projects to change lives or measure outcomes; "lives are not projectivized; a person's life does not change in a project site!"[191] His words ring true, but even if lives themselves are not transformed in line with project objectives, individuals' orientations to life possibilities are informed and redirected by their encounters with such projects.

[190] Interview, Edward B., Chipapa (Balaka District); August 23, 2008.
[191] Field notes; February 10, 2008.

Figure 2.5: Children observe the field team and project minibus (Photo: Author).

Soap as Impossible Gift

Research participants do not easily forget the past: instead, they draw on historical exchanges to frame contemporary interactions and expectations. This confounds a conception of the soap as true or pure gift. In *Given Time,* Derrida (1994) engages with anthropology's preoccupation with the object of the gift. He critiques Mauss, especially, for too easily assuming that the gift provides an exception to stringent laws of economy: a thing freely given with no expectation of return may exceed or escape the "economy." As stated, Mauss shows that gift-giving always entails obligations and reciprocity. Derrida questions Mauss by asking if a gift can actually exist. Does a gift not reinscribe economic laws even more effectively because they become internalized in the actors who

"freely give" gifts?

Derrida suggests that the gift is impossible: for a gift to be given, both giver and receiver must experience an absolute or radical forgetting (1994:16-17). In this reading, the research project would have to give the soap to participants *without recognizing it as a gift* and the research participants would, likewise, anticipate no future exchanges with the research project. Furthermore, since participants provide the project with information *before* they are given soap, information becomes the gift and soap is the counter-gift. While the information's non-materiality distracts both parties from conclusively naming it a gift, both parties in the exchange perceive the soap as a gift and both parties stake certain expectations on the soap. Derrida writes, "For there to be a gift, there must be no reciprocity, return, exchange, countergift, or debt" (1994:12). Considering the Malawian research situation, we agree with Derrida's claim that the gift (soap) is, indeed, an impossibility.

Why are researchers, policy makers, and even research participants invested in maintaining soap as a "gift?" Why must the exchange of soap for information be interpreted in the frame of the gift? Like Mauss, researchers are drawn to the gift as an object capable of standing outside laws and markets of economic networks. The soap, cast as a gift, is a fitting object, exchanged in places far from the center of transnational offices where data is analyzed and enlisted into knowledge claims. Many of these places, in fact, stand outside of or remain peripheral to global economic flows. However, the intersection of research projects' worlds with those of research participants exacerbates relations of lasting asymmetry, a rough terrain where the soap—imagined as non-economic—enters economic and circular exchanges. First, the gift of soap is given only

after research participants give information or blood samples to the research project. While the soap is soon consumed, the information is immediately enlisted into channels of circulation that begin at the point of extraction. The gift's appearance as free and disinterested, as Mauss suggests, conceals its "truth." The extraction of information is the precondition for the project's giving of the gift; the soap becomes a Derridian impossible gift by virtue of its entrance into a relation of exchange or reciprocity. Further, the soap's role in ethically closing off the project's perceived obligation to its subjects distracts attention from the converse side of this exchange: the information extracted from respondents quietly attains an "economic" value as data. Finally, Derrida's sense that the gift should deny reciprocity or symmetry is confounded by the exchanges of soap for information. Namely, soap is "given" *for* information and both researchers and research participants experience anticipations and expectations within these exchanges. Researchers expect that the giving of the gift will enable future extractions of similar information from the same individuals or research sites to go smoothly; research participants expect the information and time they "invest" in surveys or interviews to bring favorable returns (in the form of interventions, medicines or solutions to local problems brought by the project to them in the future).

The gift of soap, then, is not *aneconomic*: its differing interpretation by researchers and research participants only serve to draw attention to the interlocking systems of exchange that characterize international AIDS research. Yet researchers, international ethics boards, and policy makers stubbornly cling to this impossible object because the soap-gift serves an important social and ideological function. By compensating research participants with a gift, research projects remove their subjects

from their time, their markets, and their (first-) world. By locking research participants in Malawi in "another" time and "another" economy, research projects reproduce the unequal power relations that enable "us" to research "them" in the first place.

Africanists and policy makers are well poised to analyze the actual exchanges that inhere in "gifts" ranging from global health clinical trials in the developing world to foreign aid to microcredit projects. As projects with good intentions in contexts of global health (or economic) crisis and exceptionalism, they often remain immune to criticism or problematization. A systematic analysis of the everyday relations and interpretations of the kinds of exchanges that comprise international research can shed light on "outcomes" that are often overlooked in a system that relies on quantified and technical measures and end points. Although global health projects are held accountable to high standards of data collection, to local and global ethics boards and to funding organizations, they are often unaccountable to the local research participants who are expected to continually participate in their projects. What outcomes do these individuals expect? Even as the gift of soap plays an obvious role in legitimating the knowledge produced by international research projects, it also serves to (imperfectly) cover over or mute the exploitative social relations that make its "magic" possible.

While Chapter One elaborated the social infrastructure of the international AIDS research project, focusing especially on the ways in which expatriate AIDS researchers imagine, recruit, evaluate, value and, most importantly, *produce* the object of local knowledge, this chapter has argued that the soap-for-information exchange that underlies the production of legitimate, ethically collected data is subject to multiple and competing interpretations. Even as the compensatory model for human subjects research enjoys

transnational dominance and relies on the preservation of the soap-gift (or other small, non-monetary means of compensation) as symbolic of ethical exchange between researchers and their subjects, these subjects draw on historical knowledge, public memory and informed analysis of costs and benefits to make claims on research projects. In their shared focus on the "field" of research fieldwork, both chapters suggest that everyday practices, social relations and exchanges have important influence on the knowledge produced by international AIDS research projects. Chapter Three shifts the lens from the field where data is extracted to the office where data is collected, ordered and made to circulate. It takes a more top-down approach, focusing on how shared epistemic virtues among researchers and their audiences have significant effects on everyday fieldwork practices that aim to produce "high quality" and "timely" data.

Chapter 3

Seeing Like a Researcher: Miniaturizing Social Reality and Managing Uncertainty

In this chapter, I shift my focus to the office and to the *movement* of people and information between the field and the office to illustrate, first, how data are unmoored from their context and, second, how they come to circulate widely. I suggest that data's mobility is central to the legitimation of knowledge about the AIDS epidemic in southern Africa by empowered audiences who rely on evidence—not first hand observation of African settings—to make policy and frame interventions. I follow data in order to ethnographically illustrate the multiple transformations that information undergoes in its travels from the initial interview encounter to the field office and beyond. How is information detached from its local context and mobilized as AIDS-related data? What sociocultural processes underlie the conversions of people into data points, households into dots on a map and survey responses into "high quality data?" What invisibilities and limits are enfolded into the project of "seeing" like a researcher?

Quantification as Sociocultural Process

Numbers are the primary way that we know about AIDS in Africa. Claims such as "11.9 percent of Malawians are infected with HIV"[192] or "1.6 percent of the total adult population of Malawi is infected with HIV each year"[193] are numerical generalizations that assume congruence with social reality. These statements are considered authoritative because they are based on data collected from a distant local place. Data are enlisted into knowledge claims about the AIDS epidemic that miniaturize a complicated geographic

192 UNAIDS 2008.
193 UNGASS 2010.

place into a manageable and mobile form that can circulate globally, sending an efficient abstraction of "Malawi" from the field to a university office to reports for the Malawi National AIDS Commission and refereed publications for an international audience, and from there onward to the UNAIDS compilations of data from Malawi on its website, where they are frozen for eternity. A nation's or even a village's complexity and dynamism cannot, of course, be wholly captured by researchers: much potential information must be ignored or excised in the process of miniaturization by which people or places become data points. But individuals can be surveyed, interviewed, counted or HIV-tested in order to generate numbers that are taken as authoritative representations of women and men grappling with the AIDS epidemic. This chapter describes and analyzes the techniques of enumeration that, first, allow researchers to "see" and manage rural realities and, second, permit the miniaturization of local social worlds into widely circulating global data.

Although many international social science research projects do not claim that their findings represent a larger national reality, nor seek to intervene into social problems,[194] other actors enlist or consume their findings to make knowledge claims in the fields of policy design and global health—fields with more explicit stakes in representing and intervening in the AIDS epidemic in southern Africa. The numbers become the foundation for proposals for funding programs that attempt to alter behavior by transforming individuals' perceptions and values. In Malawi, the kinds of research questions that projects seek to answer are circumscribed: the National Research Council of Malawi (NRC) and the National AIDS Commission (NAC) mandate that all research

194 Researchers do, however, usually claim that they do research to direct interventions to places where they are desperately needed; they begin their research in places with the highest prevalence of HIV and end with recommendations for doing something based on the findings.

must be relevant to pressing national social problems or national development objectives. Malawian researchers, especially, are committed to contributing to the good of their nation. They often reiterated this investment: "We must not do research that simply excites us but research must impact on local populations."[195] Moreover, while international research projects maintain a degree of topical freedom enabled by their access to international funding sources, their "freely-chosen" interests must also be convincingly linked with the Malawi state's interests in order to get approval from the Malawi ethics board and to enlist the collaboration of Malawian researchers who wish to improve their nation;[196] this intertwines research activities with policy, as evidenced by the well-known phrase "policy-relevant research." However, the main venue for the presentation of research findings across these projects—what matters for the international researchers—is peer reviewed academic journals.

I argue that the production of authoritative knowledge by research projects necessitates that raw information in the form of survey responses or HIV-test results be detached from its local context and miniaturized—converted—into widely circulating data. This conversion, however, is delimited by and adheres to a set of standards shared by demographic social scientists working in Africa. The chapter takes shared epistemic virtues—ethicized expectations about data and how it is collected—as an entry point for exploring processes described ethnographically below. Epistemic virtues are internalized and enforced values that act as a sort of measuring stick for the data collected by researchers (Daston and Galison 2007:40). These virtues are: precision, accuracy,

195 Interview, Malawian demographer; December 15, 2007.
196 The linking of development and policy to research was evident in former president Kamuzu Banda's speech at the conferring of the first degrees earned at the University of Malawi: "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).

timeliness, sample size and sample purity, reduction of human error and clean data. Each of these not only guides fieldwork but also produces categories, identities and practices that reinforce and challenge these standardizing values.

In its focus on the ethnographic realities of making and circulating numbers, this chapter responds to Lampland and Star's (2009) suggestion that quantification has largely escaped attention as a sociocultural project in itself. Drawing on participant observation with case study projects, interviews with project staff and analysis of the tools and instruments utilized by these projects, I demonstrate how social realities are miniaturized into data, an easily transportable relic of realities. Amid the vagaries of fieldwork, standards govern this miniaturization in order to protect and reproduce four main epistemic virtues characteristic of "high quality data." First, I draw on participant observation of survey design meetings, translation sessions, and implementation of survey questions to show how accuracy and precision are maintained. Second, in order to ethnographically explore the virtue of timeliness or "timely" data, I highlight how the mandate to "keep time" in the field manifests in the gestures, comportment, habits, and interactions of fieldworkers and research subjects. Next, I show how the demand for large sample sizes and sample purity rely on particular orientations to research subjects and adoption of a "good fieldworker" identity by interviewers. Finally, I suggest that maps, photographs, data entry procedures and other technical objects and techniques reproduce the ideal object of "clean data" that is as much as possible free of errors and missing data points.

I conclude by arguing that the practices described in this chapter effectively manage the uncertainty inherent in knowledge about the epidemic. The numbers made by

research are provisional and uncertainty is enfolded into but does not threaten authoritative knowledge about the epidemic. The conclusion elaborates how researchers paradoxically acknowledge and manage uncertainty that underlies claims about AIDS and suggests that, in the face of critiques that they "overlook" complexity and context, they see exactly what they want to see.

Seeing Like a Researcher

After James Scott's well-known *Seeing like a State* (1998), this chapter considers *Seeing like a Researcher*. According to Scott, the state exerts effort to immobilize or "capture" its citizens long enough to count them, tax them, conscript them or prevent rebellion. Here, substituting "researcher" for "state" points to a contemporary Malawi traversed, ordered and enumerated by international research projects and institutions; like Scott's state, researchers "see" and render citizens' micropractices legible. Since the advent of democratization in 1994, large numbers of international research projects and NGOs have blanketed this small, landlocked country (Morfit 2011); many center their activities on the AIDS epidemic. Like the state,[197] research projects assemble a corpus of techniques of enumeration to make the society they are interested in legible, to arrange or organize the population in ways that simplify the functions of data collection, analysis and circulation of findings. Scott's central insight that state representations "[do] not successfully represent the actual activity of the society they depicted or were intended to [but rather]…only that slice of it that interested the official observer" (Scott 1998:2-3)

197 Cf. Ferguson 2005 for a critique of Scott.

resonates with the limited ability of researchers to see or know their research subjects, limits that they, for the most part, readily admit.

States and research projects enlist counting and enumerative practices to make populations visible. Population censuses, counting of households and quantification of acres of farmland have historically borne a clear relationship to the power of a sovereign or governing body to see or to preserve order within its bounded territory. Enumeration, in its emphasis on counting the units that comprise a *national* entity, then, depends simultaneously on the individuation of such units and their ultimate amalgamation into a population (Foucault 1978/2007:42). Bourdieu and colleagues view social scientific population knowledge as central to these "state forms of classification" and as "part and parcel of the work of the construction of the representation of the state which makes up part of the reality of the state itself" (1994:3). They moreover pinpoint the central role of *informational capital* collected through research or enumerative practices in permitting the state to accumulate economic capital via unified taxation (1994:7). Agnes Riedmann (1993), writing on fertility projects working in Nigeria, employs the term "bureaucratic surveillance" (citing Giddens 1987) to critique the long legacy of outsiders gathering information. Her account follows in the wake of similar critiques of, for example, the use of technologies of measurement, inspection and enumeration by colonial powers that sought to maintain and reproduce healthy bodies to engage in migrant labor (Packard 1989, Vaughan 1991, Mbembe 2004).

The case study projects in Malawi employed a wide spectrum of enumerative techniques to help them see and represent, e.g. units of a population ("X village is comprised of 20 households, 10 of whom have sent males abroad for migrant labor") or

units of human bodies infected with a virus ("35 percent of women in Y village are infected with the HIV virus"). These techniques included: surveys, household rosters, sampling, enumerating and/or mapping households, utilization of GPS technology to map rural areas and use of voice recorders to preserve qualitative interviews. Statistics and representations generated from these data collection methods circulate transnationally and play a role in universalizing and authorizing claims about the AIDS epidemic. In their claims to represent a larger reality, these numbers produced by data collected during fieldwork are tidily condensed packets of social reality. While statistics in eighteenth century Europe were often kept behind closed doors, in the nineteenth century, the circulation of statistics in printed form became standard. When spoken, read on a page or reported at a conference, then, statistics appear decontextualized from their origin, to "exist apart from the institutional contexts in which they were created…ready made for circulations in future texts and contexts" (Briggs and Mantini-Briggs 2003:266). Yet as Bowker and Star (1999) show, the decontextualized character of statistics is a convincing performance or an effect of their claim to represent and measure realities. I show that behind numbers lies a matrix of everyday social practices that allow researchers to see, to represent realities and to lend legitimacy to their eventual knowledge claims.

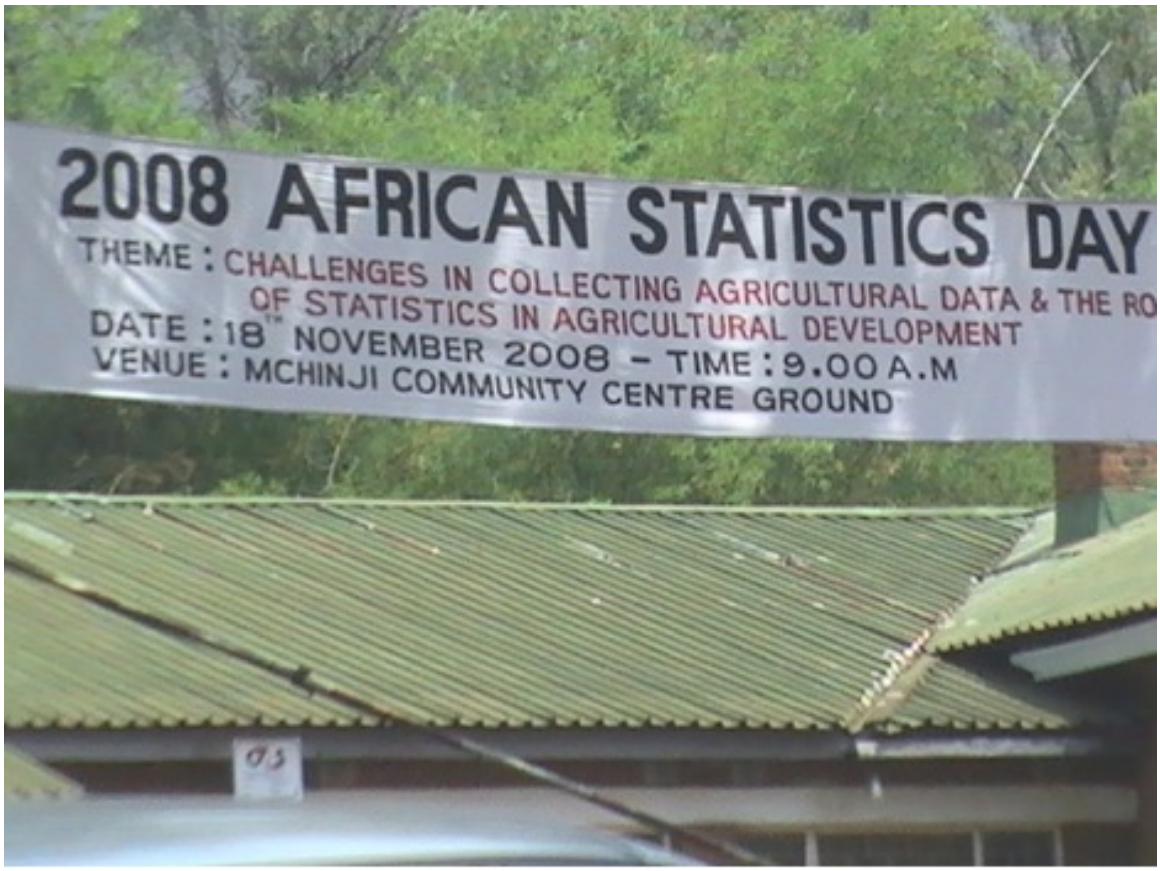

Figure 3.1: A banner for "African Statistics Day" hung in Zomba, Malawi (Photo: Author).

Data as Social Relic

International research cultures and other associated transnational formations such as global health programs are characterized by their fast pace, their need for data that are mobile and able to circulate widely and their privileging of efficient and sparse representations of African AIDS over ones characterized by "thick description" or ethnographic depth. As data must be shared, compared and enlisted into multiple and overlapping projects, policy makers and the researchers who provide their data employ techniques of enumeration to quarantine only that portion of reality they are interested in or are able to see. Researchers' ability to see their human subjects depends fundamentally

on the effective shrinking of a complex social world into a small snapshot or data point. This chapter is not concerned with the accuracy (or inaccuracy) of representations of rural reality; rather it focuses on the everyday relations and practices in which information is "miniaturized" into data. Crucial to this process is a simultaneous *magnification* or solidification of representations as "real." How does a random sample of individuals come to stand in for a larger rural Malawian social world?[198]

Such data, despite temporal and geographic estrangement from its original context, nonetheless possesses a certain power absorbed from this context—as a kind of social relic, it stands in for, through its very extraction from, a local site. Peter Brown shows how the circulation of highly portable relics of the saints[199] in Christian late antiquity collapsed the imagined distance between the "believer and the place where the holy could be found" (1981:88-89). Relics, as physical containers of sacred and potent power, are survivals, traces or vestiges of an increasingly distant reality,[200] yet even the tiniest relic captures and magnifies the *praesentia* of the saints. For Brown, its miniature form magnifies the relic's power. It is precisely their portability that assigns legitimacy to relics' distant and authentic origin: "Translations—the movement of relics to people—and not pilgrimages—the movement of people to relics—hold the center of the stage…" (1981:88). In international research cultures, I read data as an analog to the relics Brown describes.[201] In their very portability and in the traces they bear of a local reality

198 It is important to note here that there are many kinds of "international research culture," and that even those that do not rely on numbers as common currency must shrink or miniaturize reality. Consider researchers who utilize, for example, focus group discussions in three sub-Saharan African nations; these findings will necessarily rely on decontextualization as much as those described here.

199 These relics were various kinds of material objects associated with the saints that served to incorporate the sacred power of those holy people (especially their bodies). They included: corpses, parts of corpses, clothing, books and "contact-relics" such as vials of blood or water that had touched the saints' bodies.

200 The word relic comes from the Latin *reliquiae* meaning remnants, remains, or dregs of a dead person (OED).

201 I understand data, even at the moment of collection, as always already archaic. Data must be collected efficiently as researchers are urged to publish their findings and use their data as quickly as possible lest their findings become outdated. Latour urges us to take

inaccessible to their audience, data unify and construct expert communities as they move between interested persons. When a datum is untethered from its original context, it bears traces of a distant local context even as it joins the global circulation of "tiny fragments of original relics."

The portability of the local in enumerated forms from one site (the field) to many others (the office and beyond) serves also to capture and freeze a sliver of local social reality.[202] In her book *On Longing,* literary critic Susan Stewart engages the human yearning for true and unmediated experience amid objects (such as texts or artifacts) that remain stubbornly inauthentic because of their distance from an original or authentic context. In considering the relation of narrative to objects or experience, for example, she explores the museum as a site in which time is arrested and objects become "miniaturized" and less susceptible to any sort of contamination. Stewart points to the capacity of the miniature to "create an 'other,' transcendent time which negates change and the flux of lived reality" (1993:65). Like Stewart's museum, the field office permits data numerous degrees of removal from everyday life.

The data analyzed and interpreted in the office becomes a miniature form or a "still shot" of rural Malawian social reality.[203] As Stewart suggests, the usability of this miniature form depends on its fundamental disconnect or difference from the reality of those who "see" it or analyze it. That is, "the miniature does not attach itself to lived historical time" (1993:65). Miniaturization is the process by which "the field" (reality)

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interest not in the "seemingly miraculous…" internal thought processes of scientists but rather in the mobilization of scientific objects (such as "tallies, totals, graphs…") previously viewed as "the object(s) of a cult" (1987:237).

202 Fabian (1983) shows how the writing practices and conventions of both travel writers and anthropologists deny the co-temporality of the subjects being represented. The demographic social scientists discussed here, too, often circulate their findings in the "ethnographic present" (i.e. "This paper assesses the current impact of HIV/AIDS on the Malawian workforce…").

203 This miniature form (i.e.: knowledge claims about or representations of Malawian social reality) will travel far and wide; however, its audiences or observers remain unable to obtain any more than a solidified view of an increasingly distant rural reality: "The observer is offered a transcendent and simultaneous view of the miniature, yet is trapped outside the possibility of a lived reality of the miniature" (Stewart 1993:66).

becomes readable, manageable, and usable. The very anachronism of the stable, bounded and, now, quarantined snapshots of reality with the office gives them and the knowledge they produce authority. In their stability, these snapshots become "immutable mobiles" (Latour 1987).[204] However, the immutability of represented reality in the office and beyond depends on the mutable nature of the actors and objects involved in collecting data before it reaches the office (Schumaker 2001, Raj 2007). Many people and things must collaborate and network to first produce, and then legitimate, the miniature and condensed version of reality. "Much work has to be done" to maintain a knowledge claim (Latour 1987:254). I argue that knowledge claims are produced and maintained via dual processes of miniaturization and magnification that adhere to epistemic virtues.

Ensuring Accuracy and Precision

While the next chapter will show how data serve as the foundation for evidence as expressed in knowledge claims or statistics, this chapter is concerned with the ways in which the procedures and processes of data *collection* anticipate the criteria that will assess that evidence. In this way, the epistemic virtues mentioned above serve to mediate, guide and inform the everyday processes through which data is collected. First, data must be accurate and precise if it is to be enlisted as evidence for knowledge claims. Accuracy dictates that data must be as true a representation of reality, of an individual or a phenomenon as possible. Precision mandates that data and findings resulting from it must be replicable, obtainable in the same form again and again. In order to collect data in line

204 Latour (1987) uses this term to refer to the easy mobility of knowledge/objects through techno-scientific networks and to these objects' ability to retain key features even as they travel. Data is both mobile (it shifts planes between the local, national and global) and immutable (it retains a sacred power as locally collected from a distant field and it is assumed to have been collected with "care" and "quality control" in mind.) The immutability is maintained and reproduced as an effect of the work of what Latour calls inscriptions (e.g. journal articles, conference papers, presentations, grant proposals, databases) that make up the core of scientific knowledge work.

with these criteria, research projects must ensure that fieldwork teams collect accurate information in standardized manners that will be replicable in the future—over longitudinal time. While sampling strategies are also influential determinants of accuracy and precision, this section centers on ethnographic analysis of the ways in which survey questions are designed and how it is that questions are "translated" to ensure that they elicit the most accurate responses.

Designing the Survey

As surveys are, in effect, a bridge between social reality and data, survey design significantly impacts the data collected. In order to collect accurate codes or numbers—and a more "real" reality—survey questions must be effectively translated, ordered and designed. On a muggy night in January 2008 in Zomba, Malawi, I sat around a small table with three researchers from the University of Malawi and four American researchers;[205] they were reviewing the draft of a lengthy survey about adolescent sexual behavior in central Malawi. This was the collaborative work of research: honing in on the best ways to ask questions and the optimal translations of concepts from English into Chichewa. Two days later, this survey would be piloted with a small sample in order to test whether the translations were adequate. We were there late into the night trying to predict potential difficulties that each question would be met with in the field.

The researchers spent the evening perfecting translation of concepts and words from English into Chichewa; if a question's translation collects the wrong information, the accuracy of the data is measurably reduced. At the meeting, the Malawians present

205 Field notes; Janurary 19, 2008.

focused on one series of questions that asked respondents to "locate" themselves on imaginary steps that ranged from "1-poorest" (in Chichewa, *anthu ovutikitsitsa,* people who have troubles) to "6-richest" (in Chichewa, *anthu ochita bwino*, people who are doing well) amid their community.[206] A Malawian sociologist suggested that respondents could interpret the term "community"[207] in a myriad of ways; he argued for a narrower, closed specification such as "village."[208] Other concerns about this question centered on whether respondents were to consider wealth in terms of their own family or in terms of the family they married into. The Malawians present agreed with an American researcher that family and individual could effectively be mapped onto one another—challenging the ability of the question's collected responses to represent or accurately capture reality and illustrating a gap between data and what it is meant to signify. The problem of how to translate the terms "house,"[209] "household"[210] and "home"[211] also arose. In one case, respondents were to be asked a question about where they built a home after marriage. Would the respondent mention the location of his matrilocal home? A matrilineal home? A household where he was living because he could not afford to construct a home at the moment? Each of these unknowns had the potential to erode the accuracy of the response to this kind of question. In this way, the problem of translation was resolved only when research survey design teams came to a consensus about how they assumed respondents

206 The Chichewa text read: *Muganizile ma step kuyambira mpakana sikisi; pa wani ndi pamene pali anthu ovutikitsitsa m'mdera lanu lino, pa sikisi ndi pamene pali anthu ochita bwino m'mdela* [sic] *lino.* In English text: Consider the six separate upward rising steps; on the bottom one there are people who have problems in this area, on the sixth are people who are doing well in this area.
207 The word under discussion was *mdera*, which translates loosely and depending on context to area, locality, region.
208 The Chichewa word for village is *mudzi*, denoting very specifically the respondent's immediate environs, or the area administered by a local chief.
209 The Chichewa term for house (with the connotation of a structural shelter and 'home') is *nyumba*. There are many ways to refer to a household in Chichewa; most of them are descriptive. For example, one might say *anthu a banja limodzi*, meaning the people in one family. I also heard people simply refer to household as *banja* (family).
210 The complexities of the universal circulation of the term "household" in international global health and other research has been noted by social scientists. In a paper presented at the 2007 meetings of the Union of African Population Scientists (UAPS) in Arusha, Tanzania, population studies researcher Ernestina Coast (London School of Economics) systematically identified the differences between "household" as defined on surveys and locally meaningful concepts of the household (cf. Guyer 1981).
211 Home was typically referred to as a joined locative-possessive (as in *Wapita kawawo*, He has gone to *his home*).

would hear the question or about what was gained or lost by using one versus another word for slippery concepts.

The necessity of collecting accurate and precise data also influenced survey design by drawing attention to how possible responses to questions should be coded. The researchers discussed at length the merits of open or closed survey questions, and usually by the time fieldwork began, any open-ended questions had been converted into closed questions and the possibility of responding in an unscripted way was relegated to the category "Other: _____." (Interviewers were encouraged to "minimize" their use of this category if possible to "make data entry easier").[212] For example, the final version of one survey provided twenty-five intricate codes for a response to a question about what the respondent did to deal with an economic shock experienced in the past year. These codes ranged from "1: Spent cash savings" to "12: Went elsewhere to find work" to "21: Reduced non-food expenditures;" the interviewer, then, was faced with the task of classifying his/her respondent's spontaneous response beneath the most appropriate number. Often, this involved listening to a respondent's long-winded description of his/her experience of a certain financial crisis and then ferreting out the information important for numerical classification. Though trainings included "situational practice" (where interviewers practiced classifying fuzzy information accurately beneath the available categories), the space between the survey questions and the dynamic conversation that happened in reality necessitated negotiations of the questions and answers in real time. The project built up defenses against inaccuracy by encouraging interviewers to *probe*: to get respondents to "classify" their own responses without the

212 Field notes, Fieldwork trainings; January 2008.

aid of the interviewer. However, standards of data collection put in place to ensure accurate data did not always fit seamlessly into the interview encounter.

In many cases, respondents would remain quiet or simply laugh in the face of a difficult question. During one interview I observed,[213] a female interviewer asked a female respondent one of the scripted questions: "What do you expect your children to provide for you when you are older/elderly?" The respondent chuckled and looked away, quiet. The interviewer probed, pushing her to say what she meant—to elicit an accurate answer. The respondent refused, but her answer was obvious from her comportment and laughter: She expected her children to provide her with everything; they are her children! Flustered, the young interviewer checked off "shelter," the most capacious of the available responses, notifying the respondent of her decision. These scripts of closed questions and aversion to the "other" category can serve to reproduce again and again a distance between reality (here: the true, accurate or spontaneous response of a villager to a survey question) and data (the response recorded on to the survey in an open- or closed question).[214] While the ways in which sampled individuals arrived at decisions differed, the complexities informing a coded response remained unseen by researchers once decisions were miniaturized into numbers. However, to conclude that researchers did not care or need to know the details is overly simplistic; though researchers themselves often pushed for more open-ended questions to be coded after data entry, in fact, Malawian supervisors often pushed for closed questions, usually to simplify the work for data entry clerks and interviewers. This speaks to the messy entanglement of the interests of a complicated group of actors; importantly, it illustrates that the outcome of the frictions

213 Field notes; June 12, 2008.
214 Marian May (2008) elaborates on the negotiation inherent in telephone-survey questions on birth timing; she points to the inevitable unintentional censoring of information or stories by interviewers in the interest of obtaining a response.

between these interests will not always manifest directly from the desires of the most empowered actor. Here we see that the practical and everyday concerns of research *fieldworkers* trumped the researchers' own interest in preserving open-ended categories on a survey. Accuracy and precision both rely on the fact that questions can, first, collect a single, clean numerical or coded response and, second, be asked in the same way again and again.

"Translating" the Survey

In addition to dealing with the challenges that linguistic translation posed, the issue of cultural translation also preoccupied research teams. If a respondent does not truly understand a given question or what it seeks to capture, his or her response is not valid and becomes "bad" data. In some cases, measures put in place to improve data collection by making methods more locally relevant had the unintended effect of further stretching the distance between data and the reality it signifies. Many of the expatriate (and also Malawian) researchers were preoccupied with ensuring that local people could understand the questions asked of them on a survey. Often, projects attempted to increase local understanding by utilizing exercises to translate complicated concepts like probability into simplified forms. Meghan Vaughan describes how filmmakers who produced colonial health education films in Nyasaland and Northern Rhodesia[215] relied on the imagined creation of an audience for their viewers (1991:196); they presumed that African viewers received images differently, saw differently and were liable to be more easily confused. Similar kinds of assumptions about audience were evident in one

215 Nyasaland is now Malawi; Northern Rhodesia is now Zambia.

project's implementation of what became known as *nyembanyemba* (the beans) among research staff and research participants. *Nyembanyemba* aimed to make the complicated concept of probability accessible and understandable to rural Malawians;[216] respondents were asked to place a certain number of beans in a dish to estimate how likely it was that they would, e.g., go to the market in the next two weeks, experience food shortage or contract HIV/AIDS (one bean if it was unlikely to happen, 10 beans if it was certain to happen, see Figure 3.2). In another similar exercise, the HIV voluntary testing and counseling (VCT) teams used a pictorial thermometer to ask rural respondents to indicate the "temperature" of their quality of life. Although these numerically-grounded elicitation methods had been tested and verified in academic journals and at international conferences, the villagers' responses to the exercise were, on the whole, negative. One woman summed up the generalized discomfort with the beans exercise: "If you want to play, go over there with the children!"[217] Some research participants viewed this exercise as infantilizing, despite the intentions of the researchers who designed the surveys. The implementation of these tools shrinks reality not only by making respondents feel "small" but by predicting the contours of audience or narrowing the lens ahead of time. How does this tool meant to increase the accuracy of data work in the field?

216 In developing world settings, especially, it is felt that simply asking respondents for a probability or percent chance is too abstract, and that visual aids are needed to help them express probabilistic concepts. This commonly involves asking respondents to allocate stones, balls, beans or sticks into a number of bins. See Delavande *et al* 2010 for a critical review of methods for measuring subjective expectations in developing countries.
217 Field notes; May 18, 2008.

Vernacular Probabilities: The Beans (*Nyembanyemba*)

Over the course of a few weeks during fieldwork, the beans were an important topic of negotiation between the different levels of the research project. While the exercise had been validated as a measure for survey research in developing countries, the researchers implementing it as part of this survey had some misgivings, explaining, for example, that they thought it might elicit inaccurate responses by "seeding" a numerical response when introducing the exercise: "You can say: 'For example, if I think I might go to the market this week, but I also might not, you could put five beans in the dish.' The respondent might proceed to provide that number more often during the course of the exercise."[218] The supervisors, in their role as employees to the project and managers of the interviewers who were implementing the exercise, had to carefully negotiate a small space between the researchers' mandating of the exercise, their own views about the beans and the incessant complaints of the fieldworkers. On a daily basis, fieldworkers complained that the beans were a waste of time, that respondents grew bored, that respondents did not understand *nyembanyemba* and that they themselves thought it was silly. Further, fieldworkers often lost or adapted the equipment needed to conduct this portion of the survey, dropping one or two of the ten beans required or using the plastic dish provided to them as a receptacle for a lunch of greasy chips. In general, supervisors told their charges to stop complaining and encouraged them to "improve your attitudes- the bad morale among your villagers is coming from you! They can tell you think *nyembanyemba* is *chabe* [worthless] and this allows them to protest. Go be excited about those beans!"[219] However, at the nightly meetings with the American researchers, the

218 Field notes; June 3, 2008.
219 Field notes; July 29, 2008.

supervisors spoke up for their charges. Most frequently, they suggested to the Americans that the beans exercise was a misfit with "Malawian culture"—that it was difficult for Malawians to understand.[220] Statements such as this one point to the irony of a "culturally relevant" tool for measuring probability being classified as "outside" or irrelevant from a vantage point within a culture.[221]

X3	**Tsopano tiganizire za mayi wathanzi wa m'mudzi mwanu yemwe alibe kachilombo koyambitsa EDZI. Tengani nyemba zomwe ziyimire m'mene mukuganizira kuti maiyu atenga kachilombo koyambitsa matenda a EDZI.** *Consider a healthy woman in your village who currently does not have HIV. Pick the number of beans that reflects how likely you think it is that she will become infected with HIV …*	**# of beans in plate**
a)	**Atagonana kamodzi ndi munthu yemwe ali ndi kachilombo koyambitsa EDZI** *during a single intercourse without a condom with someone who has HIV/AIDS*	[_____]
b)	**M'miyezi khumi ndi iwiri(12) ikubwerayi akakhala wa khalidwe la bwino** *Within the next 12 months (with normal sexual behavior)*	[_____]
c)	**Ngati akwatirane ndi munthu yemwe ali ndi kachilombo koyambitsa EDZI m'miyezi khumi ndi iwiri(12)ikubwerayi** *within the next 12 months if she is married to someone who is infected with HIV/AIDS*	[_____]
d)	**M'miyezi khumi ndi iwiri(12) ikubwerayi ngati pali anthu ena omwe amagonana nawo kuphatikizirapo akunyumba kwawo** *within the next 12 months if she has several sexual partners in addition to her spouse*	[_____]
e)	**Nanga atakhala kuti amagwiritsa ntchito kondomu pogonana ndi amuna enawo, mungayike nyemba zingati?** *What about if this woman we just spoke about [in X3d] uses a condom with all extra-marital partners? How many beans would you leave on the plate?*	[_____]

Figure 3.2: Sample questions from the "Beans" (expectations) section of the 2008 survey implemented by a case study project.

I present some insights from field notes written about interviews where *nyembanyemba* was implemented in order to highlight the everyday sorts of negotiations that take place. Tapika,[222] a 24-year-old woman was interviewing a 35-year-old man in a

220 Field notes; May-September 2008.
221 One possible motivation behind statements about "culture" such as this that supervisors made was to do away with the beans exercise to make data collection easier and more streamlined.
222 Pseudonym.

village in central Malawi.[223] The man was relatively wealthy for the area, as was evidenced by his tobacco balers nearby. Before she started, he proudly showed her these balers, explained that they had cost 100000 *kwacha* (about $700), and that he charges people in the village 150 *kwacha* (about $1 at the time) per bale to bale their tobacco. She sat behind his house on a mat he had set out on the ground and the interview proceeded smoothly until she reached the beans exercise. Though he was first reluctant to engage in this section of the survey ("I really should do this? [move the beans around] Can't I just answer the questions?"), he quickly became a willing participant. After Tapika provided him with instructions, he eagerly proclaimed: "So, if you ask me how likely it is that it will rain today, I should say, maybe 2 beans, because look [pointing to the clear sky], it's just not!" I read this energetic enthusiasm as a product of my presence and as emergent from this man's clear valuation of education and status; the statement was a performance of his knowledge of probability and his ability to clearly and quickly grasp the instructions. As compared to other interviewees I observed, this man appeared to consider each question thoughtfully and did not rush to answer using the beans. Halfway through the long section, however, he grew tired of the beans and started to respond by mentioning numbers without manipulating the beans and the dish in front of him. At this point, Tapika grew frustrated and proceeded to physically pick up the number of beans he said each time and place them into the dish—as if to say: Look, you have to continue to do this. Her counterpart grew increasingly annoyed at her gestures, in one instance swiping her hands away from in front of his place. The defeated Tapika finished the section without requiring the respondent to physically engage with the beans.

223 Field notes; June 14, 2008.

This vignette illustrates how the interview encounter is a site of negotiation. First, the respondent made known his own "reasonableness" by making an initial effort to follow instructions and go along with something he initially found unattractive. His later disinterest in the exercise marked his effort to disengage from a dynamic where the interviewer asserted her status by requiring him to "play" with the beans. Tapika, as a young woman interviewing an older "village" man, negotiated this relationship carefully and, I believe, also felt compelled to perform the "scripts" of the beans that she had learned in training. My presence at the interview likely intensified her interest in faithfully staging the beans section of the survey; in other interviews where interviewers did not know they were being observed, they had no problem with respondents who refused to touch the beans or, sometimes, did not even remove the dish and beans from their canvas bag. In this case, Tapika felt obliged to be identified as a "good fieldworker" trying to convince a "difficult research subject" to participate in the beans exercise. After we finished, we walked to a teahouse together where Tapika seemed anxious that I had judged her to be a less than competent fieldworker.

Nyembanyemba is a portion of a survey that simultaneously promises to increase accuracy, but threatens to reduce precision. As an internationally validated tool for collecting information about probabilities from developing world respondents, its translational function promises to make the abstract concept lucid and accessible to respondents who must understand it in order to answer questions accurately. Further, it would seem that exercises such as this one permit respondents to externalize their thoughts[224] and, thus, to allow research assistants to determine whether or not the

224 The manipulation of beans can be read as a physical trace of inner thought processes.

respondent is hearing the question correctly. However, as this section has shown, the bean exercise and its associated tactile aspects open a space for the precision of data collection to decrease. That is, across each interview, the interactions and negotiations between parties can work to alter or inform the responses provided by respondents. Whereas monitoring the handwriting or pointing out empty spaces on a survey page is easy, determining whether each interview implements the beans exercise in the same way every time or gauging the divergent levels of fatigue and frustration likely to be manifested across respondents is close to impossible. As one researcher suggested, it is probable that the manner in which the exercise is presented and explained may "seed" answers in the minds of interviewees. Regardless, as a culturally relevant, internationally accepted translational tool, the beans convincingly perform and index enhanced accuracy and precision in data collection.

Timeliness and Timely Data

International AIDS research projects and the larger global health system rely on rapid circulation of data and representations so that actors may effectively intervene into a pressing health crisis. Time is of the essence. However, as has already been illustrated in other chapters, the imperative of the research project to "keep time" often comes into friction with other interlocking temporalities: village time, weather delays, the erratic and uneven temporal projects of fieldworkers themselves and even the time and schedules of other projects working in the same village(s). Nonetheless, the imperative to collect not only high quality, but also timely data serves to standardize everyday fieldwork practices.

This section illustrates how these standards translate into the fieldwork context, and also how interviewers negotiate these standards as they stand at temporal crossroads.

"Keeping Time"

Extensive fieldwork trainings mold fieldworkers into reliable and efficient data collectors and work to exclude potentially unreliable fieldworkers from the field. In most cases, those most unsuited for fieldwork were immediately dismissed when they turned up for work late in the mornings or were caught drinking on the job. Seemingly insignificant missteps, however, were more difficult to exclude. Specifically, the projects' emphases on data that is simultaneously "timely" and "reliable" sometimes placed fieldworkers in difficult positions. The precarious financial position of interviewers and supervisors hired by research projects is elaborated in Chapter One; across all the projects I spent time with, employees were simultaneously thrilled to have a job and terribly afraid of losing it.

I observed a disconnection between village time and project time across all the projects I followed; this bifurcation of field and office time solidified an imagined distance between social reality and its representation. It instilled anxiety in the interviewers about job security. For example, on the fifth day of training for one project, the interviewers had just been handed back their first assignments—filled in surveys they had piloted the day before in a nearby village—covered in supervisors' red pen marks. Most had made many mistakes and were feeling frustrated, wondering aloud how to effectively probe for good information, keep a chat going and still keep time. Since part of being a good interviewer meant doing more than one interview in a day, many of the

new interviewers were concerned about how to find a balance between diligence and the time being kept by the project. After sitting through a lecture earlier that morning about the importance of keeping time in the field, many were genuinely worried they were at risk of losing their jobs because they took their time filling in the survey and engaging respondents. One outspoken man expressed a widespread complaint when he said to his supervisor, "I'm a good interviewer and I know it. I get your information. But I am not as speedy as others…this means I might lose my job."[225] According to the fieldworkers, then, keeping office time increased the probability of human error and the likelihood of subpar data.

Once information is detached from its origin, it is transported to the office where data entry co-occurs: here, it is extracted from the questionnaire and reformatted into data. This miniaturization relies on the presumption that the office is a place where *data* are made and manipulated. Following Latour, the office serves as a center of calculation--a place where disparate, foreign and variegated things are brought together to be analyzed. The office is removed from its physical setting through everyday linguistic and performative place-making practices. Anthropologists Akhil Gupta and James Ferguson (1992, 1997) define place-making as the ways in which culture, power and place intertwine with one another to govern how terms such as "local" or "global" come into being and are assumed by categories of person. In the context of international research fieldwork, the field and the office become signifiers that are taken up and absorbed into subjectivities, social relations and everyday practice. The way these terms are deployed

225 Field notes; May 16, 2008. Though hiring and firing practices differed across projects, these words rang true. The previous day, a few interviewers had been let go because they were slower than other interviewers. Supervisors were reluctant to hire interviewers who were "too dull" to catch on quickly enough, walked too slowly, complained too much about the long fieldwork days, had bad penmanship, talked too much or, in some cases, women who had babies and might get tired of walking with extra weight.

across contexts influences not only how social actors imagine these places, but also how they interact within and outside them.

Principal investigators and Malawian collaborators linguistically quarantined the fieldwork office from the field. This office (and offices that are further afield) is a place from which researchers can objectively observe, analyze and manage rural realities that are imagined beneath a wide umbrella term: "the field"—an undifferentiated place both distant from and incommensurable with their own positionality. In these conversations, the field was a placeholder for unfamiliarity, roughing it and geographic difference. Researchers often discussed the challenges of preparing to travel to a place (even for a few days) that was foreign and mostly unknown to them. By stepping into the field office, however, the vagaries and unknowns inherent in fieldwork were neutralized. A researcher at the World Bank in Africa described how she wished she could spend more time "in the field," but how overall she preferred "sitting in the office in the US and crunching numbers and having those numbers and categories be anonymous, not personified."[226] Between the field and the office(s), enumeration transforms personalized information into anonymous numbers; this process depends fundamentally on the temporal and spatial autonomy of the office from the field. Whereas the temporalities of the field were unpredictable and chaotic, the temporalities of the data entry office were marked by rituals (tea breaks and scheduled meals), expectations of measurable productivity (number of surveys entered today) and the sustained sound of tapped computer keys "marking time."

226 Interview; September 19, 2007.

The friction between the field and the office is illustrated by a set of responses to interviews and a formal survey with supervisors and interviewers about their experiences working "in the field" (see Chapter One). When I asked them to describe the best thing(s) about their jobs as supervisors and interviewers, they invoked the fundamental distance or difference from the local or the village in their responses, even though some of them were actually from these villages. They said things like: experiencing new and different cultures of people, meeting people from very different backgrounds than my own, learning about local culture and how they live in a village and experiencing a different culture, like learning to live how the villagers do. Each of this sampling of responses indicates the *newness* and *difference* that the field context stood for among the fieldworkers. Interestingly, the data entry team members on the same projects did not express these sentiments; as they were not "out in the field" all day but, instead, confined to the office, they expressed boredom or fatigue. These juxtaposed affects of excitement and boredom mirror the difference between a paper survey covered in dust and the sanitized space of a database. As will be discussed later in this chapter, data is "cleaned" in the office.

Although time is of the essence, the complex overlapping of the temporal aspects of fieldwork intersect in unpredictable ways, often necessitating negotiation between different kinds of time. However, the starkest distinction is between the fast-paced time of the office and the slow-time of the field. These temporalities persistently entangle, but the very mobility of data between them necessitates that they also are autonomous from one another. This autonomy is upheld by place-making practices centered on dress,

speech and comportment as described in Chapter One and by the well circulated and widely adhered to standards of collection of timely data.

Shelling Maize, Drawing Lines and "Chatting"

One specific interview encounter illustrates the interlocked temporalities of a sociocultural context. Here, I wish to show how an interview encounter is a site of multiple interests that are negotiated as an interview flows forward. In July 2008, I accompanied Janet,[227] a 26-year-old female interviewer to her meeting with a 39-year-old woman called Namoyo.[228] When we arrived, Namoyo and her mother were shelling maize. Before "getting down to business," the four of us sat quietly together, each working at the maize. At first the potential interviewee was put off by the prospect of a long interview, but as the conversation progressed, she grew more open to the idea. When Janet mentioned that she was from a nearby village, the woman grew excited: "How nice! I'm so glad you're working for them [the research project]; Usually the people who come to chat with us are from Lilongwe and Blantyre [the capital and commercial capital, respectively]." Maintaining our place on the verandah and continuing to shell maize as a group, we began the questionnaire. Every so often, children, goats and chickens darted across a walking path nearby, briefly disrupting the flow of the survey questions.

From the beginning, Janet introduced the survey as an informal "chat:" "Naphiri [the author's Chewa name] and I are just here to have a chat with you! We will just chat… let's chat!" In both English (chat) and Chichewa (*cheza*), "to chat" implies to

227 Pseudonym.
228 *Ibid.*

speak in an informal, non-linear, undirected and non-temporally bounded manner[229]—to "free form" a conversation. But as soon as Janet brought out the survey and her pen, it became evident that this chat would follow the order of the questions written on the survey pages. The first portion of the "chat" comprised Namoyo verbally "filling out" the household roster that was the first page of the survey. On the page was a table with fifteen columns and ten rows. After asking Namoyo to list each of the members of her family that live in her household, Janet wrote the names one by one into the blank rows. Once all the names were recorded onto the sheet, she asked a series of questions about each household member. "How old is X? What is X's relationship to you? Is X's mother alive? In what year did X move here? What is the highest level of schooling X went to? Is X married? Is X ill?" Many of the answers provided by Namoyo had to be 'coded' by Janet with a relevant number (e.g.: for level of schooling, Standard 1: 0 Standard 2: 1 Standard 3: 3). This involved Janet pausing while she leafed through an accessory packet of survey codes in order to locate the proper code to be supplied.

A month earlier, the interviewers had been told to maintain good penmanship, to write neatly. As Namoyo delivered her responses to the survey questions, Janet took care to record the responses neatly; she even used a ruler as a straight line beneath the letters she wrote. Her efforts to adhere to the rules governing interviewing meant that the time it took Janet to record information was significant. Though we were all happily sitting in the sun shelling maize, the chat was marked by long periods of silence and awkwardness as Janet monitored her own penmanship and ensured she was seen as a "good

229 Of course, "chats" are always formal and structured in some manner. In Bantu languages, especially, the initial greetings portion of a chat is very structured and culturally scripted. However, the Chichewa verb "to chat" implies a casual conversation with little to no concern for the passing of time.

interviewer," not only by me but by the researchers and data entry clerks who would enter the data later. She also used the ruler to "strike out" deceased household members with a straight black line. Despite the "roadmap" provided from the survey from beginning to end, the chats were certainly not linear—in this instance, Namoyo could not recall the names of her parents in law when initially asked by Janet; later, during another section of the survey, she suddenly recalled them, interrupting the seamless and linear flow of the interview session and prompting Grace to flip back a few pages to enter the information. Like the rhythmic shelling of maize, the survey's chronology served as a mere backdrop against which the interaction meandered.

The interview encounter was a negotiated space of flows and stoppages of information, a social field in and of itself. As in many cases, the interview between Janet and Namoyo was marked by the interlocutors' mutual testing the waters. Early on, Namoyo commonly responded to questions with an "I don't know" or other "simple" answers. When Janet asked her to mention the amount of money she loaned to others in the past year, she claimed: "none." Janet looked at her dubiously, laughed, and asked "Not even five *kwacha* [about $0.04]?" The woman laughed, and then agreed that she had, indeed, loaned friends, neighbors and family members money in the past year. Later, Janet had to return to this box on the survey again when it turned out that Namoyo could remember the amounts she donated to individuals she listed by name. Similarly, Namoyo claimed she could not remember the ages of her own children.[230] When Janet pressed her, she could. Finally, over the course of a series of questions that covered "wealth indices,"

230 In these cases, we might view Namoyo's responses as lies. Bleek, in considering his experience with lying informants in Ghana, views the lies research informants tell (following Salamone 1977) as a meaningful form of communication and not its negation (1987:314).

Namoyo grew frustrated and visibly annoyed at having to verbally provide responses to questions that she felt were exceedingly self-evident to Janet. As a good interviewer who had been taught never to miss a question, Janet meticulously spoke each question: Does your household own: a TV? Solar panels? Does your household have a metal roof? Namoyo laughed in the face of questions such as these: Janet could easily see that she possessed none of these items—she was poor! Yet, when Namoyo laughed, Janet still pressed her to verbalize her actual response: "No."

Like Namoyo, many respondents adopted an ambivalent stance toward the interview encounter. Often, this aligned with the interviewers' own ambivalence. Janet's affect in responding to Namoyo's sighs of frustration showed that these questions were not her own; she was the mouthpiece for the project. Namoyo, picking up on her disinterest in this matter, made repeated stabs at taking control of the interview encounter by being selective about which questions she wished to answer, by providing inconclusive or vague responses or by feigning non-knowledge before finding an answer. These efforts each tested the contours of the interview as a social space: How invested was Janet in securing answers to each of the questions? How much could Namoyo reveal? What sorts of information should she divulge? Was Janet able to detect when Namoyo provided "bad" information? Namoyo relished the chance to talk to Janet and myself; as outsiders, we were an invaluable source of information. Namoyo asked us how things were in other districts we had traveled to, whether we had any children and so on. The linear form of the survey was disrupted and made circuitous when it was inserted into the social relations and space of the interview encounter. The standards and guidelines that interviewers learned in training sessions became embodied and localized

to the interview context. The imperative to write neatly and to be meticulous appeared in the field as awkward silences, with goats bleating in the background and informal conversation filling the gaps. The imperative to "ask every question" became the site of a negotiation between interviewer and interviewee as each tried to gain a foothold to express and secure her interests. The command to leave no blanks prompted push and pull exchanges between Janet and Namoyo, with the former probing and prodding for information that the latter was recalcitrant about providing.

Sample Size and Sample Purity

Early in the chapter, I suggested that numbers are the primary way in which we learn about the AIDS epidemic in sub-Saharan Africa. As statistics and numbers perform authority as they circulate across world stages, however, the processes involved in their production—that make them—often go unnoticed. The next section considers how numbers are made by focusing on how the epistemic virtues of sample size and sample purity structure and inform the everyday processes involved in sampling. How do field teams ensure that as many respondents in the sample as possible are reached and "counted?" How do projects see to it that they interview the correct respondents and defend the sample against "invaders"?

Sampling as "Seeing"

To be captured and to circulate, realities of interest must be decontextualized or isolated. Sampling is one enumerative technology that effectively and efficiently decontextualizes data from its immediate context. It is invariably done in the office,

usually at the time that the research proposal is written and well before any fieldwork occurs (and sometimes before expatriate members of the research team have set foot in the country). Samples are efficient: it takes far less time and money to interview 5,000 Malawians than 15 million Malawians. In order for the few to represent the many, the sample must be carefully selected (typically: randomly), and close to everyone in the sample must be interviewed. Only then will it be considered authoritative in the social science research community, only then can the results of the analysis of data collected from the sample be deemed representative of the larger population from which it was selected. Sampling strategies for my case study projects ranged from random sampling to snowball sampling to other, less scientific, forms of selecting respondents, but all strategies had a relationship to enumeration via the marker of authoritative social science knowledge called "a sample." A sample is meant to represent a larger population: only if a sample is deemed representative can the results of analyzing the sample be generalized to that larger population. In effect, a sample is a collection of persons that is cordoned off from the larger population for the purposes of management. Though sampling strategies differ, the social science projects I studied tended to draw their samples from populations smaller than the nation but larger than a village: usually they were administrative areas or geographic blocks of space identified as enumeration areas (EAs) by the National Census.[231]

An Ex-Sample

231 If projects sample Census-identified enumeration areas (EAs), this is useful for future enlistment of data into national development efforts. Thus, even though research samples are rarely representative of the national population, the sampling characteristics of rural populations often align with those of national surveys.

In order to illustrate the process and importance of sampling strategies for the production of knowledge, I provide a brief characterization of the data collection activities and intentions of one of the four case study projects. This project has been working in Malawi since 1998, making a longitudinally "pure" and "clean" sample of the utmost importance. After all, the data provide a "rare record" of more than a decade of demographic, socioeconomic and health conditions in Malawi and are often referred to and utilized by national government members and policy makers. However, this data is also the object of analysis for researchers themselves, who seek to answer a wide set of questions centered on the influence of social networks on the spread and mitigation of the HIV virus. I was present for two waves of data collection by this case study project: in 2005, and, most recently, in 2008 (for the fifth "wave" of data collection). In addition to the interview/survey and HIV test standard for the project since its initiation, this fifth wave incorporated some new aspects of data collection: weight and height measurements for all respondents and children five years or younger; an 800-person sample of parents of past respondents was added to facilitate examination of intergenerational relationship; blood samples were collected for a sub-sample of 1000 respondents in Balaka District in order to measure levels of various biomarkers, such as: high density lipoproteins (HDL), low density lipoproteins (LDL) and hemoglobin (HbA1c). A tabular summary of the expansions to the sample since 1998 is presented here:

Wave/Year	1/1998	2/2001	3/2004	4/2006	5/2008
Sample	Ever-married women age 15-49 and husbands	Same as prior PLUS new spouses of men/women who remarried btwn. 1998-2001	Same as two prior PLUS 1500 adolescents, aged 15-24 (ever- and never-married)	All respondents from prior waves PLUS spouses of 2004 adolescents and new spouses	All prior PLUS 800 parents of respondents drawn from family listings of 2006 respondents.
Numbers	2602	2548	3298	3669	4052

Figure 3.3: Sampling strategy of a case study project, 1998-2008.

It should be clear from the accumulative additions above that the researchers leading this longitudinal study "see" far into the future. They select and isolate their samples based on the long-term questions they anticipate answering. The sample strategies, too, are co-produced and try to capture real life changes in real time. For example, the accumulative and ongoing addition of spouses and new spouses to the table aligns with increasing divorce rates in Malawi.[232] Sampling, though, must be done carefully in order for longitudinal study and long-term trends to be accessible to researchers. Specifically, each of the thousands of individuals who comprise the sample must be successfully "followed" from one year to the next. This effort is compromised not only by the participants' resistive tactics described in Chapter Two, but also by attrition caused by factors such as migration or mortality. In Salima town, one case study project found that during fieldwork in February, many respondents were missing. The proximity of Salima to the capital, Lilongwe, meant that at that time of year many men

232These efforts to track ongoing and real-time changes in the lives of respondents were often confounded: on one occasion in central Malawi, for example, two people had gotten married *and* divorced during the time between surveys, pointing to the limited ability of the categories to capture "real life."

migrated temporarily to work in the tobacco factories in preparation for largescale tobacco auctions that open around mid-March.[233] In these cases, even family members could provide little specific information about their male relative: "He is staying in Lilongwe, living with someone." Diligent field teams, during the "tracking" phase of fieldwork (discussed below) would, in some cases, follow up on such cases even despite the meager clues provided by relatives as to the respondent's whereabouts.[234]

Decisions made about sampling in the office had direct impact on the fieldwork team. Orders came down from the office or, in most cases, from technically trained principal investigators who oversaw sampling, that the sample was to be selected in a certain way, that everyone in the sample must be interviewed unless they refused to give consent or were absent and that all the interviews had to be conducted within a limited time frame (usually several months). For example, one case study projects was investigating a series of research questions regarding the messages that Malawian church and mosque leaders imparted to their congregations about condom use, sexual behavior and other AIDS-related issues. The researchers decided that in order to effectively collect data that would answer these questions, the project needed to sample a variety of religious leaders (e.g. pastors, sheikhs, elders, congregants) across five different local religious denominations. The researchers specified the numbers in each category. These precise numbers often directed movements in the field. Field teams once spent an entire day in a mini-van "searching for Baptists" to no avail.[235] Similarly, for two other projects, the end of fieldwork was always a difficult and hectic time, as it comprised following up

233 Tobacco is Malawi's largest source of foreign currency and its most important cash crop. These seasonal migrant workers head to Lilongwe in preparation for the auctions just outside the city. At these events, massive bales of tobacco from all over the country are displayed in long lines to be tested for moisture content and appearance by buyers. Most tobacco heads to South Africa, from where it is shipped around the world.
234 Field notes; February 2008.
235 Field notes; June 24, 2008.

with individuals or households who had been missed during the first round of interviewing in a certain village or census-delineated enumeration area (EA). Called "tracking," this phase of fieldwork is important to the representativeness of data since the people who cannot be found might be different in important ways from people who can be found (e.g. younger people or wealthier people might be harder to find than older, poorer people). Tracking implies asking families or friends of absent respondents as to the respondent's whereabouts. Minivans carried team members from small villages in rural Malawi, for example, to distant neighborhoods where a person had migrated for work or to an aunt's house in a different district. Senior research fellows I interviewed at Malawi's Centre for Social Research (CSR) emphasized that sample size and complete enumeration were major markers of authoritative knowledge. They evaluated the Centre's own survey fieldwork practices well: "We have very large sample sizes and we are skilled at getting to all of the sample households."[236] While a sample necessarily "sees" only a portion of a larger reality, it magnifies that portion of interest. Qualitative samples are invariably small (a "large" qualitative sample would be 100 respondents, whereas a large sample for a survey would be 5000). It is difficult to show representativeness for a small sample: thus, Malawian social scientists who specialized in qualitative research often bemoaned their plight in trying to overcome critiques of small sample sizes that prompted policy makers to dismiss their research. This makes the position of more qualitative social scientists in Malawi precarious as they seek to conduct research studies with large enough sample sizes to produce authoritative results with

236 Interview; October 11, 2007. CSR is one among many institutions that provides fieldworkers on a contract basis to international and national research projects.

increasingly limited funding.[237] In Chapter Four, I discuss other ways in which qualitative research is delegitimated in the Malawian context.

Watching Over the Sample

Malawian researchers and expatriate principal investigators alike often used the phrase "sample purity" to emphasize the need for a clear-cut, isolable sample quarantined as much as possible from confounders or contamination. Even during colonial times, researchers were preoccupied with enumerating pure, isolated samples that were easily "seen." In a 1955 letter, the East African Virus Research Institute based in Entebbe, Uganda, corresponded with a Dr. Z. H. Murcott, Director of Medical Services in Zomba, Nyasaland regarding work on yellow fever epidemiology. The brief letter details how blood samples from Chilwa Island should be labeled (including name, sex, place of residence and amount of travel) and elaborates exclusionary characteristics (those recruited by labor boards for work in South Africa or those who have served in the army). The excitement about this island sample was palpable: "The island offers a fairly *closed community* [my emphasis], great possibilities for all kinds of investigations, which if done might produce much valuable information" ("Medical Surveys" 1935).

Although social scientific survey research is not burdened by the same imperative for biologically pure samples biomedical research is, these projects also orient themselves toward "sample purity."[238] The fieldwork office, for each case study project (with one exception), was located near to but not within "the field" comprised of sample

237 Holland (2006) provides a clear discussion of the internal and external influences on the institutionalization of the social sciences in Malawi. She shows how neglect of the university system in Malawi has deinstitutionalized and decredentialed the social sciences.
238 Petryna (2009) documents global pharmaceutical companies' search for "treatment naïve" (pure) populations in low- and middle-income countries.

households. This permitted the researchers to imagine the office as a neutral and central place where data could "stand alone" or be unmoored from its complicated context. At the end of each day in "the field," the trip back to the fieldwork office was central to the ability of research teams to analyze and manage data. The mobility between these two spaces paralleled the mobility of miniaturized data points to the office and beyond once they were extricated from their social context in the field. The coming and going, in fact, allows for efficient and easy data analysis; during the minivan trip back to the office, data become purified and depersonalized through their detachment from the impurities of the field. As the village grows smaller and smaller in the rear-view mirror of the minibus, the survey responses waiting to be entered into a database also "shrink" village realities.

Sampling is a technology that allows researchers to see individuals who are "inside" the sample and avoid the distraction of the individuals outside the sample. In July 2008, HIV-counselors in a minibus were flagged down by a women's group who wished to be tested for HIV; the supervisor assured them that they could without knowing whether they were "in" or "out" of the research sample.[239] Though supervisors and interviewers often said they wished more individuals could be "in" the research sample, their gaze was necessarily focused on the parts of reality they needed to "see" in order to do their jobs. This placed fieldworkers in an awkward position in the face of villagers' critiques that sampling strategies were unfair, and in the face of some of those who were excluded wishing to be added in.

Often, sample purity was threatened (or compromised) through tactics employed by rural Malawians to benefit from the project's presence in their midst. Three of the four

239 Field notes; June 4, 2008.

projects I studied were longitudinal studies; they must interview the same respondents year after year in order to discern trends in individual (and population based) lives over time: What percentage of respondents have become infected with HIV? How many respondents became divorced (or married) since the team was last there? How have economic trends affected household wealth in different regions of Malawi? The answers to these questions necessitate a certainty that the same respondent is interviewed from one year to the next. While I have discussed the issue of informants lying or not disclosing information, I noted in Malawi that informants can "lie" even before they open their mouths to answer questions. In one case, an interviewer arrived at a household, looking for a male respondent he had been assigned. He was met by a woman who claimed to be the wife of this man and who reported that her husband was "out." The interviewer, glancing at the "specs" he had on a piece of paper for his male respondent, said: "Wait, but John does not have a wife, according to our information. He is unmarried." At this, the woman insisted that they had married since the last time the project had come through; she gave her surname as his. Following project protocol that "new spouses" should be interviewed, the interviewer settled in for the long session. Toward the end of the session, the respondent returned home. When the interviewer mentioned who he was and that he would interview John following his session with the wife, John laughed aloud, saying, "Eee! She is not my wife!" At this, the "wife," too, began laughing at her cunning. In reality, she was John's sister in law; her husband was working in Lilongwe. Visibly frustrated, the interviewer ended the session with the woman; this had been a waste of an hour. For a moment, he deliberated whether or not to provide this woman with soap—"*Sopo?"* she asked expectantly, holding out her hand.

The interviewer reluctantly dug into his bag and handed it over. Important about this exchange is the way in which the women was aware of the project's sampling strategy: she told me later that she had "felt like chatting" and "wanted some of that soap" and reported she heard from a friend that the project was interviewing spouses too.[240]

Though sampling characteristics and strategies are presumed to be opaque to respondents (aside from being described as "random"), the bulk of gossip in this area had amounted to a widely circulating public knowledge of how to "become" a respondent and how to be "inside" the sample. Though the project employs many measures to ensure that sample purity is maintained, this woman had flexibly inserted herself into a category yet unbounded by the strictures of historical interaction with the project. Because she was performing the role of a "new spouse," the project lacked a photograph or information (about her age, appearance and so on) with which to properly verify her identity. She manipulated her identity within the confines of the project's technologies of seeing—in this case, the white chalk imprint of the project's acronym above the door of their household and the map that had led the interviewer to "her" household. The woman's desire for soap speaks to a larger general preference of villagers to be "inside" as opposed to "outside" project samples, despite the complaints and ambivalences about research I highlighted in Chapter Two. There is a widespread sentiment that, "we will get something in the future out of participating." While this woman was motivated to "lie" by her desire for soap, other respondents inside the sample worried about being "dropped" from the sample.

240 Field notes; July 4, 2008. A similar case involving a woman who successfully pretended to be her sister occurred in my field notes on March 7, 2008.

Maintaining sample purity also implies many maneuvers on the part of the fieldwork teams. These maneuvers are unscripted, always evolving and responsive to contextual and unpredictable conditions—both environmental and social. Sample purity necessitates quarantining the sampled individuals and creating defenses against infiltration of the sample by non-sampled individuals, delays in reaching all the sampled individuals and respondents' refusals to be interviewed. Though the project had a standardized arsenal to confer it immunity from such invaders, the fieldwork teams often had to fill in gaps in this arsenal with "leg work," adaptations or real-time modifications. Working on a longitudinal project implies two things: 1) Interviewers and supervisors are likely to be retained from year to year; and 2) Fieldworkers interview the same respondents from year to year. In this way, although research comprises ephemeral and short-lived stranger intimacies, these are intimacies constructed from the stuff of historical and anticipated encounters. These intimacies that stretch across the time lag between field seasons necessitate that certain strategies be employed to ensure that good relationships are maintained and reproduced between not only the project and the community, but also between the interviewer and his/her subjects (and potential future subjects). How does the project-level mandate to "treat our sample with respect" manifest itself in the everyday interactions between fieldworkers and villagers? Moreover, how do these interactions serve to enhance or erode sample purity?

For the social science research project, a pure sample refers to a sample in which as many of the listed respondents are surveyed and/or HIV-tested as possible. It also means that measures must be taken to prevent the project staff members from interviewing the wrong people and to discourage respondents from refusing to be

interviewed or hiding from interviewers. The official, project sanctioned tools toward this purpose comprise: photographs of respondents to verify identity, hand drawn maps that direct an interviewer to his assigned household and some limited identifying "data" collected from the respondent in past years.[241] However, the fieldworkers soon developed more effective ways for preserving/ensuring sample purity. These were: establishing social, business or leisure relations with those in the sample, repaying villagers promptly for property broken or affected during fieldwork and drawing their own maps (with the help of local people) when the written one failed or was invalidated by environmental conditions or terrain.

In order to maintain sample purity and to collect good data, a research project must maintain good relations with the community. After all, those "in" the sample are also "in" the community to varying degrees. In addition to being transparent and clear about research protocols and logistics (how long a project will be in the area, what information the project needs, how the information will be used) and going through the proper channels before speaking to respondents (a project first presents itself to the district office and police station and, from there, to Traditional Authorities (TAs), chiefs and sub-TAs), good community relations are also maintained through everyday, more mundane practices. Research teams have come to view the field not as a social context frozen in present time and space but as an always evolving and dynamic place whose contours stretch across time. Actions in the present are geared toward preserving and reproducing positive relations between the project and the community. While it is the researchers who have perhaps the greatest interest in maintaining these good relations,

241 For example, if a respondent claims he is 39 years old this year, and his past response was 42, it is the interviewer's job to discover whether he is lying or a different person altogether.

the research assistants, as current *and future* employees of research projects who return again and again to the same locales also have a vested interest in ensuring that their future work is as easy and painless as possible. My suggestion in Chapter Two that research participants assess the costs and benefits of participation in research through the lens of a *long durée* of interactions and exchanges between themselves and outsiders is one example of how a wrong move in the present can increase the likelihood of low participation rates, refusal and resistance to research in the future.

One rainy day during fieldwork, a project SUV was slowed down by grasses that were as tall as humans. The van swam through the reedy green impediments like a barge caught in mud, often getting mired in wet ground hidden beneath the ciliac protusions. The day was frustrating and morale was low; the absence of the road that was apparent during the dry season made the work of finding the sample households close to impossible. Suddenly, the SUV struck something hard. Out of nowhere, a young man emerged, yelling that we had run over his family's clay pot filled with the day's relish (*ndiwo*). The supervisors got out of the car, apologizing profusely to the man. He accepted the apology but suggested that the project owed him compensation for the breakage. The supervisors looked at one another indecisively. Should they give this man monetary compensation for his loss? Was this the right thing to do? Which money should they use? After a few brief minutes of whispering among themselves, one supervisor went to the man and offered him six hundred *kwacha* (about $3.50 at the time). The man received it graciously. The supervisor later explained the story to the researchers and was

compensated for the money he had borrowed from himself to cover the costs of the broken pot.[242]

This incident provides insight into ways in which projects seek to protect good relations between themselves and their research subjects. In fact, these relations are viewed as protective of sample purity. Although the broken pot was technically "nobody's" fault—i.e. it was hidden beneath grasses and its breakage by the SUV could not have been prevented—the project's front line staff took responsibility for this item by deciding to financially compensate the injured party. Later that evening, I sat down with the supervisors who made the decision and asked them to explain why they decided to pay the man for his pot. They suggested that the gesture was one of good faith; the exchange of money for the broken pot performed and reinforced the project's commitment to fair and positive relations with its subjects and its adherence to the dictum "do no harm." Giving the money, they said, ensured that this man would not go back to his household and village with bad feelings for the project that could influence whether he or his family and friends welcomed the project in the future or participated in the survey. A simple and seemingly minor good faith exchange, in this instance, serves an important role in ensuring future sample purity and retention.

242 Field notes; February 30, 2008.

Figure 3.4: Fieldwork vehicles caught in the mud during rainy season (Photo: Author).

Clean Data

Clean (high quality) data is another major epistemic virtue. "Data cleaning" is a familiar term to demographers and other quantitative social scientists, for whom it indicates the procedures and techniques applied to a database *after* data has been entered in order to identify errors; this section is interested in showing how the imperative to collect "clean data" informs the everyday relations, movements and tactics of fieldwork and fieldworkers.

Data cleaning of databases ensures that errors introduced either during the fieldwork or data entry phases of research are ferreted out from the final product.

Common errors include: missing data, a “blank,” typing errors upon data entry, data collected from the wrong respondents, column shifting (entering data that belongs beneath one column in the neighboring one) and fabricated data (i.e. “cooked data,” in the sense that the fieldworkers use the term). For example, if a data entry clerk enters data incorrectly, the error can have significant effects on numbers derived from the data. If a respondent does not answer, is faced with an inapplicable question or does not know an answer, an interviewer typically codes the non-response (missing data) as “999.” This code must then be recoded as missing information; if not, the figure (999) will be counted as regular data, which could lead to the inflation of the mean of a given response to a question. Errors are cleaned out of data in two phases: detection and correction. Errors are detected through descriptive statistics, logic checks and assessment of frequencies. Once errors are detected, they are corrected so that the integrity of data is not lost or compromised.

Figure 3.5: Data entry team in the field office (Photo: Author).

Even in addition to these after-the-fact procedures, research projects adopt a number of strategies to mitigate errors before they enter into the database. Researchers and research teams, for example, are aware of the possible effects of "human error" on their data.[243] A group of demographers (Mensch *et al* 2008) concerned with possibly unreliable data discuss differences in self-reports of sexual behavior among a sample of unmarried adolescents in rural southern Malawi who were either administered the survey by a live interviewer or via ACASI.[244] Dionne (2011) finds that Malawian research assistants who are from a local area (and likely the ethnic group in the respective area)

243 Researchers are suspicious of data they collect and have questioned its reliability and flaws in the collection process in many developing countries (cf. Cleland *et al* 2004 or Hewett *et al* 2008).

244 ACASI stands for audio computer-assisted self-interviewing, a technique designed to collect data on sensitive issues. The software is designed so that the respondent hears both the question and the response categories through headphones and then answers each question by pressing a number on a keypad. This can help reduce what researchers term "social desirability bias."

were no more efficient in completing interviews more quickly and no more likely to report a higher degree of cooperation during the interview. Poulin (2010) compares reports of first sexual encounters between a standard survey and an in-depth interview, concluding that flexibility and reciprocal exchange in the context of a face-to-face interview may produce more truthful reporting than more standardized methods. I had many conversations with expatriate and Malawian principal investigators in which they very clearly discussed the limitations and possible confounders of their studies.[245]

Cooking and Callbacks

"Callbacks" were an element of fieldwork meant to protect the quality of data collected by research projects. When an interviewer returns to his supervisors with a completed interview (all questions of the survey filled in neatly), supervisors check the survey in a form of "quality control." This checking involves a meticulous reading of the survey: Are all the responses filled in or do blank spaces remain? Are the responses legibly written? Do the responses make sense and appear logical? Callbacks were an important tool employed by the project to manage and monitor its labor force. Once per week, supervisors were responsible for ranking and evaluating their interviewers, based largely on how many callbacks an individual had done that week (callbacks were viewed as a major waste of project resources because they require extra time and fuel). One case where the answer to the last question was "no" shows how callbacks are a space of negotiation and maneuvering on the part of interviewers who occupy the middle space

245 One project addresses decontextualization of data by providing some context through analysis of journals kept by amateur "hearsay ethnographers." This project aims to capture the content of informal conversations that otherwise go unnoticed or undocumented by questionnaire methods. These journals attempt to collapse the distance between miniaturized data points and their origin in a dynamic context.

between research participants and their bosses. One day in late July, an interviewer called Mike[246] came back to the team minibus with a survey. He sat down in the back of the bus, settling in to drink his lunch—a bright pink (strawberry flavored) *Mahewu.*[247] One of the supervisors, Ethan,[248] began checking his completed survey. With a flourish, he circled something on page 7. Sighing, the interviewer moved to the front of the van to ask what was wrong. Ethan explained that there was a contradiction between the number of children a man said he had in his first versus his second marriage. The survey indicated that he had 4 in his first marriage, and 3 in his second. On page one of the survey, however, the household roster indicated that he had eight biological children living with him. Why the extra child? After he finished checking the remainder of the survey, Ethan handed it back to the interviewer and told him to go back out to inquire with the man about the discrepancy. Exasperated, the interviewer complained that the house was more than a half hour from the van on foot. Because he was from this area, he suggested visiting the household to do his "callback" tomorrow morning on his way to the trading center where the vans picked up interviewers at 7am. "I'll get the response then. I'll figure it out then," he said.[249]

The next morning when we picked Mike up at the trading center, he handed Ethan the corrected survey. Although the numbers now matched up, Ethan pressed Mike to relate the conversation with the respondent. Mike fidgeted and grew obviously uncomfortable; Ethan accused him of cooking data. "You didn't even go back! You just walked here after crossing out your first marks!" Later that day, Mike was dropped off at

246 Pseudonym.
247 *Mahewu* is a widely sold maize based food-drink with a sour taste that acts as a popular meal for poorer southern Africans.
248 Pseudonym.
249 Field notes; May 25, 2008.

the very same household to fix his mistake(s). After dropping him, the minibus drove away. In the late afternoon, we passed by the house again: Ethan shouted, "Wait! Stop here!" He proceeded to the house to ask the respondent in question whether Mike had returned to re-ask him the question about his children. "Eee… No! *Palibe*!" [No! He hasn't been here!] With this negative response, Ethan re-asked the question, recorded the data, and came back to the van to scold Mike, whose hanging head and stony response to jeers from his fellow interviewers ("Cooking in the kitchen!") betrayed his embarrassment. Following this exchange, I asked him to explain his reasons for not completing the assigned callback. He said, "You know… it's harder than you think to make someone sit for two, three hours or more and then have to go back and say *"Hodi!"* again. It's like you become a laughingstock." He explained that he had preferred not to go back to revisit his respondent due to embarrassment, having already wasted a lot of the man's time. Knowing full well that being a "good interviewer" means collecting complete and correct (non-contradictory) information, Mike had nonetheless decided that it was not worth it. Instead, he wished to avoid the social awkwardness and histrionics necessary to collect a piece of information he later described as "insignificant."[250]

Mapping Human Terrain

In order to collect "clean" data, research projects must be able to identify sample respondents against a background of environmental and social complexity. In other words, they must interview the right people. Maps were an important tool that served to make complex and unknown social and geographic terrains in the field more manageable

250 Field notes; May 26, 2008.

and visible. Maps, however, were not stable and permanent texts, but rather dynamic works-in-progress. Many kinds of maps and mapping exercises were involved. When teams first arrived in a district (especially if they had not previously worked there), they headed to the National Statistics Office (NSO) or district offices in order to collect recent maps of the human and physical landscape that would be their home for the next few months. However, the maps did not always "tell all;" in addition to determining physical impediments that might block the project from accessing certain districts (such as rivers, mountains or the lack of a tarmacked road), mapping a potential fieldwork area necessitated local knowledge of the social terrain. In the fieldwork office a few days before sampling began for one case study project, an American researcher pored over a large map borrowed from the NSO that covered an entire table when unfolded. She suggested a certain enumeration area close by the field office might be a good site for sampling. The Malawian research supervisor, however, disagreed. "You see, that particular area of Chiradzulu… the chief there does not live with his people. He dwells in Limbe [a part of Blantyre city about forty minutes by car from the office]. It will be difficult for us to talk with him and to acquire his permission to sample his village and if we have any problems [with the villagers] there will not be an authoritative figure to help us get out of them." This piece of knowledge became part of the mapping exercise prior to fieldwork in that it motivated the movements and direction of the teams. The project decided not to work in this district, despite its convenient location and friendly physical terrain; the social impediments were too great.[251]

251 Field notes; November 19, 2007.

Formal maps like those stored at NSO, however, were not the only, or even the most important, way in which field teams found their way. Two case study projects created maps that were accumulative condensations of archived project-knowledge. These maps were contained on 8.5 X 11 inch sheets of paper. At the top of each was a space where the household number could be listed (household numbers were often chalked on to the top of the house in question) and a box for comments to be written by the interviewers. These maps were mobile, and typically part of the toolkit of objects carried by each interviewer. These comment boxes often contained handwritten information meant to direct future interviewers to the household. For example: "The household is behind a small thicket of trees just off the dirt path running behind the training center. It is a sundried mud hut and there is a waterhole out back." In addition to these verbal instructions, the maps contained pictorial and symbolic representations to help show the way: miniature trees, churches, kiosks, vegetable stands, rivers, roads and paths marked with arrows pointing in the right direction. Of course, these maps were necessarily imperfect and perpetually inaccurate—from one year to the next, a vegetable seller may relocate or a tree may be felled by lightning or human hands or a water hole may run dry. In this way, each crop of interviewers was instructed to correct or improve the maps as needed. Using a pen or pencil, they drew over, crossed out and refined the maps. In cases where things had drastically changed, interviewers started over on a fresh piece of paper.

These maps were invaluable tools in locating sample households, especially remote or far off homes. Once interviewers arrived at a household, there were still unknowns. In some cases, families had moved away to an unknown location, leaving the

house empty or filled with a new family. Even those households that retained their original inhabitants can be "mis-read." The maps that help interviewers find sample households are complemented by photos meant to enable the research assistants to determine with certainty the identity of an assigned respondent. Increasingly, each respondent in the sample is photographed with a digital camera.[252] The photo is then printed out in the field office (usually in color) and attached to the clipboard of the interviewer assigned to visit that particular respondent that day. Yet, this did not solve the problem of "names." After all, relatives could meet the two main criteria for identification, even if they were not the respondent: they both resemble and possess the same surname. If the respondent information indicates that the name in question is Banda, the true Banda's younger brother can very easily be misrecognized as the correct Banda. Similarly, interviewer error could lead to misrecognition (A person's name spelled as "Simon" on page 5 of a survey could be "Simson" on page 11).

Further, the field office is a place where highly personalized information (such as number of sexual partners, HIV status) is, first, depersonalized through the assignment of a numerical ID code to each respondent and, second, universalized into a combinable and intelligible data point part of a larger data base. Depersonalization, in fact, unmoors information from its human source and converts it into the universal currency of statistics or data, the package in which it will continue its journey away from the field. Ultimately,

252 While some respondents refused to have their photo taken, most were cooperative. The taking of the "snap" (as photos were commonly called in Malawi) was a quick and simple affair. The respondent in each case held up a piece of paper on which was written, in thick black marker, their personal identification number. This gave the photo a certain posterity—in future years, even if the respondent's appearance changed, their number remained the same—it was uniquely theirs in the way a hairstyle, shirt or body type cannot be. The photographs produced some unexpected outcomes. Gradually, villagers became aware of the "snaps;" interviewers often showed them around to garner local help in locating respondents and respondents themselves noticed them on the clipboards. Soon enough, project participants began asking whether they could keep their photographs. After some discussion between the supervisors, they decided this was an agreeable arrangement. The photos took on a dual role: as a technical device through which researchers could better "see" and as an unscripted and unintended gift that villagers appreciated more than the soap.

the surveys are given to a data entry team (sometimes in the office in the field and other times in an office as far away as New Delhi) and transformed into coded numbers. These databases, saved on flash drives or laptops, become the objects from which researchers make generalizations and claims about AIDS risk or prevalence in Malawi; digitized files stand in for and come to represent social reality in rural Malawi.

Managing Uncertainty

Researchers have a toolkit or arsenal of technical objects and techniques to help them "see" social reality. The mechanisms of this seeing rely on the capture and miniaturization of slices of rural Malawian reality; data, or relics of social reality, attain mobility only in miniaturized forms. However, researchers' seeing also depends on an ability to magnify the realities contained in these miniatures. The processes described here have illustrated this simultaneous miniaturization and magnification through isolation, decontextualization, depersonalization and mobility of information and reality. I have also shown that these processes do not occur helter-skelter: they are highly standardized and formalized in their aspiration to align with four major epistemic virtues. Even as these virtues permit "seeing," they also create blindspots. Each of the epistemic virtues described in this chapter also has an epistemic underbelly; seeing is also not-seeing.

While this chapter would now logically move to elaborate all that researchers miss, I am most concerned not with pointing out the shortcomings of knowledge produced by research projects, but with elaborating how epistemological commitments and values both *enable and constrain* representations of social realities. How is

uncertainty simultaneously acknowledged and effectively *managed* by producers and consumers of AIDS knowledge?

Building AIDS Narratives, Managing Data

The language of social science in Africa plays an enormous role in truth-making and underlies globally circulating AIDS narratives. "Power," "statistical significance," "sufficient sample size," "reliability" and "Chi-square tests" work to magnify the certainty and mitigate the uncertainty of data. These epistemic virtues, if adhered to, are presumed to establish credible evidence—AIDS truth. In her account of the sacred position held by the randomized clinical trial (RCT) biomedical clinical research, Vincanne Adams reconstitutes the relationship between medical "facts" versus "beliefs" by showing that RCTs are assigned an inherently magical power that, while necessarily juxtaposed with "criminal" and non-scientific traditional medicines, does not recognize itself as magical (2003:680). As Chapter Four shows, the faith in and reproduction of facts about the AIDS epidemic in Africa become sacralized and magical through the exclusion of other 'facts' that risk eroding this power. We hear and learn about AIDS in Africa through narratives built from data. Co-opting Levi-Strauss'[253] memorable opposition of the raw to the cooked, I suggest that data serves the same function as mythemes—a datum is the smallest unit of evidence that can be combined and recombined into certain "stories" about the AIDS epidemic and rural social reality in

253 In *The Raw and the Cooked* (*Le Cru et le cuit*), the first volume of *Mythologiques* (1964), Claude Lévi-Strauss asks why myths across cultures are so similar. Setting out the principles of his method of structural analysis, he elucidates the shared features of different myths and the transformations that link them. His analysis centers on the mytheme, the irreducible unit of a myth shared by many myths (e.g. the trickster). Myth, as culture, acts to bring order out of chaos—culture organizes knowledge into binary opposite pairs of things and simultaneously functions to resolve contradictions between them. Lévi-Strauss suggests that culture "cooks" raw materials from the environment or social context to organize and make them intelligible to its subjects.

Malawi. A data point—say, a numerical response given by a household member to the question: How many sexual partners have you had this year?—becomes one among many such points collected in similar ways across households (or across villages, regions, and national borders). Each of these points, like a mytheme, is converted from raw numbers on a survey to the cooked evidence that underlies knowledge claims about the epidemic. Further, these data simultaneously rely on and resolve contradictions or oppositions—between researchers and the researched, knowns and unknowns, facts and beliefs, urban and rural, center and periphery and HIV positive and negative. Further, the stories social scientists tell about AIDS in Africa are populated by characters ("the sugar daddy," "the prostitute," "the unfaithful husband," "the circumcised boy") that are as consistent across contexts as Lévi-Strauss' famous mythological "trickster." Each of these characters organizes a set of data points and becomes subsequently interlocked with other building blocks to construct widely circulating narratives (or myths?) about the AIDS epidemic. As becomes clear in Chapter Four, these stories attain a certain sacred status when actors remain wedded to them and reproduce them.

Seeing and the Self

Managing uncertainty also entails managing the people and processes that produce data. Obviously, the vagaries of fieldwork and messiness of rural social context as it interferes with fieldwork and data collection disallow the ability of researchers to assemble completely "clean" databases. Yet, researchers, like others, rely on an arsenel of tools to mitigate the "dirtiness" of fieldwork (human error or imprecise sampling, e.g.) of their final product—data worthy of being evidence for future knowledge claims. Daston

and Galison suggest that epistemological objectivity relies on certain epistemic virtues: certainty, precision, replicability. Just as scientific "seeing" cultures internalize and enforce these virtues by appeal to ethical values (2007:40), social scientists adhere to a unique set of epistemic virtues that serve as a measuring stick for evidence and knowledge claims made by members of this research culture or knowledge community. These virtues center on researchers' primary preoccupation with timely, high quality data. This kind of data is accessible only through replicable collection methods, through standardized guidelines that seek to tame the potentially unruly or unscripted practices of research assistants, through large enough sample sizes, through application of tests (e.g. Chi-squared or t-tests) to data and through highly scheduled and time-sensitive everyday fieldwork plans. Each of these virtues helps to ensure the power, statistical significance, ethical collection and certainty of knowledge about AIDS.

This particular orientation to social reality as something that must be condensed consistently across hundreds or thousands of exchanges between interviewers and their subjects relies on techniques of data collection, but also techniques of self. As this chapter has shown, a corpus of gestures, techniques, habits and temperaments ingrained by training and daily repetition and adopted by researchers and their employees are a precondition for the production of knowledge. They also enable researchers to "see" the parts of social reality that interest them while disregarding or averting their eyes from those that do not. Data and the social scientific self are always in flux together, mutually constitutive and dynamic against a backdrop of social reality.

Captured, "Good Enough" Numbers

An emphasis on timely, mobile and high quality data privileges the capture of a number over the accuracy of a numerical sign itself. Interviewers, for example, discussed the vagaries of responses to questions such as "How many people have you received financial assistance from in the past month?" Respondents were generally hesitant, prefacing any response with the Chichewa filler, "Eeeeee…." as they attempted to remember, to convert a long series of exchanges into a number. With prompting from the interviewer, they would finally settle on a figure—say, "10." The number was written into the box on the survey by an interviewer. That evening, "10" would be entered by data entry clerks into a computer program to store coded data. Researchers, when writing papers in the future, would dip into the preserved database, enlisting this "10" into a generalized claim such as "Rural Malawians, on average, receive financial assistance more than X times per month." Any uncertainty on the part of the respondent was not a threat to the evidence as defined by researchers: high quality data is not defined by details about how a number is produced but rather by the fact that the number is captured. After all, while the project cannot adequately "measure how much" a respondent may be "lying,"[254] it *can* measure how many responses are not captured. In this way, projects whose databases or knowledge claims are deemed to possess a high degree of certainty enlist but also mitigate forms of uncertainty inherent in the *production* of numbers.

Researchers also manage uncertainty by making claims that are representative but not comprehensive. Numbers and other data thus refer to but do not encompass social

254 This issue of lying informants is a concern for all social scientists who rely on information from others and whose main form of "seeing" is, in actuality, through listening. The four case study projects were not naïve in this regard. Researchers and fieldworkers alike ferreted out obvious inconsistencies in responses, were thoughtful about whether known or strange interviewers collect better data, and implemented complementary projects that attempted to capture everyday realities as they unfolded when research projects were not in town.

reality. This is most evident in the ways in which data analysis takes interest not in unitized but in aggregated individuals. Upon entering a sample or a database, numbers or data associated with individuals are stripped of context and complexity. This was most obvious during the time I spent with a cash transfers project in central Malawi. These interventions are an increasingly popular way to provide social safety nets in sub-Saharan Africa and are often evaluated through numerical measures of their beneficial effects on households and communities (Doocy *et al* 2006; Harvey 2007). Davies and Davy (2008), for example, found that an emergency cash-transfer programme [sic] in rural Malawi brought widespread benefits to the regional economy as a whole based on "multiplier estimates of 2.02 to 2.45." These multiplier estimates were outcomes of research that collected data via questions similar to the ones asked of Namoyo by Janet above; they took interest in the circulation of money through local social networks and beyond. Recipients of cash transfers were asked a series of questions meant to determine how these individuals 'use' their money: do they start new businesses? Do they buy more commodities from local businessmen? Do they enroll another child in school? Often, respondents who provided numerical or other responses to such questions orally contextualized their answers. One person described that his last month had been unusual because he borrowed money from neighbors to stage a party for his young son's initiation. A sick relative's transport to hospital or an influx of cash from neighbors at the time of a spouse's funeral are other kinds of contextual qualifiers lost to researchers or policy makers. Further, the answers to these questions will vary depending on the time of year in Malawi, where economic ebbs and flows emerge from agricultural cycles of planting, harvesting, and selling maize. During the rainy season, families may invest

more resources in curing children sick with malaria than in the dry months when mosquitoes are less ubiquitous. Some places that benefited from these cash transfers may have duly benefited from an influx of food aid unbeknownst to those estimating the transfers' efficacy. However, each of these vagaries is minimized by the capture of a number that serves to magnify the validity of the data and the success of the project. The figures miniaturize the spending, saving and investment practices of ten thousand sampled households;[255] the primacy of the sample and the imperative to count and measure makes the details of any one household's actual behaviors irrelevant to policy making and testing of a model for intervention. After all, an average, not an individual household, is representative of reality.

Uncertainty was also managed by formulating studies around questions and concepts that were easily measurable, even if they were overly simplistic or mismatched with a local context. Surveys are usually broken down into sub-sections that center on concepts formulated in the west, for example, "social capital," "marriage" or "sexual behavior." In the social capital section of one survey, for example, a question asked, "How safe do you feel from crime and violence?" Responses ranging from "very unsafe" to "very safe" were coded by numerals circled by the interviewer from one to five, respectively. While many respondents felt "very safe" in their villages, the question overlooks the fact that multiple definitions of crime and violence inform the answers given. If some respondents interpret this question in terms of a definition of crime and violence associated with images of gangs, guns, pangas and rape largely imported from South Africa, others classify the perceived widespread phenomena of witchcraft and fear

255 The average transfer was $12.26 per month for five months.

of bloodsuckers as "crime and violence," even if they omit explicit mention of these forms when they respond. While these kinds of crime significantly impact, for example, inter-village relations and trust (social capital networks), they remain largely inaccessible to researchers who see through the lens of this question and take up the statistical norms generated by an analysis of the average response given to this question. "Very safe" on average is not necessarily very safe in reality. Yet, the evidence that Malawians feel "very safe" in their villages is considered valid without recourse to the context; even in places where widespread witchcraft accusations produce profound insecurity, "very safe, on average" is *representative* because it emerges from sufficiently large sample sizes and attains high statistical significance scores. Further, any claims based on this question are, for the purposes of researchers (and policy makers) valid even if they exclude what is considered to be irrelevant to practical, individual centered AIDS interventions anyway: aspects of rural African social reality coded as superstitious, backwards or dying out. I suggest that although coded numerical responses, in miniaturizing or simplifying rural realities, do miss out on important social contextual factors, the larger knowledge projects into which these numbers are enlisted devalue this kind of evidence and preemptively direct researchers' gazes.

Provisional Numbers and Overlapping Temporalities

What falls out of sight of research projects? The greatest irony perhaps lies in the fact that the presentist, static and stable representations (relics) of the social collected by "social" science necessarily rely on the excision of data from the social. In discussing what he considers as a paradigm of the government of living beings and an

archaeological form of biopolitics—pastoral power—Foucault suggests that the shepherd is in charge not of individuals but of a herd (1978/2007:125-136). This pastoral orientation to the herd is characteristic, also, of the researcher's orientation to the sample. As this chapter has illustrated, the project's main priority and concern, in the field and in the office, is with the sample. It watches over this sample using the techniques of seeing and enumeration described in this chapter. Though the project, on some level, "knows" each of its individual subjects (via the face-to-face interview encounters) much as Foucault suggests the pastor or the shepherd know the individual members of his "flock" of souls or sheep, its imperative is a homogenizing of lives through reduction of each of these encounters into data. Individuals and context fall out of sight as the sample becomes visible.

Critics of AIDS policy in sub-Saharan Africa suggest that policy and the evidence it rests on misses or misrepresents important aspects of social reality; this is often an explanation for their failure or shortcomings. These critiques often presume that researchers are overlooking something or that research practices are inadequate. I argue, however, that researchers do not miss anything; they see exactly what they intend to see; their data make stability and fixity in representation possible (Lampland 2010). Even as they employ methods, objects, and techniques that serve as a lens into and a receptacle for the "social," researchers' gaze is trained on that portion of reality bounded by the major epistemic virtues discussed in this chapter. More importantly, "AIDS research" already focuses its gaze on AIDS, developing tools that seek to capture a specific slice of

reality that is infected or affected.[256] As I have shown here, the everyday practices of research fieldwork anticipate and align with these ethicized ways of seeing to make good numbers and high quality data. The priorities, standards and the privileging of numerical evidence in the policy-research nexus create an epistemological matrix that places researchers in a house of mirrors.

Though international research projects might be considered an exemplar of the role of standards and enumeration in this era, research is often taken as uncritically good and the investment in its forms or products often distracts attention from the processes that produce the forms. Enumerative practices and technologies play a central role in collecting pieces of local reality, transforming them into data and enabling their wide circulation. The practices I have discussed here—survey design, sampling, achieving sample purity, a questionnaire with short answers for huge questions or large slices of life, human error, voice recording and imagining the field and office as different—serve to transform people into numbers, households into dots on a map and voices into disembodied data stored in a recorder or a survey. Even as they produce blind spots, they cover them over by framing them as "outside" the scope of research's (or policy's) interests. This chapter has illustrated the smuggling of uncertainties into numbers, data and knowledge claims about the epidemic. We might consider these numbers as "provisional" or "false;" the uncertainties they contain are accepted, first, because numbers are instrumental tools and, second, because their provisional status is acknowledged by those who produce them.[257]

256 Malawian interviewers and supervisors were "AIDS-fatigued;" they were bored with asking questions about AIDS and suggested that projects focused too much attention on HIV. A key informant said: "It's all about AIDS, all AIDS all the time. But what can anyone do? AIDS is money!"

257 See Lampland's discussion of false and provisional numbers. For her, these kinds of numbers are distinguished from numbers as we usually interpret them: referents to stable entities that carry the same meaning no matter what their context. False and provisional

Central to this transformation of information into data is the office's position outside the space and time of the field and its imagination as a neutral, sanitized space. Michael Lynch discusses a similar transformation when he shows how lab practices reduce an animal into an "abstracted version of a laboratory rat--- a set of contingent material and literary products of laboratory work" (1988:272). Lynch elaborates to show that the laboratory rat is a cultural object "held steady by a community of practitioners" (1988:279) as it is "rendered" through mechanized and mechanical actions into data. This chapter has shown that international AIDS survey research relies on similar transformations and that numbers are social relics which are "held steady" by shared epistemic virtues.

The common strand that runs through the fieldwork practices described here is time. International research cultures' concern with timely data, fieldworkers' sense that they were always running out of time, and the disconnect between field and office time indicate that overlapping temporal incongruities play a role in knowledge production. I suggest that temporal incongruity is a condition for the production of social scientific knowledge about the AIDS epidemic. Though our imagination of the field/office dichotomy recognizes these characterizations of field and office time as accurate because they are so familiar, the social effects of the intersection of the temporalities of these social fields are very real. While the field/office bifurcation described here appears to lock researchers in their time-sensitive and efficient offices and to incarcerate Malawians in a solidified "local," anthropologists maintain some flexibility within a discipline that

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numbers, in contrast, are self consciously aware of their temporary match with reality, the conditions and limits of their production and their imperative task as instruments to stand in for or act as placeholders for reality (2010:378). In the case of AIDS in Africa, the reproductive life of research is ensured by provisional numbers. Both the production and the use of these numbers is formalized via reference to acceptable uncertainty of the conditions of their production.

still privileges slow time and sustained engagement.[258] Capitalizing on this "slow time" to ethnographically explore the spaces and times within and between the field and the office can help demystify the power of miniaturization of everyday life into numbers and mobile forms of knowledge. Anthropologists have recently begun to attend to social conditions under which numbers are produced and in which they are sanctioned. Further, while many scholars focus on the technicization or medicalization of the social, I suggest it is important to consider how the episodic presence of research projects and their attendant enumerative practices in places like Malawi *produces* new kinds of social relations (jealousy among those outside of a sample), social groups (researchers, research assistants, scouts, "the researched") and social realities (as in when an area becomes "highly AIDS infected" or is classified as "super poor").

Yet anthropological critiques of knowledge production and deconstructions of expert cultures also rely on techniques of miniaturization and on temporal incongruities: the slow time of ethnographic fieldwork necessarily intersects with the frenetic time of AIDS research projects. The temporalities and cultures of research projects, research participants and the anthropologist are co-produced. When I explained to my informants that I was "researching research," they often asked: "Who is researching *you*?" This question invites us to consider how we too "see like researchers" when we employ

258 In contrast to timely data, anthropology privileges "slow" data. Whereas demographers said they felt they were less competitive for academic positions when they spent more time in the field, anthropologists must spend a minimum amount of time in the field to be deemed legitimate by their discipline. However, anthropologists' shared epistemic virtues of slow time, learning language, sustained engagement, critique and thick description mean that their representations of local contexts are often invalidated when assessed according to the epistemic virtues described in this chapter. In meetings with funders, for example, Malawian policy makers were expected to present figures and facts *representative* of the *immediate* AIDS situation. This was a game of time and numbers, quite literally. Funders often cut off Malawian speakers if they went on for too long, or pressed them to "use numbers" if they slipped into narrative. Numbers, unlike stories or thick description, are tied to material resources in that they determine whether Malawi receives continued funding for AIDS research and intervention. As will be described in the next chapter, more qualitative data were often viewed as 'interesting' or 'complementary' to more quantitative data, but usually could not stand alone as powerful evidence. In this way, anthropologists' persistent concern with translating their findings into policy or making anthropology "useful" points to the distance between two competing epistemic communities with different moralized codes for assessing validity of evidence.

disciplinary investments and instruments such as thick description, persistent critique, number aversion and slow time.

We have come now to the end of our time in the field and in the office; packing up the accoutrement of our temporary offices and removing our "field shoes," we head to some less peripheral sites where the data discussed in this chapter is enlisted into knowledge claims as good (or bad) evidence to justify AIDS policy or other interested interventions. While this chapter has analyzed how data about the epidemic is made in the field and how a group of shared epistemic virtues predict the validation of the evidence to be based on it, the next two chapters make clear how interested and empowered audiences for claims about the epidemic determine whether evidence is good or bad. Chapter Four and Chapter Five explore the porous boundary between research and policy and dissect the evidence underlying knowledge claims that seek to reorganize social worlds in Malawi.

Chapter 4

Making Evidence and Performing Knowledge

While Chapter Three analyzed the social processes and mechanisms by which data are collected and mobilized, raw data are only the foundation for evidence that underlies claims about AIDS—the subject of this chapter. Ideally, in order to be legitimated, knowledge claims about the AIDS epidemic must be rooted in good evidence amassed from raw data and effectively communicated. More diverse audiences and publics who use information about the epidemic have access to a proliferation of evidence. Whereas evidence has long been presumed as a "thing" or taken for granted, the mélange of actors and experts involved in producing and validating knowledge about AIDS in Africa—policy makers, African researchers, expatriate researchers, funders and local target populations—has begun to erode the assumption that evidence is solid or rooted in stable fact(s). This erosion of stability is often masked by elaborate spectacles or performances that are committed to upholding certain definitions of evidence and maintaining its solid facticity. This "solid evidence" is central to formulating policies and interventions into the AIDS epidemic that are framed as evidence-based. The next two chapters consider how evidence is assessed and deployed in the AIDS policy-research nexus.

Evidence as Social Artifact

I argue that evidence is an *artifact* of social interactions, interests and conventions that underlie knowledge production and circulation within and across groups. This chapter subjects evidence to an autopsy to unravel the entangled social processes that

give knowledge momentum within and across audiences and to show how evidence is often more permeable and "softer" than assumed. The autopsy is organized around some main questions: What determines the truth of an assertion or knowledge claim? What is "good" evidence and for whom? How do performances of knowledge facilitate the translation of evidence into claims that are convincing to specific audiences?

Taking a performative and social constructionist approach to the making of evidence, I suggest that audiences employ four main criteria in determining whether the evidence to support a claim is good, bad, sufficient, or inconclusive: 1) Does the claim align with shared, already validated scripts or assumptions?; 2) Is the speaker reliable or linked into respectable knowledge networks?; 3) Is the knowledge claim deployed within a convincing presentation or performance of knowledge?; 4) Does the claim translate to its audience (is it relevant)? Spanning these four questions, the chapter shows that the truth of a knowledge claim is verified only when the evidence attains a certain culturally shared threshold of bounded acceptability. To illustrate how these criteria operate, the chapter presents case studies of knowledge claims made in specific social contexts and analyzes the kinds of evidence employed to support them. These case studies explore three main types of evidence commonly deployed across the policy-research nexus: cultural, numerical and the transnational.

Following case studies, Chapter Five examines in greater depth these criteria and the processes of evidence-making that are generalizable among knowledge performances in the policy-research nexus. Thus far, I have followed the footsteps of the actors and objects that comprise international social science research projects in Malawi. In Chapter One and Chapter Two, I examined the "field" of fieldwork, showing how the social

infrastructure of knowledge production is built and exploring the relations and exchanges that comprise fieldwork. Chapter Three moved into the fieldwork office to explore the standardizing, miniaturizing and time-conscious practices that transform information into necessarily "timely, high quality data." Following the data produced in the office on its path out of the office, we arrive now to the "downstream" sites where this data is enlisted into knowledge claims about the epidemic that are evaluated and validated or challenged by audiences based on whether they emerge out of what they take to be "good" evidence or not. I use the term downstream to refer to sites distant from those of the production of data—conferences, articles, workshops and meetings—where knowledge claims aim to direct action or intervention into social problems (Latour 1987:22-25).[259] This chapter's discussion of "making evidence" provides case studies that draw on data produced by the four research projects; it examines a wide spectrum of claims and evidence to illustrate patterns among various actors and types of evidence.

Evidence is necessarily qualified by descriptors such as good, bad, insufficient or sufficient. The assignment of these qualifiers emerges from shared expectations held by evaluators or publics. I am interested in how kinds of evidence are formulated, put into use and articulated with one another and how people attribute and evaluate the use of evidence by themselves and others.[260] Policy makers, researchers, NGO staff and villagers assess a knowledge claim according to criteria or epistemologies that are valued

[259] Latour classifies knowledge claims ("sentences" for him) into positive and negative modalities. While the former leads a sentence away from its conditions of production and makes it solid enough to render some other consequences necessary, negative modalities are sentences that lead a statement toward its conditions of production to explain why it is solid or weak (1987:23). I suggest that most claims made in the arena of AIDS policy-research are positive modalities: they motivate a change of course, intervention or action (as in: "The highest risk group in Malawi is men who have sex with men" or "District hospitals are short on anti-retrovirals (ARVs))." These claims prompt audiences to conceive of "downstream" sites (clinics, government hospitals) where evidence can be applied. Upstream statements breed uncertainty: "Smith *et al*'s recent study contradicts evidence that multiple concurrent partnerships (MCP) are a key driver of the epidemic." Audiences are prompted to reconsider the grounds or proof of statements or facts before applying them to social realities.

[260] Cf. Lambek 1993.

by their knowledge community. These assessments can change depending on counterarguments, situated interests and relevance. Thus, we note that evidence is *made* in social interactions and through cultural conventions within the policy-research nexus.

The policy-research nexus, as I term it, is the junction between policy concerns and research interests or practices in sub-Saharan Africa. The sites that are discussed in this chapter—ranging from small meetings, research conferences, archived and current policy and research documents, journal articles and media accounts—are sources from which evidence is drawn and in which it is made. Though the previous portion of this study focused on sites specific and unique to the international AIDS research project, the spaces discussed here are pertinent to exploring the politics of knowledge production in AIDS research because they are venues where evidence is activated and mobilized. Building on raw data (say, responses collected on a survey or cheek swab samples), evidence comes into play when it is enlisted in knowledge claims. Research and policy spheres in Malawi rely fundamentally on evidence; researchers collect evidence to fill gaps identified by policy makers who require data to intervene on social problems. Though my own object of study is the manifestation of the policy-research nexus in Malawi, this nexus is actually a global-assemblage. The nexus (the "whole") is comprised of dynamically interacting parts; "once a [whole] comes into existence it can affect [its] parts" or the "materials out of which [it] is formed." Conceptualizing the policy-research nexus as an assemblage directs our attention to the micro-macro mechanisms through which a whole provides its component parts "with *constraints* and *resources,* placing limitations on what they can do while enabling novel performances" (De Landa 2006:34-35).

The rapid spread of the AIDS epidemic through Malawi and other sub-Saharan African nations directed national and international attention, resources and expertise toward the region, locking it into a larger matrix of other highly-infected nations. The wide circulation and mobility of the HIV virus through populations was accompanied by the circulation of expertise that sought (and seeks) to contain it. Policy is central to this effort in that it provides the conditions for, legitimates and directs the flow of interventions, resources and expertise.[261] AIDS policy-making and AIDS research are linked and operate hand in hand with one another, a position that was borne out across many discussions I had with both researchers and policy makers. In the words of a policy maker at Malawi's National AIDS Commission (NAC): "[Policy and research…] is a constant back and forth. Back and forth."[262]

In its focus on boundaries and translation of knowledge, this chapter views evidence-making as boundary work (Gieryn 1999), where authority or legitimacy are assigned to a speaker or knowledge claim based on cultural conventions shared by the audience; these conventions determine what falls inside or outside the bounds of authoritative knowledge. Those engaged in knowledge production practice boundary work when their positions are challenged by counter-evidence by asserting particular kinds of expertise that give them sole ownership or sovereignty over some part of social reality or body of knowledge. Returning to Bourdieu's conception of the social space, actors enact not only knowledge but interests in relation to other occupants of the field. Evidence is the foundation of these struggles to maintain or reproduce boundaries; performances of knowledge seek to protect and police the boundaries of "good"

[261] I view policy as the link between research and governing life (knowledge and power) in the transnational social field of AIDS knowledge production and intervention.
[262] Interview, NAC; April 28, 2008.

evidence. For example, when a member of a community based organization claims, in response to policy makers' suggestion that condom use is increasing in rural Malawi, "No! This is wrong! Children are picking [taking] those condoms from kiosks and using them to make balloon-toys; this depletes the stores,"[263] he is doing a number of things simultaneously: contesting the validity of the claim and its evidence, asserting his dominion of expertise over the sphere of "condom use in rural Malawi" and implying that more material resources (condoms) should be distributed in his area.

Ethnographic Sites

The sites discussed in this chapter and in Chapter Five are conceptualized as "stages," or places where performances of knowledge take place: they are comprised of scripts, props, leading actors and supporting actors. Evidence is viewed as an artifact or outcome of social processes that manifests itself in spoken claims, written claims, tacit claims or silent claims (in the case of no evidence or denial of evidence). The bulk of ethnographic data comes from five conferences where AIDS research findings were disseminated to audiences.[264] Other data come from Malawi and international AIDS policy documents, programs and information packets from the conferences and interviews and archived documents at district health offices in Malawi and in the Malawi National Archives (MNA).

[263] Field notes, NAC Zonal Research Dissemination Meeting, Mzuzu; October 22, 2008.
[264] These were: the Union of African Population Scientists conference in Arusha, Tanzania (December 10-14, 2007); the Annual Review of the National HIV and AIDS response in Lilongwe, Malawi (October 1-3, 2007); the first annual National AIDS Commission (NAC) Zonal Quarterly Review and Dissemination Workshops in Mzuzu, Malawi (October 22-23, 2008); the 11th Annual Malawi College of Medicine (COM) research dissemination conference in Blantyre, Malawi (November 24, 2007); and the 2008 Malawi National Research Council (NRC) meeting in Lilongwe, Malawi (March 11-14, 2008).

Cultural Evidence Case Study Claims

Even amid increasing valorization of numerical evidence, "culture" acts as a main form of evidence that justifies investments in research or interventions meant to mitigate the spread of the AIDS epidemic. Employing case studies drawn from participant observation of conferences and other forums where knowledge about AIDS is negotiated, this section shows the diversity of functions that the term "culture" can play in such discourse and illustrates ways in which certain actors capitalize on its unintelligibility to further their interests. Later, I will suggest that even as culture is repeatedly validated as evidence for knowledge claims, it is, in actuality, a blackbox of evidence across multiple knowledge communities.

Sexual and other practices and traditions coded as cultural became, first in the context of the burgeoning epidemic among gay men in the 1980s US, and, later, in the context of African AIDS, associated strongly with risk. Both the "perverse" practices of 1980s-era homosexual men—barebacking or using amyl nitrate poppers to enhance sexual pleasure[265]**--**and the "backwards" traditional practices[266] of sub-Saharan Africans were named as risk factors for HIV and this designation became evidence that directed flows of resources, social stigmas and interventions. As many have demonstrated (Goffman 1986, Treichler 1992, Sontag 2001), the assignment of risk, danger or disease to subcultural groups has devastating consequences for subjects who comprise these "risk groups" as they navigate fearful, judgmental societies that erect boundaries between healthy and diseased citizens. At the same time that expert discourse has framed "culture"

[265] See early medical reports of the American epidemic in Gottlieb *et al* 1981, Goedert *et al* 1982 and Shearer *et al.* 1982.
[266] Since democratization, media in Malawi often represent AIDS or human rights interventions as "against culture." For example, one particularly polemic newspaper article claims: "Human rights organizations have mushroomed in the country… and started suppressing the cultural riches" and that "organizations have sprung up to use HIV/AIDS as a weapon against culture" (Masingati 1998).

as inherent to or naturalized in these risky subjects, it has also centered its interventions on changing culture.[267] Increased attention to this object has framed it as either good or harmful, healthy or unhealthy; AIDS interventions have sought to eradicate or instrumentalize pieces of culture toward ends such as mitigating the spread of AIDS or promoting national development (Taylor 2007, Widlok 2008, Peters *et al.* 2010).[268] In Malawi, the National AIDS Framework (NAF) names cultural practices as one of the drivers of the epidemic (NAC 2009).

Precisely because culture is imagined as already *different* from or as *outside* of normative selves or social groups, it is opaque to those who research and intervene into the AIDS epidemic; culture must be translated to audiences who are interested in understanding its relationship to the epidemic. Whether plucked from the dark corner of a San Francisco bathhouse or from an all-night initiation ceremony for young boys in rural Malawi, this translation brings culture from the outside in. One major site of this translation is the conference setting. In forums comprised of actors ranging from academic researchers, NGO staff, community members or stakeholders, policy makers and funders, knowledge claims are sent out into the world to be validated or challenged by empowered audiences. "Culture" also operates more subtly in less visible venues than the conference—meetings, policy forums, documents and media.

In this section, I analyze the ways that culture is conceptualized, discussed and circulates as a category of evidence for claims made at national, local and international AIDS research conferences, meetings and in published documents. After illustrating how,

[267] This ambivalence—the instrumentalization of culture as both risky and protective—mirrors the biomedical discourse on Africa in the mid-twentieth century: Christian missionaries and others viewed the primitiveness of African societies as a factor predisposing them to disease while another explanation saw deculturation resulting from urbanization, migrant labor and industrialization as causative of disease (Vaughan 1991:201-202).

[268] Peters and colleagues (2010) suggest, for example, that the media "obsessively" recounts cultural practices that may be only periodically or infrequently performed.

when, why and by whom culture is enlisted into discussions in these forums, I suggest that the signifier "culture" is empty and magnetic; it waits to be filled with the capricious and diverse intentions of interested actors. It is, in fact, by deploying the term or its corollaries at strategic moments that actors lend legitimacy to their knowledge claims or arguments. Those who purport to translate culture in conference forums capitalize on culture's persistent untranslatability and cultures' stubborn incommensurability (Povinelli 2001). Below I present case studies in the form of claims that are representative of patterned claims made about the AIDS epidemic in Malawi. Each of these claims plays an important role in furthering positions and interests and maintaining boundaries of knowledge valued by actors who operationalize them.

Claim: Cultural Practices are Fueling Malawi's Epidemic

In June 2008 at an upscale lakeside lodge in central Malawi at mid-afternoon, a group of six people (an American epidemologist-consultant hired to head an evaluation of prevention strategies in Malawi, a Malawian co-consultant affiliated with the University of Malawi, American graduate students in biology, demography, anthropology and sociology) sat around a table.[269] Richard Castell,[270] the American epidemiologist, asked questions about their research: "I have heard from chiefs that people infected with AIDS are going out to infect others. Is this true, do you think?" "We've heard some things about cultural practices exacerbating AIDS risk. Are they?" He sought evidence that he would use in his analysis of ongoing prevention interventions in Malawi. A "yes" answer would serve as evidence to further his claim that "Cultural practices are fueling Malawi's

[269] Field notes, meeting; July 14, 2008.
[270] Pseudonym.

AIDS epidemic," while a "no" answer would be evidence that this statement was false. The participants had different interpretations of the term "culture." Important here were the backgrounds of the interlocutors, particularly their proximity to what was being imagined as culture or cultural practices. The standard anthropological answer: cultural practices are, of course, widespread, but the real issue may be governmental and non-governmental efforts to focus on culture as a way to blame AIDS victims for their own failed interventions. Though endorsed by the majority of those seated around the table, this claim failed to find legitimacy or to be validated for further use.

Immediately following this claim that presumed a critical anthropological definition of culture, the Malawian consultant, Blessings Chimanda,[271] counter-argued that there was significant evidence that cultural practices are fueling Malawi's epidemic. When he was asked to cite this evidence specifically, Chimanda authoritatively suggested that a "number of studies have been done."[272] Positioned as he was as an expert hired specifically for his expertise on matters such as this (and recommended by other expatriate researchers in the epidemiologist's network) and as someone in much closer proximity to Malawian culture than the American researchers at the table, Blessings' evidence attained a truth threshold.

The validation of this evidence eclipsed some forms of evidence even as it tacitly enlisted others; the evidence had a genealogy beyond its current form. For example, one source of evidence cited by Chimanda—a survey of cultural practices conducted in

[271] Pseudonym.
[272] Later, Blessings referred to two studies on cultural practices in 2005-2006, overseen by National AIDS Commission (NAC) and the Malawi Human Rights Commission (MHRC). He also mentioned a 1996 study completed by the Salvation Army in one district of Malawi.

2005—interviewed a wide spectrum of "guardians of culture,"[273] asking them questions that presupposed culture as "risky" and assumed a definition of "culture" as tied to traditional or archaic practices, instead of as a diffused and dynamic object. Further, the practices addressed by the survey were decontextualized and parsed from local settings; the impossibility of researchers observing or participating in rituals or circumcision ceremonies meant that information provided by respondents was "second-hand" and, in cases where respondents protected secret knowledge, partial.[274] While rural Malawians indicate that HIV infection from cultural practices is not their main concern,[275] these kinds of word-of-mouth evidence are devalued by two major criteria: 1) They are not solicited via methods validated by the audience for these claims (surveys or focus groups that can elicit statistics or numbers to target interventions); 2) The persons making such claims are, in contrast to the local expert, *too local* to provide good evidence. If villagers' knowledge about AIDS risk was accurate, they would not be in the midst of an AIDS epidemic and consultants would not be called in to provide expertise. The claims they make outside the space of research are not considered good evidence.

In making his counter-argument, Blessings used culture as a "distancing" tool by drawing not only on commissioned research studies, but also pre-circulating stereotypes and assumptions that underlie and are reinforced by research studies. His evidence was bolstered, instead of destabilized, by arguments made by speakers "distant" from Malawi; he is, after all, a "local" consultant. Although one might assume that Blessings' interest in a claim that cultural practices are fueling the epidemic is motivated by potential future

[273] This study took place from June-August 2005, implemented by National AIDS Commission in collaboration with an American research project. The cultural guardians interviewed included male and female traditional initiators, village birth attendants, chiefs, and traditional dancers. Individual and focus group interviews followed pre-established interview guides.
[274] Field notes, fieldwork with 2005 NAC Cultural Practices study; June-August 2005.
[275] Field notes; 2007-2008. A forthcoming paper by Watkins and Poulin suggests that rural Malawians identify "men with money" as a major HIV risk; these men are said to spend their money on prostitutes and beer.

financial gain (through employment on studies regarding cultural practices), the relatively small financial investment in this research area makes this unlikely. Nonetheless, he draws boundaries around his knowledge about culture by excluding not only distant (foreigners) but too proximate knowledge and epistemologies (villagers who are authoritatively "spoken for" by previous research findings).

The exchange discussed here was a platform upon which a future, more influential performance of knowledge was built. Five months later, the findings and results of the consultancy were presented to two audiences by two different actors: 1) To a Malawian audience of policy makers, stakeholders and government officials by Blessings; 2) To a regional audience by a Malawian researcher not involved with the consultancy.[276] Richard gave Blessings a skeleton of slides and graphs assembled from their joint findings about key drivers of Malawi's epidemic. However, Richard later learned that in designing the Powerpoint presentation for Malawian audiences, Blessings had "filled in the blanks" by misrepresenting the "actual data." Blessings, for example, identified "culturally accepted" intergenerational sex as a key driver of the epidemic; the actual statistics indicate this is overstated. Richard emailed another American researcher, who corroborated his observation of the discrepancies between the "hard data" and their "downstream" performance. He suggested: "When it comes down to mismatches between what the data say and what the conventional wisdom is (or what Blessings believes), *the data lose*." Even as it misrepresented the "good evidence" amassed about key drivers of the epidemic, Blessings' presentation succeeded: it was later disseminated to another Malawian audience of stakeholders by another Malawian researcher. His textual

[276] Research notes, e-mail correspondence; October 2008 and January 2009.

references to "conventional wisdom"[277] aligned with cultural scripts/accepted knowledge that circulated in his internal audience and his position within a network of Malawian experts on the epidemic assigned him authority. His insertion of interpretations "around" and "between" the graphs and tables point to his particular investments and interests in his claim(s)—rooted in culture as evidence.

Claim: Unhealthy Cultural Practices Should Be Changed or Eradicated.

Training manuals seek to teach local people how to positively change negative cultural practices. Informants involved in AIDS research, intervention and policy-making about the "culture" problem often referred to manuals such as "Communicating Cultural Change to Traditional Leaders" (Salvation Army 2005), to validate their information.[278] Such manuals are drawn on as one piece of evidence that cultural practices pose risks for HIV transmission by policy makers and Malawian researchers when writing research proposals.[279] The manual's audience encompasses cultural guardians themselves, trainers, policy makers and researchers. It included images meant to represent traditional healers, circumcisers, traditional birth attendants and chiefs. Often, these images depicted these individuals in the center of a group of villagers, armed with a clipboard and marker—ostensibly to teach them about risky versus good culture.

The objective of the manual is clearly stated: "To reduce the risk of HIV/AIDS transmission through cultural practices by supporting facilitators to work with traditional leaders on cultural change." The manual provides district officials with a step-by-step

[277] Watkins (2004) points to consequences that can occur when researchers rely too much on "conventional wisdom" about the epidemic in Malawi. She challenges the accepted knowledge that Malawians are in denial, ignorant or silent about AIDS.
[278] Interview, HIV/AIDS Programming Officer for a large NGO, Lilongwe; September 23, 2007 and interview, National AIDS Commission (NAC) staff member, Lilongwe; April 28, 2008.
[279] Field notes, meeting between Malawian social scientists at Centre for Social Research (CSR), Zomba; October 23, 2007.

chronology that begins with how to build relationships with communities and progresses to how to initiate positive cultural change. The pronouns used by the authors of the document enable their interventions into "bad culture" by creating a distance between communities and interventionists. In step one, the manual asked interventionists, "What would *we* like *them* to do?" A central problem is that this question assumes culture is uniform in rural areas and villages in Malawi (2005:2-3).

Manuals like this one are rooted in evidence that declares villagers to be different from researchers, trainers or interventionists even as they ignore other kinds of evidence. In a series of interviews I conducted with rural dwellers in the sample populations of my case study projects, I asked what "culture" meant to them. I began by asking what it meant to be Yao, Tumbuka or Chewa in Malawi. From this point, I moved further afield and asked people to describe what "Malawian-ness" is (often asking them to juxtapose it with Zambian-ness or Zimbabwean-ness). People responded that being Malawian meant having "our culture" or *chikalidwe chao*; this phrase can most accurately be translated as something like "having proper, Malawian comportment" or "being a good woman/man/chief/child," or "behaving well."[280] *Chikalidwe* is tacit knowledge, a composite ideal structure and practice. Yet, as in the case of the research studies cited by Blessings above, interventions into cultural practices presume that culture is a feature of practices or selves that can be isolated and changed or eradicated. Appendix 2 of the same manual, for example, provides a table that relies on this premise: its first column lists "high risk cultural practices" and the second column lists corollary "modified practices" (2005:35).

[280] Interviews, Salima and Balaka Districts; January-March 2008 and July-August 2008.

The evidence upon which this manual attains legitimacy is also bolstered by a number of "intertexts"—circulating national discourses that align with claims of risky culture. A report by the Malawi Human Rights Commission (MHRC) suggests that "cultural practices infringe on the human rights of individuals and groups of people" and that "some elements of culture may be obstacles to development" (2005:7). This claim is challenged when put into conversation with my rural respondents' views that human rights threatened *chikalidwe*; they suggested that "bad" people now do as they wish in the village and enjoy protection under the sign of "human rights."[281] These responses may view the past—"before human rights *yabwera ku Malawi* [arrived in Malawi]"—nostalgically. However, considering human rights as cultural practices themselves that intersect with other kinds of cultural norms and practices can explain why informants view them as contributing to a loosening of sexual or marital norms and a relaxation of cultural rules that govern the behavior of men, *afisi* [witches], young people or women. Claims that seek to change traditional, "unhealthy" culture rest on evidence that culture in Malawi is confined to villages, dangerous and backwards. This evidence, however, is produced and legitimated—made—when claims are presented to audiences by those with interests in preserving boundaries around local expertise, initiating more research in rural areas, confining pathology or sickness to "others" and upholding a national discourse of modernity and development.

[281] Interviews, rural Malawi; 2005, 2008. Cf. Interviews, Bondo District, Kenya; 2004. Many respondents claimed that witches now go unpunished because human rights disable local systems for disciplining them.

Transnational Evidence Case Study Claims

As a global problem, the AIDS pandemic has known no borders and prompted the emergence of transnational organizations—NGOs, biomedical and social scientific research groups, activists, public health organizations—that claim to protect the public good. As diverse as these institutions may be, they have adopted universal currencies and a *lingua franca* to enable exchange, conversation and planning. Though each of these organizations and institutions adopts their own conventions for producing and validating knowledge about the epidemic, they often align their activities with a set of transnational orientations that circulate widely. In this section, I show that a set of shifting but shared transnational "hot points" for AIDS research and intervention serve as a compelling and convincing form of evidence that bolsters knowledge claims. These hot points, or shared priorities or norms, are resources for researchers, activists and government officials' efforts to reduce the spread of HIV/AIDS. How do transnational norms and assumptions work as evidence?

Claim: Stigma is a Major Problem in Sub-Saharan Africa

In the spacious office of the modern building of a major NGO in Lilongwe, the HIV/AIDS mainstreaming officer, a Zimbabwean called Chuma Chibanza[282] sat behind a large desk. He was in Malawi for a few months on a contract to evaluate how effectively this NGO had "mainstreamed" AIDS into their activities. He frequently traveled around south and eastern Africa and his exposure to AIDS conferences, meetings and interventions across borders has socialized him into the shared language and priorities of

[282] Pseudonym.

the global AIDS effort. Like many of the elite experts described in Chapter One, Chibanza is an "AIDS cosmopolitan." Many of the people he works with in Malawi are expatriates; his simultaneous occupation of a cosmopolitan (widely traveled in Africa) and local (African) role enhances the credibility of claims he makes about stigma or other AIDS-related issues in Malawi. He foregrounds this: "I have seen a lot, in terms of AIDS." Chuma spent much of his time focusing on drawing awareness to stigma in Malawi and abroad. Emphasizing the degree of the problem, he said: "In some communities, I have observed around the waterhole…[people] refuse to help this [HIV positive] woman take a bucket [put a bucket] of water on her head because they think if they touch her, they get infected. This is a lack of appropriate information." This story about the woman at the water hole circulates widely;[283] filling out discussions of stigma and often serving as evidence that stigma still operates perniciously in rural areas.

Stigma is an important term that serves a translational function in social groups composed of diverse individuals as an object to "latch on" to, a familiar conversational signpost. Introduced in the mid-1980s to international AIDS discussions,[284] Erving Goffman (1963) first suggested that societies conceptualize "undesirable differences," markers or traits based on shared definitions of difference or deviance. Goffman's original elaboration viewed stigma as very much the result of social, structural forces whereas much of the literature and interventions in the realm of AIDS-related stigma conceptualizes and circulates a very different notion. Public health and human rights

[283] For example, my junior and senior undergraduates at the University of Malawi mentioned the same story of the woman at the waterhole during a class in which we were discussing Goffman's work in the context of the AIDS epidemic in Malawi.
[284] Though African AIDS literature and policy did not frequently invoke stigma from 1980-1985, by the mid-1980s the term proliferated. Some suggest that this may be a result of white gay activists taking up positions as HIV program managers in Africa and Asia in the mid-80s (Nguyen 2005) and transporting their ideas with them. Specifically, this translation from gay male experience with AIDS to "universal" AIDS accompanied the silence=shame formulation, where being silent or "not out" causes others to discriminate against and stigmatize you. (Conversations with Adia Benton were formative of the ideas I present here).

interventions have viewed stigma as a negative individual behavior or assessment often rooted in emotions or in misinformation such as about how HIV is spread.[285] This has worked to produce stigma as something *in* individuals as opposed to something attached to them (Parker and Aggleton 2003:15). Yet despite the wide circulation of the term and the fact that it is often a foundation for interventions and policy,[286] few have tested its conceptual adequacy or interrogated it for validity (2003:13). In Chuma's case, the sheer ubiquity of the term served as evidence to accentuate the officer's claim. He could draw on countless documents, policies and NGO-initiated research studies on the topic to "prove" that stigma is a pressing and entrenched problem across Africa—and, in fact, across the spectrum of other AIDS-affected developing nations. In most venues, this evidence suffices simply because it translates across sectors and because of the sheer quantity of knowledge about stigma in existence.

Stigma as Short-Hand

Central to the circulation of evidence is its rootedness in shared scripts that are widely available. For example, newspapers and radio programs focused ample attention on stigma, usually employing a pedagogical tone. An article that bemoaned the low percentage of people who take their children for HIV tests suggested that the main reason for this is "fear of stigma" and the fallacious assumption that "if the child is positive, so too must be the parents."[287] Newspapers often covered campaigns to reduce stigma and discrimination against positive individuals[288] but also claimed that stigma is "on the rise"

[285] This emphasis on the individual resonates with the fact that stigma is most commonly discussed in academic articles in psychology and psychiatry sources, disciplines rooted in an individualist and behaviorist paradigm.
[286] Stigma was the theme for the 2002-2003 World AIDS Campaign.
[287] Matola 2007.
[288] Gausi 2007.

and suggested that stigma creates a "culture of secrecy, silence, ignorance, blame, shame and victimization."[289]

The word "stigma" was a relatively common fixture of printed abstracts circulated at AIDS conferences in Malawi, indicating its central position as a concern for researchers and policy-makers. Stigma has had a long life and persists as a legitimate object of study. The term is used in multiple ways, e.g.:[290]

> -Stigma limits the uptake of Preventing Mother to Child Transmission (PMTCT) services among women.
> -Psychosocial barriers to VCT include long distances, stigma, lack of privacy, and lack of information.
> -Stigma is still very strong, especially among children in school.
> -Fear of stigma is stronger than fear of death.
> -Due to stigma, many HIV positive persons may not participate in electoral processes such as voting.
> -Stigma clouds the causes of illness and death.

As these many usages illustrate, stigma explains behaviors from low voter turnout to low rates of adoption of PMTCT. It is a taken-for-granted negative force, often imputed with an agency all its own and it is rooted in misinformation and irrational fear. Even if the term itself is mired in confusion and linked to multiple, sometimes competing meanings, stigma and other such terms have important explanatory functions in forums like conferences, publications or policy making sessions. Acronyms and words like stigma are short-hand descriptors—building blocks that are the foundation of a diverse, eclectic and scattered social group. A researcher from the US, an activist from South Africa or the leader of a faith-based organization (FBO) in Malawi recognize the term stigma. Its pronouncement immediately generates nodding of heads; negative examples of its local operation are rarely contested—the tight link between humanitarianism and a universal

[289] CHRR 2008.
[290] Field notes; 2007, 2008, 2009.

conception of a dignified human make questioning the severity of stigma an almost impossible position. Yet, even as the term serves to knit together diverse individuals and travel seamlessly from one sphere to another, its content is not unitary.

In fact, I suggest that the deployment of the term stigma may do more to index the apparent brutal or uncivilized nature of people who stigmatize their HIV positive neighbors than to rectify an existing social problem. At an international conference on AIDS, a paper by a Zambian researcher probed the meanings of voluntarism in HIV testing in Zambia; he peppered his presentation with comments such as: " In some places a person with HIV is seen as… *I don't know what*," a statement that paints those who hold this view as ignorant. In many conferences and policy forums, there was keen attention to the dangers and horrors of stigma. At one point, a presenter suggested there were local terms for stigma: "There's a special term I got in the field but I can't remember it, one of those local languages."[291] Whether or not this statement is accurate (most Malawians use the English word to refer to "stigma"), it functions to associate stigma with extreme and dangerous negative sentiments toward HIV positive people and to "exoticize" stigma as a "village problem." Further, it solidifies the boundaries of the transnational research community by preserving one among many objects (stigma) that they convene to discuss and intervene on.

Claim: AIDS Interventions Should Focus on Men who Have Sex with Men (MSM)

Forty people attended the Mzuzu (northern region) National AIDS Commission (NAC) Quarterly Review and Dissemination Workshop.[292] Presenters included the NAC

[291] Field notes, Union of African Population Scientists (UAPS) Annual Meeting, Arusha, Tanzania; December 15, 2008.
[292] Field notes, Dissemination Meeting; October 22, 2008.

research officer and AIDS researchers (Malawian, American, Canadian). The audience was "local stakeholders"—community based organizations (CBOs), the District AIDS Commission (DAC), monitoring and evaluation (M & E) officers, members of district assemblies and members of AIDS support groups—in line with government's increasing emphasis on the wide dissemination of research findings (see Chapter Five).

At the workshop, a researcher-activist, Gift Trapence, the co-founder of a human rights NGO, presented research findings from a cross-sectional study of MSM sexual behaviors in Malawi. From the initial moment that he projected the title slide of his presentation, the audience responded with chuckles. When I asked a member of an AIDS prevention CBO who sat next to me why people were chuckling, he said, "There are none of these MSM here [in Malawi]!" This claim directly contradicted Gift's: "MSM are more significant in our country's epidemic than ever imagined."

The evidence Gift drew on to support his claim that MSM are an important invisible AIDS risk group encompassed statistical findings from an exploratory study and comparative data from other countries with high HIV-prevalence. Gift introduced his project by framing it as a contribution to the growing body of literature on MSM in sub-Saharan Africa. Aware that his audience was unfamiliar with the acronym, he explicitly defined "MSM."[293]

Next, Gift presented the statistical evidence for his claim. Explaining that his study was part of a larger four-country study, he pointed to the numbers on a projected Powerpoint slide: HIV seroprevalence for MSM is 21.0%. Complementing this figure

[293] The term loosely unifies men in global communities ranging from sex workers to African-American men on the down low ("DL") to men in developing nations who may be heterosexually married and engage in sexual behaviors with other males on the side—its reference to the behavior of "sex" rather than to the identity ("gay," "bisexual" or "queer") attempts to create a risk group without assuming certain identifications with or commitments to, for example, gay rights projects (UNAIDS 2010).

with actual numbers and the confidence interval (CI) (42/200, 95% CI) provided credibility for the statistical claim. These numbers were nested in others: new infections in MSM comprise 10-15% of the global AIDS burden. Numbers were also used to represent the gravity of the situation for MSM in Malawi. For example, low access of MSM to health care (10% had disclosed to a health professional that they were MSM) and high perception of AIDS as the main health risk for MSM were evidence that interventions should be targeted at this risk group. Finally, statistics indicated that many MSM were often beaten up, raped or afraid of "coming out." Taken together, all of these numbers were evidence that MSM prevalence in Malawi is higher than the national prevalence and that stigma against MSM leads to their invisibility. Gift called for sensitization of policy makers, HIV/AIDS key players and other stakeholders, for specific interventions and for research to explore sexual behaviors and social stigma. In addition to the statistics' contextualization in a larger regional literature on MSM and AIDS in sub-Saharan Africa, Gift made reference to the transnational when he described Malawi as "behind" other countries in recognizing gay and human rights; Gift sought to motivate his audience to take steps to "close this gap."

Gift's evidence was invalidated by this audience; it failed. First, the degree of departure of his claim from prior, accepted knowledge held in common by audience members was significant. When Gift described the main avenue of transmission for MSM (anal sex), the audience responded with shock and moral outrage: people described anal sex as "unnatural" and expressed disgust. Gift's co-presenter who had remained quiet during the beginning of the presentation confronted the audience's disgust: "You know, in Tanzania, women prefer anal sex, and in other cultures, too. Even in Arab countries,

sometimes women want to preserve their virginity and so they decide to do it [anal sex] instead. It's common…" His outburst was met with laughter. "That doesn't happen here!" a woman shouted from the back. While the audience members persisted in establishing Malawi's particularity as a "decent" nation where homosexuality does not exist, the presenters aimed to include Malawi in a larger transnational society where the issue of MSM is at the forefront of efforts to fight the spread of AIDS. In this case, as in the one above, a speaker earns credibility by using the transnational to temporalize Malawi as "backwards" or out of sync with the rest of the (more developed) world.

Despite its combination of powerful statistical and transnational evidence, Gift's claim was not favorably received. This evidence challenged powerful moral convictions that acted as evidence for a counter claim that MSM do not exist in Malawi. In making this moral evidence, audience members employed two tactics: 1) Attacking the credibility or motives of the researcher; 2) Questioning the evidence itself. Gift was asked twice to disclose his sexual orientation and accused of harboring a hidden political "mission." Attacking the evidence, one man called the presentation "hearsay" and asked, "How can you put this on paper? What is your *proof*?" The suggestion that the evidence presented did not constitute *proof* unified the multiple audience responses to the study findings; statistics and comparative transnational evidence did not effectively convince the audience of local stakeholders. Their claims that they had never "seen or heard of" a man who has sex with men and their framing of same-sex liaisons as inimical to Malawian religious and moral convictions defeated Gift's knowledge claim. Although this presentation generated the most debate in the conference venue's halls later that day, it was also the most conclusively invalidated by the audience. The cascade of numbers and

statistics in Gift's presentation did not "translate" to an audience whose primary evaluation of the claim being made rested on moral evidence.

Despite this reception, a National AIDS Commission (NAC) officer suggested that Gift seek NAC monies to do a larger study. However, later, over lunch, Gift said that NAC was historically very unsupportive of their efforts to garner global funds for research and HIV prevention. Though his NGO submitted a proposal for a larger study, he claimed that "NAC has been sitting on it for three years now." Despite the performance of support in the conference room earlier, he doubted that NAC would allocate him funds: "Some policy makers and Malawians are very traditional and stubborn." In order to conduct the pilot study, he explained, he relied heavily on the credibility of their international partner (a large university based in the US). While NAC typically values statistical evidence and endorses transnational standards and priorities, evidence has a life of its own and may be "localized" in unexpected ways.[294]

Importantly, however, the rejection of Gift's evidence gave the same knowledge claim legitimacy on other "stages" in the social field of AIDS research and policy. Though his evidence had also been rejected a few months earlier at a similar conference in Lilongwe, Malawi's capital city, he also said that this paper "has really helped me move around!" Because international human rights and LGBT organizations are most interested in funding organizations, research and interventions in places where rights are threatened or not sufficiently protected, Gift and MSM in Malawi attained a certain transnational legitimacy through local victimage. Gift has traveled to conferences and workshops in Mexico, Geneva, Zambia, South Africa and so on to present his findings.

[294] Funding for AIDS activities within Malawi is pooled at NAC. This centralized structure permits NAC some ability to publicly endorse and prioritize transnational objectives (such as MSM), but to retain decision-making power internally.

Further, because MSM is a "hot topic" in international AIDS research at the moment, Gift is frequently in the northeast US for trainings as part of a large-scale study of MSM in multiple African countries based at a large American university. Finally, the recent "gay marriage affair"[295] in Malawi saw CEDEP playing a prominent role in securing the release of the two accused men from prison and gaining asylum for the female-identified partner. Since then, Gift has been interviewed by international news outlets ranging from the *Mail and Guardian* (South Africa) to the *New York Times* to BBC Radio. He said: "After all this publicity, NAC can no longer ignore our evidence! They have to pay attention!"[296]

Numerical Evidence Case Study Claims

As described in the prior chapter, numbers and statistics are a main way that social scientists "see" AIDS in Africa; because they "travel" so easily, numbers are also the lens through which wider audiences—policy makers, consumers of media reports and other researchers—glimpse localized health and social effects of the epidemic. Many international social science research projects do not claim that their findings represent a larger national reality, nor do they seek to intervene into social problems. Other actors instead rely on their findings to produce knowledge claims in the fields of policy design and global health—fields with more explicit stakes in representing and intervening in the AIDS epidemic in southern Africa. The numbers thus become the foundation for proposals for funding programs that attempt to alter behavior by transforming

[295] In December 2009, two men (one who identifies as a woman) were arrested after holding a traditional engagement ceremony. In May 2010, they were sentenced to 14 years hard labor under a colonial penal code. Soon after, the men were freed following UN Secretary General Ban Ki Moon's visit with Malawian president Bingu wa Mutharika.
[296] Field notes, Baltimore; December 2010.

individuals' perceptions and values. Since the advent of the AIDS epidemic, numbers are the primary and "standard" form of evidence used to craft interventions and to measure progress.[297] Chapter Three explored how numbers and statistics are socially produced within everyday practices. Building on this, this section examines how numbers become evidence when they ground knowledge claims about the epidemic; these knowledge claims often have an interested, future-directed stake in reforming, improving or intervening into social realities and human behaviors.

Claim: Couples HIV Testing Should be Scaled Up in Malawi

During the meeting of the Union of African Population Scientists' annual conference at the International Criminal Court (ICC) complex in Arusha, Tanzania in December 2007, an American sociologist presented findings from a study of HIV voluntary counseling and testing (VCT) uptake among married couples to a diverse international audience.[298] Four of the audience members wore headphones to follow the English presentations translated into French in real time; twenty did not. Kate Brown,[299] an American researcher affiliated with a Malawi-based institution, argued in her paper that VCT provides many benefits in terms of decision-making process and information sharing between partners and should be more widely implemented in Malawi. Her conclusions were based on 45 qualitative, in-depth interviews with married couples; questions covered topics such as women's marital power, married men's and women's

[297] Thévenot suggests, first, that standards have become a fixture of the lives of all living entities and, second, that they draw together the relationship between regulation and objectivity. He describes how a set of cancer guidelines called "Standards, Options, and Recommendations, (SOR)" are ranked according to their varying "degrees of evidence," a classification that relies on perceived differences in the strength of the evidence. The highest degree is evidence based on randomized clinical trials. This kind of evidence necessitates more costly investments in negotiation and procedure to reach agreement about selected properties, benchmarks and tests that will define a standard (2009:794). This chapter presumes numbers or statistical evidence is the highest degree evidence in the policy-research nexus.
[298] Field notes; December 15, 2007.
[299] Pseudonym.

decision making and use of VCT within couples. Brown presented her findings using Powerpoint slides; notably, however, she deviated from the normal presentational style by including a number of photos of the couples she interviewed and Malawian village life in the presentation. She also speckled the slides with some direct excerpts from her interviews. The question and answer period indicated that although the audience received the paper well, this paper could only ever be a useful *complement* to real knowledge or true science (informed by statistical analyses and demographic variables, t tests and so on). The audience considered Brown's paper as "Not science… but interesting."[300] This orientation to the evidence being presented establishes boundaries. The audience members adopted and interestedly performed an identity as "scientists;" qualitative evidence lies outside the kinds of evidence they can accept.

Although her methods and the packaging of her findings (i.e. use of photos and "word of mouth" quotations) might have legitimated her findings for a different audience (sociologists, anthropologists), the audience of mostly demographers devalued these aspects of her presentation. A Malawian sociologist described qualitative research in Malawi as "nothing more than a side dish" to more quantitative studies; though the findings of qualitative research can "enhance" or "complement" quantitative studies, they can never attain validity on their own.[301] Although the quality of a number of quantitative presentations at this same conference was undeniably "bad;" their failure was immediately recuperated by audience suggestions that the calculations in the paper

[300] Literary critic Sianne Ngai's (2008) probing of the promiscuous circulation of "interesting" as an aesthetic or critical evaluation seems pertinent here. Namely, her description of the interesting as "aesthetic without content" (781) whose pronouncement merely works to open a horizon for further discussion of the object in question parallels the evaluations of presentations or research findings at conferences as "interesting." Though Ngai focuses her analysis on art, literary, or cultural criticism, as cultural artifacts, papers or research findings, too, are evaluated against standards shared by a community or audience of critics. While quantitative research findings and presentations assume a factual finality, qualitative ("interesting") counterparts raise more questions than they answer and "anything can presumably count as evidence at one moment or another" (781).

[301] Interview; March 11, 2008.

merely did not compute and that they were easily fixed. In each of these cases, the audience read the failed paper as just in need of improvement, better sampling, a different kind of statistical test, and etc… Other times, the response was that the study should be "scaled up" or "sampled alternatively." Qualitative analyses that were interesting and well-organized, on the other hand, were dismissed as not generalizable.

The assessment of Kate's evidence as "merely interesting" emerges from a larger matrix of assumptions that underlie authoritative knowledge. Normative definitions of science and research as technical, empirical and evidence based, for example, solidify and authorize certain knowledge claims and not others. An administrator at the University of Malawi suggested: "I do value the kind of work being done by sociologists and anthropologists, but I just think it must benefit or complement more scientific work. Who is to say, for example, with your [meaning my] study that you wouldn't find different things next time? Or if someone else did the same study, they would find different things to you?"[302] His comments privilege replicability as a characteristic of evidence; not only must research itself be replicable, genre and performances of knowledge are also expected to be consistent across contexts. For this administrator, ethnographic methods and what he later termed "meta-research" are outside the boundaries of even the toolkit associated with the descriptor "qualitative" in Malawi: surveys, focus groups or knowledge, attitudes, practices (KAP) rapid assessments. During an open session at the National Research Council research dissemination meetings in 2008,[303] a Malawian qualitative researcher in the audience publicly opined that Malawi is in a "research straitjacket:" "I think we need some more bizarre, some more adventurous

[302] Field notes; March 12, 2008.
[303] Ibid.

research." The audience members (mostly scientists) responded ambivalently and the chair of the panel attacked the speaker by calling his comments "biased." This exchange was an artifact of ongoing relations between these two individuals who placed different stakes on the outcome. While the researcher was at that moment in solidarity with and participating in ongoing faculty strikes at the university, the chair of the panel had, during the prior week, aligned himself with authorities who threatened to throw the striking faculty members in jail if they did not get back to work. Visibly annoyed with this suggestion that Malawian scientists were not innovative, he countered: "An interesting thought… But let us remember that Andy[304] here is very much a *social* scientist." In emphasizing the word "social," the chair invokes a uniquely Malawian conception of the social sciences as revolutionary and outside national interests (as, for example, historically enmeshed with social movements and the disempowered)[305] as opposed to the more conservative sciences. Investments in qualitative or quantitative research methods map onto locally circulating identities and political affiliations, informing the stakes of validating certain forms of knowledge over others.

Claim: Individuals Overestimate Their Risk of Having HIV

Four demographers published an article in the journal *AIDS and Behavior* in 2010 titled "HIV Risks Among Currently Married Couples in Rural Malawi."[306] Though performances of knowledge might be assumed to refer to spoken instances of knowledge in front of an audience, this peer-reviewed article is also a performance that includes

[304] Pseudonym.
[305] The fear of radical "social" thinking and teaching still permeates the University of Malawi (as it did in the post-independence era when former president Kamuzu Banda heavily censored the content of university lectures and curricula). In February-March 2011, the faculty of the university held protests in support of academic freedom after a political scientist was detained by the police for speaking about the recent Egyptian protests in a classroom (Email correspondence, faculty member at university; February-March 2011).
[306] Anglewicz *et al* 2010.

scripts, actors, supporting actors and audience. The claim above comes in packaging that is familiar to academic audiences: the linear, familiar and signposted cartography of the journal article.

The claim that individuals in Malawi tend to overestimate their spouse's as well as their own risk of having HIV is based on statistical and numerical evidence. However, although the researchers explain that they used population-based data from 768 married monogamous couples in a 2004 panel survey to compare respondents' perceptions of their spouses' HIV status to their spouses' actual HIV status, the evidence undergirding this claim does not come only from their analysis of their numbers; instead, it emerges also from a stockpile of past claims made about the epidemic or HIV risk perception in Malawi. This is most evident in the authors' citations that use evidence from prior research to support their own claim. They cite, for example, this previous claim:

> While the actual likelihood of infection… is approximately one in a thousand (95 % confidence interval: 0.0008-0.0015 per act of intercourse; Gray et al 2001), more than 95 % of MDICP-2004 respondents believe AIDS is "highly likely" or "certain" to be transmitted from one act of unprotected sex with an HIV-infected person (Anglewicz 2007).

First, the authors set the background for their claim that Malawians overestimate HIV risk by presenting an authoritative claim from 2001 regarding the actual likelihood of infection and, second, by introducing their own findings. After all, risk can only be *overestimated* when it differs from an accepted standard: "we…estimate the accuracy of rural Malawians in assessing the HIV status of their spouse as well as themselves" (2010:106).

The methods section speaks to the shared epistemic virtues (see Chapter Three) of its audience. Authors describe the data collection that produced the sample, how they

narrowed a larger sample and what features of the sample permitted the kinds of analyses they conducted.[307] In discussing their methods, diction and tone legitimate their presentation of evidence. For example, they do not sample helter-skelter, but rather draw a sample from a larger sample associated with a well-respected research project that has worked in Malawi for over ten years. They also provide meticulous and detailed descriptions of their data collection processes in the field. In the case of the touchy subject of HIV tests (in human subjects research), they transparently present their testing procedure, mentioning that it was implemented in line with "biomarker protocols" (citing Bignami-Van Assche *et al* 2004) and that all respondents were asked to give written consent before the tests were administered.[308]

In addition to emphasizing the credibility of their data collection methods, the authors showcase their data analysis in the "results" portion of the article. Numbers play a central role as evidence in this section. Sample characteristics are presented in textual ("HIV prevalence is 5.6 % for wives and 7.1 % for husbands") and tabular ("Descriptive statistics of spouses' characteristics and behaviors for the couples selected for the analysis are displayed in Table 2") form. Authorial claims attain a truth threshold because they are rooted in good evidence: a large sample "representative of three regions of Malawi." The authors' use of actual numbers that underlie the statistical averages points to a collection of real individuals who, in aggregate, represent the nameless, faceless sample. Tables, by presenting the numbers in neat, ordered fashion under relevant categorical columns and rows, are important props that stand in for and contain a larger,

[307] See Chapter Three for more on how research projects identify and manage a sample.
[308] See Chapter Two for a discussion of how ethical guidelines for human subjects research translate into the field.

much messier reality and serve as quick and dirty tools that readers can reference in a pinch; numbers, after all, are what matters.

As the authors begin discuss their findings, the contours of the article's audience come into focus—jargon and technical terms include particular sectors of a larger audience for this journal: "The results of the *multivariate regression analysis* allow us to identify the factors associated with believing one's spouse to be infected with HIV when, in fact, he or she is HIV negative (Table 4, left panel). Self-reported infidelity and suspected spousal infidelity stand out as the *dominant correlates* of overestimating one's spouse's HIV risk…" (2010:108, my italics). Further, audience knowledge of terms and symbols is presumed: Table 3 includes the values for a X^2 (Chi-squared) test[309] (with P-values (P>z)[310] bracketed) and a Kappa test.[311] Each of these measures is a standard that helps evidence in this particular knowledge community to attain a threshold of truth and acceptance.

The knowledge claim made in this article attains a number of criteria valued by its audience(s): good sampling techniques, large enough sample, statistical significance, application of statistical tests to ascertain validity of findings, multiple authors (including one Malawian author), ethical data collection procedures and citation of well-respected and previously accepted studies or findings. Each of these criteria is based in numbers. The evidence used by these authors propels not only a certain story-line about AIDS in Malawi, but also knowledge networks. Because it appears in a peer-reviewed journal, this

[309] A statistical procedure used with data that fall into mutually exclusive categories to see whether a variable is independent of another. For example: Are those who smoke significantly more likely to have lung cancer or is there no relationship?
[310] The p-value measures the strength of a relationship. It is the probability of obtaining a statistic at least as extreme as the one observed, assuming the null hypothesis (a general or default position such as "there is no relationship between x and y") is true. The lower the p-value, the less likely the result is if the null hypothesis is true and the more statistically significant the result is. One often rejects the null hypothesis if the p-value is less than 0.5 or 0.1 (the chance of rejecting a true hypothesis). In the example given in footnote 22, what is the extent to which the results of a chi-squared test show a statistically significant relationship between two variables?
[311] A Kappa test refers to and measures agreement between two variables; in this case, the degree of agreement among respondents.

claim becomes foundational to—as a table waiting to be set by—future knowledge claims.[312] Finally, the authors close by nodding to a necessary criteria for knowledge claims about Malawi: they must be "policy relevant" or applicable to pressing social problems.[313]

Claim: S/he is Infected With HIV

How did rural Malawians "diagnose" AIDS before HIV tests were widely available in 2005? To explore these questions, I draw on two sources: interviews with rural Malawians in 2005, interviews and discussions in Malawi in 2007-2008 and a set of "hearsay ethnography" journals written by research assistants for one case study project.[314] Researchers (above) suggest Malawians overestimate their HIV risk in "diagnosing" AIDS without a blood test. Yet, as we shall see, the evidence drawn on by rural Malawians is rooted in different sources: media, radio, campaigns and exposure to the research of case study projects to enhance its credibility.

In the hearsay ethnography journals, I examined instances where research assistants had recorded people speculating on the HIV status of relatives, friends and

[312] The article calls for specific possible research directions, for scaling up results to other African countries and for analyses that employ and improve the methodologies utilized by the authors: "The present study highlights the need not only to reconsider the counseling component of VCT programs, but also to re-orient our theoretical and conceptual models of HIV risk, as they both currently primary address *individual* behavior modification rather than couple-based protection strategies… Our findings highlight the shared HIV risk from the couple, rather than the individual, perspective when planning HIV interventions and policies in sub-Saharan African countries with generalized epidemics" (Anglewicz 2010:111).

[313] Ibid.

[314] This project hired Malawian high school graduates living in the study sample areas to write journals in which they recorded mentions of "AIDS" in their everyday social lives and networks. The collection of these journals amassed by now encompasses hundreds of distinct conversations that "overhear" thousands of rural Malawians discussing AIDS. There are some potential methodological problems with the journals: for example, pay incentives encourage productivity in production of text that may increase the probability of "cooked" (faked) data. Meanwhile, however, the presence of recurrent themes in the journals enhance their verisimilitude. Experiences recorded in these journals, incidentally, challenge international depictions of "grandparent led families," "orphans" and "widows." Despite the limitations of reading these secondhand reports as "truth," they can be read as a contingent and evolving source of stories about the epidemic in rural Malawi. The particular analysis I present here very much replicates the section of an article I co-authored with the project's researchers (Watkins, Swidler and Biruk 2010). For the remainder of this chapter, citations to the publicly accessible journals will appear as: Pseudonym IDCODE.

neighbors who were ill or have died.[315] Before expansion of HIV testing services in 2005, many Malawians diagnosed HIV by conducting "social autopsies" or discussions of the AIDS status of people around them. These autopsies are fruitful sites for exploring the ways in which people perform and evaluate evidence of infection.

Speakers consider symptoms they can "see with their own eyes" as credible initial signs of HIV infection: weight loss ("growing thinner"), weakness, sores on the body and hair loss. However, because they know that these symptoms may be symptomatic of other illnesses, they often proceed to pool evidence through conversation about the person's medical history: Did he have an STD? Was she treated for TB? Following most discussions of the physical or bodily status of some individual, speakers usually speculate on their sexual behaviors or "movements."[316] This social information is used to bolster the interpretations mobilized regarding the particular constellation of physical symptoms exhibited by an individual:

> A man told his friend who sat with him, "That lady [walking on the other side of the street] is found everywhere. I used to see her at Mzuzu, Salima, Mchinji, Kasungu, Zomba, Mangochi, Blantyre, everywhere[.] [S]he was going to these places with different men. Those days she was fat. She had to fight off the men. But now she is becoming sick, and I am sure that she has taken this HIV because her body talks…I say that the lady has got AIDS because of how she moved, I have seen her. If someone wishes to sleep with her he should know that he is making his grave."[317]

On other occasions, AIDS is inferred from a person's appearance in combination with whatever is known of his or her past, even if that does not include knowledge of sexual behavior.

[315] Nvivo 6 Coding Software.
[316] In rural Malawi, "movements" refer to sexual activities and being "movious" implies promiscuity.
[317] Anna 050330.

> I asked, "What do you think the man is suffering from?" He said, "I think I already said that the man is suffering from nothing apart from AIDS."
>
> Then I asked, "Where did he contract it?" Mr. Zex said that where he contracted this AIDS nobody knows but himself...He said that probably it was on the tea estates ... He said that on the Thyolo estates...the trees are always high and leafy and what often happens is that a man and a woman go far away from their fellow laborers for sexual intercourse and in these estates a lot of fornication happens. I exclaimed, "Indeed?"[318]

As we can see, in most cases informal diagnoses of AIDS rest on tacit knowledge, the now taken-for-granted association of HIV and AIDS with promiscuous sexual behavior and a corresponding set of physical symptoms.

In some cases, however, people attribute the legitimacy or authority of tacit knowledge to some source. These sources include: the radio, gossip and stories. For example, when one of the journalists asks his wife how she came to know that a male secondary school student is suffering from what she calls, "an unknown disease," she responds by citing the social chain through which she became privy to the information. Though this social chain may amount to little more than gossip or rumor, it grounds the piece of knowledge (that the school boy is infected with AIDS) in individuals who know or live near to the boy. We might consider this social chain alongside the one I mentioned above: academic demographers citing their peers and previous studies in a journal article:

> My wife said she learnt from her mother, who is the best friend of Mrs. Nkolokosa and she has been going to visit him to see him when he was sick. She went on saying that the patient was nearly about to die because (the wife went on saying that) her mother said that she heard from her friend Mrs. Nkolokosa saying that a patient was to die because one day the patient called/summoned his Father.[319]

People attribute knowledge they have about AIDS to sources like billboards or the radio and AIDS research projects. In a conversation among young men who speculate about their chances with a group of young women, one youth says, "but you guys we are

[318] Simon 030224.
[319] Simon 030129.

receiving HIV / AIDS messages almost, every day through radios, newspapers, drama groups, political leaders, medical personnel, chiefs and the like even research teams like LET'S CHAT team yearly they come in Black T-shirts but you can not take a lesson."[320]

The kind of diagnosis we see in the journals is a social process. Part of the ability to make a diagnosis of AIDS rests on convincing those around you that the foundation for your diagnosis is a good one. In the series of excerpts presented here, speakers legitimatize or make their knowledge authoritative by attributing to sources such as radio, to credible witnesses and by referencing facts so prevalent that they require no elaboration (cultural scripts). In this way, speakers are able to make diagnoses of individuals' serostatus without a blood test. Despite the absence of *scientific numerical* evidence, rural Malawians find their way of diagnosing or estimating the serostatus of those around them to be useful, generally "accurate" and reliable.

Conclusion

This chapter has traversed a number of sites that are knit together in the policy-research nexus: conferences, small meetings, NGO offices, academic journal articles and rural villages. Similarly, it has illustrated the astounding diversity of people who make and evaluate knowledge claims about the epidemic: researchers, villagers, expatriate consultants NGO staff members and activists. Amid this variety of people and places, evidence is enlisted into performances that seek to convince increasingly diverse audiences of the validity of certain knowledge claims. Evidence is a base for both knowledge claims and, most importantly, policy.

[320] Daniel 050305.

While many take evidence to be solid, "hard" and stable, I have argued that it is actually quite amorphous and "soft"—subject to transformations and erosion depending on its social context. Precisely because of the sheer diversity of actors brought into contact in the AIDS policy-research nexus and the social or geographic distance between them, evidence must translate to wide audiences and it must be mobile. When a speaker aims to predict the contours of his audience, the unexpected can occur. Gift Trapence, for example, presented a distilled version of his claim that MSM are a risk group in Malawi's epidemic to an audience of community based organizations but evidence that had elsewhere been deemed "good" was invalidated in this context. In the case of Blessings Chimanda's Powerpoint presentation on the findings from his consultancy, the statistical evidence remained the same—i.e. he translated the numbers correctly from the database. However, the packaging (text) via which he presented the numbers made "immutable" numbers quite mutable—his knowledge claims directly contradicted the numerical evidence but were accepted by audiences who trusted his expertise. Across each of the case studies in this chapter, evidence emerges only as an artifact of social and translational processes that underlie and direct the interested performance of knowledge to audiences. The next chapter furthers this claim by drawing out specific criteria by which audiences "measure" evidence—not only in the case studies presented here, but more generally within the policy research nexus. Further, the chapter will show how mechanisms and strategies that emphasize networking, dissemination, collaboration and knowledge-sharing as central to translation of knowledge across boundaries paradoxically work to solidify boundaries around expertise and evidence; this contributes to the production of a generally convergent rhetoric about the AIDS epidemic.

Chapter 5

"Closing the gap" Between Research and Policy

Amid social scientific knowledge production in sub-Saharan Africa, the formation of what passes as authoritative knowledge becomes the foundation for international and national policies. The policy-research nexus is a crossroads of competing and diverse claims about why AIDS continues to spread, about what is currently happening on the ground and about what should be done and why. In the last chapter, I showed that evidence is "made" in social contexts; what counts as proof of a claim for one audience may fail as proof for another. Similarly, the same evidence may act as proof for two very different claims. With so many actors and interests at the table, translation and standardization are central to effective communication and sharing of knowledge. Yet, in fact, the policy-research nexus is a place of "gaps"—between policy makers and researchers, between researchers and their research subjects and between quantitative and qualitative social scientists—that must be closed by translational strategies.

Chapter Four provided a series of case studies of knowledge claims to illustrate the social processes and boundary work that underlie the making and evaluation of evidence. This chapter aims to draw commonalities from these case studies to identify patterns common to making evidence in the policy-research nexus. These patterns act as shared guidelines that enable people from different positions in a social field to communicate effectively. The chapter maintains a focus on how actors recognize and test evidence they are presented with. First, I show that speakers, authors and audiences draw on a store of pre-validated "facts," what I term cultural scripts, that determine what it is possible or impossible to claim, and that influence what evidence will work or fail.

Secondly, the chapter suggests that designations of evidence as good or bad emerge from social relations and networks that propel knowledge forward by measuring it against shared epistemological expectations and conventions and by assigning certain speakers a degree of reliability. I show that forging new social connections and building social capital in the policy-research nexus are crucial to actors' attainment of credibility in research cultures, even as these connections may reproduce and give momentum to non-innovative and unquestioned evidence. Thirdly, I argue that recognizable performances, props and packaging effectively showcase knowledge and work to validate claims but distract audiences from their actual content: evidence. The final portion of the chapter argues that an increasing emphasis on the translation of evidence—between actors, between formats and between spaces—produces standardized vocabularies, shared rituals and the sense of "connection." However, this performance of translation and connection acts to mask disjunctures between actors and mutes or disables the disagreement and debate that are central to innovative knowledge.

Disentangling the Components of Evidence

I now elaborate three main ways in which audiences measure or assess "new" evidence in the policy research nexus. Evidence is not enlisted into knowledge claims in a vacuum: it enters an uneven and complicated social terrain where it will either be propelled forward (attain a truth threshold), delayed (subject to further evaluation or stuck until its evidence base is strengthened by other studies, e.g.) or stopped in place (deemed "bad").

Measuring New Claims Against Shared "Facts"

Across the case study claims that draw on the forms of evidence—culture, the transnational or numbers—it is clear that claims that present "new knowledge" must not deviate too far from already accepted, validated and presumed knowledge about a given topic.

I have already argued that culture is enlisted into knowledge claims to serve a distancing or differentiating function. Claims about culture and AIDS such as those in the manual discussed in Chapter Four are given momentum through reference to a larger shared imagined cultural landscape; these cultural scripts set the agenda for the kinds of claims about risky culture that it is possible to make. Further, these claims rely on crucial or crisis-words such as "need to," "must" or "difficult;" imperative diction indexes the widely assumed necessity of intervention into the problem of culture amid a raging epidemic. They announce a crisis and act as convincing packaging for knowledge claims. Across diverse realms and social fields, culture is imagined and represented as distant from the center or confined to the village. This permits speakers great latitude in performing and validating knowledge claims that fundamentally rely on the analogy between distance and difference; AIDS research and intervention assume a shared idea of culture as contained and containable to rural and remote locations. However, as this section shows, audiences assess evidence in light of the positionalities of all the interlocutors and counter-arguments present: the determination of whether evidence is good or bad always implies credibility contests (Epstein 1996, Gieryn 1999).

Shared Risk Cartographies

At the National AIDS Commission's (NAC) regional dissemination conference,[321] a collection of Malawian researchers, policy makers, stakeholders and members of CBOs or NGOs shared assumptions and drew on tacit "local knowledge" about the pockets of culture in their nation; in fact, this stockpile of implicit knowledge[322] was necessary for communication across group boundaries. For example, the representative of the National AIDS Commission (NAC), Johnson Phiri,[323] presented research findings from a recent study that focused on analyzing the social networks and exchanges that characterized social spaces where high-risk sexual practices were known to occur. The main research sites were rest houses known to be centers of sex work, bars where sexual relationships were often initiated and so-called "brothels." In reporting the findings, Johnson often referred to specific parts of Lilongwe, the capital city. One area, Chigwirizano, generated a chorus of knowing mumblings in the audience and prompted the speaker to add, "Yes, we all know what happens there…" Indeed, the relative smallness of the nation means almost everyone at the meeting possessed a certain "thin" cosmopolitanism or shared surface knowledge about the culture or characteristics of a place. In this case, the mumblings and the speaker's comment mobilize a whole set of underlying, implicit local knowledge and stereotypes associated with Chigwirizano—it is a place of nefarious sexual promiscuity, it is an urban place deprived of rural and traditional values, it is a place where men visiting the urban center for work or business hire young girls for sex and it is a place of high AIDS prevalence. In this way, shared and

[321] Field notes; October 22, 2008.
[322] Bourdieu uses this term to indicate that knowledge exists in a practical state in agents' practice and not in their consciousness, or rather, their discourse" (1977:27). In other words, knowledge is a set of socially acquired propensities of an actor to act appropriately in given circumstances.
[323] Pseudonym.

pre-existing cultural scripts of who and what are to be blamed for the epidemic are performed jointly by audiences and speakers and serve to validate the evidence underlying a claim such as "High risk sexual practices happen in Chigwirizano."

In another instance, this same shared place-based imagination of risk mobilized stereotypes associated with Malawi's diverse ethnic groups or with cultural practices. In presenting findings from the 2007 sentinel surveillance report, Johnson came to a Powerpoint slide that listed all fifty-four surveillance sites in order from lowest to highest HIV prevalence. As he read off the names of a number of geographic sites, the audience again responded; their reactions, in all cases, helped to validate the findings on the slide. In the case of places identified as high prevalence, audible suggestions of what cultural practices might have brought about high rates circulated in the room: Limbe ("Ah, the city…"), Chiradzulu ("lots of women come there from the city to do sex work") and Mangochi ("Yup… Fishing!") These proclamations, however, hinted at larger, unspoken ethnic stereotypes that circulated in the audience—for example, that the fishermen are primarily Yao men. This assumption fits into a larger AIDS landscape that presumes the southern part of Malawi (with a large Yao population) to be most infected and also to be least educated and most underdeveloped. These kinds of fact-stereotypes are revalidated and, often, find future life in subsequent research proposals and projects. In October 2007,[324] two Malawian researchers developed a proposal for a study of sociocultural values and HIV/AIDS. The researchers repeatedly inserted the phrase "cultures across the country" in both speech and the proposal itself; this indicated their imagination of the small country as comprised of self-contained units with different practices that mapped

[324] Field notes; October 9, 2007 and October 23, 2007.

on to the separate groups of people who live there. They also sequestered cultural practices to areas where the groups traditionally associated with them resided, even identifying "control group areas" where the practice(s) in question "are not known." These kinds of shared knowledge serve as a sort of benchmark by which new claims are measured and comparatively evaluated.

Circulating Pre-packaged Culture

Actors in the policy-research nexus share not only a common vision of the cultural landscape of Malawi, but also a common, pre-packaged conception of "culture." In the circulation and imagination of the term "grassroots," we see how the cultural maps onto the rural and the non-technical, lending credibility to claims that seek to change or intervene into culture. The National AIDS Commission (NAC) relies on District AIDS Coordinators (DACs) to determine which community groups (CBOs) in their jurisdiction are worthy of receiving pooled funds meant to bolster the "grassroots response to AIDS." On a visit to the District AIDS Coordinating Committee (DACC) office in central Malawi, I met the coordinator responsible for reviewing proposals "from the grassroots" (from CBOs and other local groups).[325] He explained that a proposal is assigned a score of 1-5 (poor to excellent) in each of the following categories: identity and purpose of organization, proposal summary, description of CBO, purpose/activities, implementation plan, sustainability, budget and authentication.[326] To be successful, he said, a CBO must score a minimum of 33 points. As I leafed through the stack of yellowing proposals collected in a bulky black binder, it was evident that international and national policy

[325] Interview, DAC central Malawi; March 19, 2008 and field notes; March 19-21, 2008.
[326] Due to a rash of "briefcase" NGOs and CBOs started by opportunistic businessmen wishing to "cash in" on funds distributed through a NAC system that does not hold recipients very accountable, standards have recently come to encompass authentication.

level concerns and priorities had percolated "down" to the rural authors of the proposals in the binder. Almost all the proposals mentioned, for example, "monitoring and evaluation" and "sensitization"[327] as integral to successful implementation of their projects. In general, the proposals were handwritten; some were in Chichewa, but a few were in (poor) English. Authors sought NAC funds for many activities such as: orphan care, pig or goat farming, caring for elders or the infirm, sports equipment or a public address (PA) system for youth clubs, home-based care (HBC)[328] trainings and painkillers for HBC kits.

The district evaluating committee attached to the proposals reasons why certain applications were not good enough to be funded. By what standards of evidence did reviewers assess these proposals? How did various objectives "translate" into worthiness to receive these funds reserved for people at the "grassroots?" First, it was clear that proposals had to align with the review committee's imagined ideal type of "proposal written by a local grassroots group;" this imagination tended to evaluate the presence of local culture as compared to "technical" or "professional" criterion. For example, one of the most neatly organized and meticulously typed proposals was rejected with the following note: "We should advise the CBO to write its proposal on its own;" later, "It seems this CBO director is a civil servant or has had some help typing and preparing the proposal." These sentiments indicate that neatly prepared, word processed and collated proposals did not fit with the committee's imagining of the "grassroots," thus marking

[327] As its name implies, monitoring and evaluation (M & E) refers to tracking of project outputs and outcomes via pre-defined indicators of effectiveness and to assessing impacts during or after completion of a project. M & E emerged out of recognition of the need for increased accountability in funding structures. Sensitization implies something like consciousness-raising, usually through definition of a "new" message multiple times; One proposal in the binder included a budget line for 208,250 *kwacha* (about $10,000) for "Sensitization Activities" in three trips to nearby villages.

[328] HBC came into existence as a response to the inadequate capacities of health care systems across sub-Saharan Africa to serve AIDS partients. HBC mainly draws on female local volunteers who provide HIV prevention education, some clinical care and counseling on diet and "positive living" with HIV.

such groups as outside the purview of NAC's pro-poor, grassroots targeted funding scheme. The committee further solidified the boundaries separating the technical and the cultural when they rejected the Fisherman's Association of S____'s proposal to conduct research on the AIDS knowledge, attitudes and behaviors of their members. The committee noted that this local group lacked the expertise or skills to conduct human subjects research. Though the survey questions the group suggested to guide their research were by no means sensitive or ethically charged, the rejection of their proposal by the committee drew strict boundaries around the realm of "research." The committee assumed that research cannot be conducted by "just anyone" and that extensive technical training in methods, ethics and research design were needed to implement a project like the one proposed by the fishermen. Both of these rejected proposals were too forward in their aspiration to transgress boundaries between the cultural and the technical or "expert." This resonates with anthropologist Elizabeth Povinelli's framing of the dilemma of Australian aboriginals who seek state recognition in a context of liberal multiculturalism: "[These] subjects must identify with the impossible object of an authentic self-identity… [and] are called on to perform an authentic difference" (2002:6). The rejected proposals can be read as failed attempts by local subjects to perform an authentic "grassroots" to their audience despite the coordinator's suggestion that these groups are often coached backstage ("We [the committee] go out to tell them [villagers] how to write things to get money").

AIDS Cosmopolitanism and "Emerging" Issues

Shared implicit cultural scripts rely on transnational language and tropes, as well. In her discussion of the spaces of transnational modernity that characterize global forums on international human rights violations, Sally Engle Merry suggests that in the post-colonial era, the, "glamour of the modern is still juxtaposed to backward others, but now it includes those who are "developing" but still burdened by culture (2006:102)." An international conference of demographic social scientists researching the AIDS epidemic in Africa was held at the United Nations International Criminal Tribunal (UN ICT) in Arusha, Tanzania in late 2008.[329] Hundreds of researchers and policy makers at this transnational venue produced and shared knowledge about the epidemic. Fittingly, this venue was nicknamed "The Geneva of Africa,"[330] capturing its simultaneously local and transnational character. As a place where patently local grievances and past atrocities are tried by international adjudicators, the UN ICT was a place where local knowledge was presented to international audiences. Even the headsets and translation technology usually used in the court proceedings were co-opted by the conference organizers to translate French presentations and panels into English and *vice versa*. Real time linguistic translation is a tool that aims to collapse geographic and cultural boundaries that impede the sharing of knowledge and has come to be expected by audiences whose members are diverse, but unified by their interest in addressing the AIDS crisis.

In conference settings, speakers frequently made reference to the transnational to indicate their "AIDS cosmopolitanism." In a presentation on changes to the ethical

[329] Field notes, Arusha; December 10-14, 2007.
[330] Arusha was first christened with this nickname in 2000 when former US president Bill Clinton visited the city to witness the Burundi peace signing pact. He compared Arusha with Switzerland's city of the same name, which, like Arusha, is host to a plethora of UN offices and international organizations.

review guidelines for research to be conducted in Malawi at the annual National Research Council (NRC) meetings, a Malawian bioethicist, Lackson Muntha,[331] anchored his statements in the transnational to give his them efficacy and power: "In addition to our own existing documents, we have authority from the global and universally validated guidelines."[332] He indicated that Malawi had some "catching up" to do when he said "I was in Europe and the systems for regulating research were very rigid there, more rigid than here." In rooting his presentation in "universal" standards, Muntha fixes the audience's gaze on models and guidelines set by Europe. Later, he emphasized again Malawi's place in a larger transnational regime of bioethics: the ethical standards in Malawi are "universals—they have been developed and put down by CIOMS, Helsinki, ICH-GCP."[333] Further, the "pressing" issues faced by his local committee were cast as "emergent" and found traction in language used at the transnational level: ethics committee shopping, biopirating and intellectual property. In line with Malawi's plans to "update" its ethical oversight guidelines, he highlights a number of technical improvements to monitoring of research; for example, an electronic submission system for protocols ("we are moving away from a paper-based system") and new software that will examine what research projects are doing in real time. This, like "technization" across many sectors of the international research in Malawi, will allow Malawi to be more effectively linked in to a global network it is currently "just stepping into." This bioethicist claimed authority and gave his interests and agenda (to revise and update Malawi's ethical guidelines) momentum by giving his evidence: the larger outside world

[331] Pseudonym.
[332] Field notes, Lilongwe; March 12, 2008.
[333] Council for International Organizations of Medical Science (CIOMS), the Declaration of Helsinki (Helsinki) and International Conference on Harmonization Good Clinical Practice (ICH-GCP).

has universally already adopted such guidelines. As a bioethicist in Malawi, his corpus of transnational travel and exposure to multiple ethical contexts maintains the boundaries of his expertise and distances him from those who might seek to contest his claim that these changes were worth investing in.

This same temporalization of the international imaginary (where Malawi aspires to, but has not yet achieved transnational modernity) occurred when "hot-button" or morally charged issues were discussed. At dissemination meetings, some participants suggested that sex work be legalized in Malawi to encourage female access to preventative and health services. Members of the audience were clearly uncomfortable with the moral implications of this policy recommendation and questioned its validity; in their eyes, the evidence that legalizing sex work was a good thing, rooted in recent qualitative research studies, was not good enough to exceed the moral evidence that sex work is a shortcoming of a god-fearing society (as in the case study of the MSM claim in Chapter Four). After a brief discussion, a NAC staff member closed off those who had a different opinion through explicit reference to the transnational:

> People think it's time to hide things now. When HIV arrived in the 80s, they said it's not an African problem and we buried our heads in the sand. The same happens with sex work today, It's going on! People feel as if we regulate it enough, we can control the fire. Like what is done in other more advanced countries like Thailand. They [sex workers] have rights [there].[334]

Categories or risk groups, too, are given legitimacy in discussions about AIDS transmission due to their transnational relevance or universal circulation. One paper at the dissemination conference[335] claimed that multiple concurrent partnerships (commonly known as MCP) were a major driver of Malawi's AIDS epidemic. An audience member

[334] Field notes, Dissemination Conference; October 23, 2008.
[335] Ibid.

asked whether the researcher thought that "people in the north" of Malawi (widely known to practice polygamy) had heard of MCP. The researcher responded: "I think in the next year the phrase 'MCP' will be known to everyone—all over." Later in the day, another researcher referred to MCP as a "key driver" of the epidemic. The factual or scientific basis of this claim did not rest primarily on evidence produced in Malawi but, rather, in its familiarity and legitimacy within the international AIDS research world.[336] Transnational circulation of risk categories, in fact, became good evidence that these categories were relevant to or worth funding in Malawi, as well. Further, well-known categories serve to unify the diversity of international actors present in these situations: they assemble diverse people through their intelligibility. MCP is an import from global funding priorities and international, comparative research contexts; yet, on the ground in Malawi, this category circulates widely and reorganizes national assumptions, research priorities and etc…In another example, a National AIDS Commission (NAC) staff member indicated that Malawi AIDS policy would soon include IV-Drug Users (IDU). Though he insisted this risk group was "emerging" in Malawi, I, nor others I spoke with, had noticed real life evidence of this.[337]

Investment in Provisional Numbers

Numbers become evidence only when they emerge out of and corroborate past accepted knowledge claims rooted in statistics. In Malawi in 2008, UNAIDS fomented what colleagues and lay people in Malawi termed a "numeric scandal:" they revised their estimate of national prevalence of HIV/AIDS in Malawi down from 14 percent to 12

[336] Epstein (2007) is a key proponent of MCP.
[337] Field notes; December 12, 2008.

percent.[338] This downsizing was widely discussed in newspapers and radio, and often came up in the discussions of rural villagers. Without knowledge of the back-stage mechanisms by which this revision came about, they were confused about this shift. At conferences, too, the friction between these two numbers was evident; while many speakers cited the earlier figure in presentations, others began to correct them. This large-scale change in a well-circulated estimate had ramifications across other spheres where numbers were used; the public was now more skeptical of numbers. While a prominent research project presented the prevalence rates it collected across three regions of Malawi, ranging from 7-11 percent, CBO members raised questions: Why are your estimates different from the UNAIDS ones? What is *the* number?[339]

Across sub-Saharan Africa, the ongoing march of statistics and numbers produced annually serves as a benchmark of a country's progress. Most notably, a significant reduction in prevalence of HIV in Uganda (from thirty percent in 1992 to eleven percent in 2000) has since produced a massive discussion around why these numbers declined. Uganda has become notorious as an "AIDS success story," a benchmark or ideal for other countries in the region to mime and emulate.[340] The change in Malawi's prevalence rate and the framing of Uganda as a success story rest on accepted knowledge in the form of authoritative numbers. However, in the first case, we note that the historical numbers were invalidated by "new and improved numbers;" in the second, we note that "new numbers" relied on comparison with old numbers to attain credibility. Both kinds of

[338] This new estimate resulted from a number of factors: development of new tools based on the recommendations of a UNAIDS reference group; improvement and expansion of HIV surveillance systems in sub-Saharan Africa; collection of data adjusted for non-response and other biases by national population-based surveys (UNAIDS 2008).
[339] Field notes; October 22, 2008.
[340] The generally accepted reasons for this decline are: high level political support with multi-sectoral response, decentralized planning and implementation of behavior change initiatives, interventions centered on women, youth, stigma and discrimination, active religious response to the epidemic, Africa's first VCT services, condom social marketing, intensified STI response, decrease in multiple sexual partnerships (USAID 2002).

numerical evidence, however, have since become standardized and institutionalized into the ongoing AIDS response in the region.

Finally, numerical "indicators" often act as a shared matrix that floats between the national and international levels. A senior researcher at the Centre for Social Research (CSR) emphasized that "numbers have become god" since the gradual institutionalization of a sector wide approach (SWAp) in Malawi since 2004.[341] In the health sector, adoption of the SWAp has "streamlined" and "harmonized" the activities of funders, government, institutions, researchers, policy makers and stakeholders. Under the SWAp: funding agencies share strategies, targets and standards of evaluation; the government increasingly relies on management and accountability systems; and implementation of AIDS and other health-related activities are reviewed according to jointly agreed milestones or indicators.[342] In October 2007, the Annual Review of the National HIV and AIDS response in Malawi was held.[343] At the "review," the influence of the SWAp was clear: Malawian partners presented their progress to annual donors in numbers. Held at a central and well-known hotel, this was a closed meeting between signatories of the HIV/AIDS Pool donors, NAC board of commissioners, ministry of finance, ministry of health, ministry of child and women development, HIV/AIDS development group, chairperson of the Malawi Global Fund coordinating committee, sector wide approach secretariat, UNDP, CDC, Office of the Director of Public Procurement and the auditor general.[344] The acting director of policy and programmes [sic] for NAC presented the target indicators for NAC implementing partners, addressing whether or not these targets

[341] Interview; October 25, 2007.
[342] Ibid.
[343] Field notes, Lilongwe; October 1, 2007.
[344] Contacts at the University invited me to attend.

had been met or exceeded (or there was not enough data). This meeting was an ideal site in which to assess how numbers serve as a kind of evidence that donors use to answer questions about how well a country is performing. Did Malawi meet or exceed a large enough portion of its target indicators? Should donors fund them (more or less money) next year? Numbers measured achievement across categories such as: prevention, treatment, impact mitigation and communication. In the category of communication, for example, indicators measured numbers of: booklets produced, campaigns held or life skills workshops staged. Treatment indicators measured the number of Malawians on ARV treatment. NAC claimed that Malawi exceeded its target of 80,000 people on ARVs (they attained 114,000). As the donors filled in their sheets, it seemed that the Malawian presenter said "no data" for more indicators than she presented numbers for. The donors sat quietly, unfazed. This "review" enacted and reproduced the Malawian government and donors' joint acceptance of the conditions attached to aid funds ("You must be held accountable" or "You must provide us numbers at the end of the year.") In the end, the Malawian government achieved a sort of nominal compliance by collecting some, but by no means all, of the required data. The evidence they brought that day, though nominal, was enough to evade potential penalties for non-compliance from their funders (in fact, they received a promise for more funding in the coming year). This exchange shows that donors are interested in continuing aid but also in appeasing increasing worries that their beneficiaries are not held accountable. Malawian government officials are interested in ensuring the uninterrupted flow of aid to Malawi.[345] Donors gain status, symbolic and social capital by donating; Malawian government officials gain financial and social

[345] In 2008, 40% of Malawi's budget was funded by foreign aid (STC 2009).

capital through nominal compliance with donor demands that are represented as a contract. The initial and obvious asymmetries between donors and receiving governments make the discourse and flow of aid possible in the first place.

Assessing a Speaker's Network or Credibility

In addition to measuring knowledge claims and evidence against past evidence, audiences also evaluate the position, commitments and knowledge networks of speakers who claim knowledge. Whether evidence attains a truth threshold depends on the position of a speaker: namely, is he inside or outside of a given knowledge community assessing his claims? Credibility can be assigned based on a speaker's alignment with various identity categories—indigeneity, disciplinary training, kinship, research or policy network membership—and can be collected through networking.[346] Researchers and policy makers in Malawi are especially interested in forming relationships with people who might hold knowledge that is different from or complementary to their own because a larger number of "different" people in one's network increases the probability that one's claims will be favorably assessed by wider audiences.

"Face Time" and "Greasing the Wheels"

Networking, or building social relationships to increase social capital, plays a central role in knowledge production, circulation and validation. Conferences are a major site where social networks are reinforced and expanded; they provide a "neutral" space away from the "daily grind" where diverse actors are brought into contact. International

[346] Networking is a cornerstone of Malawi AIDS research and intervention policy; the National AIDS Commision (NAC) devotes $19 million USD to four key objectives that center on strengthening partnerships across sectors, organizations, districts or communities.

and regional AIDS conferences that bring together researchers, policy makers and others from different geographic sites play an important role in foreign researchers' accumulation of both informational and social capital. Conferences are one site where diverse people are enfolded *into* networks that are assumed to focus attention on problems *out there*.

Conferences were characterized by a certain surface-level cordiality and politeness. This often masked social relationships that were strained or taxed in everyday life; the conference halls were a "neutral" space where people could reinvent themselves or recast their relationships to others in another time and place as "experts" with similar interests in a social problem. Chats in the corridors were both casual and urgent—interlocutors were not only catching up or introducing themselves, but also interested in acquiring useful information or meeting "the right people." In one-on-one conversations and in public forums, people felt each other out, tested the waters and collected new information. For example, at one such conference, an American principal investigator (PI) and a Malawian researcher called Ahmed P.[347] talked over cocktails and snacks.[348] The American researcher knew Ahmed for many years via infrequent and intermittent trips to Malawi and ongoing email exchanges. After some small talk, the PI pressed Ahmed about whether he knew anything about, "the progress of our study through the national ethical review process." As the study was taking unusually long to gain approval, the American PI saw this run-in with someone potentially "in the know" at the conference as an opportunity to garner information that could demystify the unusually slow review process and reassure herself that her study would be immanently approved.

[347] Pseudonym.
[348] Field notes, Arusha, Tanzania; December 11, 2007.

Though Ahmed's facial expression betrayed his non-knowledge, he quickly whipped out his phone and began to SMS[349] "someone who might know!" stating that although he was no longer on the committee, he did know many of the current members. The SMS gesture appeased the American PI and was a convincing performance of the durable network, historical relations and obligations between these two individuals.

Because expatriate researchers are infrequently in African countries and, even when there, find little time to meet with local researchers or collaborators who may be out of the country, conferences are important opportunities to get "face time" with local collaborators. Expatriate researchers working across Africa "counted on" running into their local collaborators at conferences. One principal investigator on multiple studies in African countries said that he does not come to these conferences to attend panels or gain knowledge but to "grease the wheels, chat, figure things out and connect with my collaborators from all over the place."[350] He was disappointed that his Malawian co-principal investigator on an ongoing research study in Malawi had "left [him] in the cold;" they had planned to have some important conversations about the progress of an ongoing project and "other matters" until he received an email "at the last minute" from his colleague who explained he would be unable to make it to Tanzania because the conference was "refusing to pay his way." Later, at the conference dinner, a Malawian demographer joined the table where the American researcher was sitting. Quickly, the latter introduced himself and proceeded to say, "We need some…local Malawians to get involved with our research. I'd love to talk with you about your existing collaboration

[349] SMS is text-messaging.
[350] Interview, American demographer, Arusha; December 15, 2007.

with [another project]."[351] The two researchers exchanged business cards, a common "gift" at conferences, and agreed to get in touch in the future. This scene points to the increasing commodification of local knowledge as expressed in the emphasis on role ("We need a local expert") over content (meaningful participation, knowledge sharing) in networking. However, African researchers also suggested that their primary reason for attending conferences was to accumulate social and informational capital; in their cases, these forms of capital were easily convertible into economic capital.[352]

While the prior instance indicates that researchers seek to recruit "different" individuals to their networks, conferences were also sites where they could amass knowledge or connections with people "similar" to them. Expatriate researchers shared meals with one another; those working in the same African country "compared notes" on their experience navigating local research bureaucracies or finding local partners. Researchers who were new to a country sought advice and tried to get the "lay of the land" from more seasoned researchers. Finally, researchers who were at the conference but not currently "in country" met with graduate students or researchers who were living more long term in Malawi to ask questions about what projects were currently working in Malawi, about current events in the country and about their experiences at internal research conferences. In general, researchers enjoyed more casual and informal relationships with these individuals who were "similar" to them and from whom they could obtain "straight answers" and "local" knowledge more efficiently. Further, these "in country" individuals often acted as couriers of information or objects that other researchers sent to an African country: USB cards, laptops, messages or books.

[351] Field notes, Arusha; December 15, 2007.
[352] Interviews, African partners, Arusha; December 11-15, 2007.

Determining if a Performance of Knowledge is Convincing

In addition to evaluating new knowledge claims against a backdrop of already accepted implicit knowledge and assessing the credibility or networks of a given speaker, audiences also validate knowledge based on whether or not it is packaged in a convincing or familiar manner. Genre is at the center of the translation of knowledge because it is precisely how information or content is made intelligible to audiences; it engenders certain expectations, affects and reactions in an audience. In the case here, whether the audience's expectation is met or not has a great impact on the assessment or evaluation of the knowledge being translated. In his careful attention to the social organization of knowledge and expertise in a society with many internal knowledge traditions and kinds of "experts," Lambek (1993) draws attention to the importance of *genre* in whether or not certain claims are assigned legitimacy or authority. He views knowledge itself as a resource to be manipulated or drawn on by individual agents who, through initiative, performance and rhetorical skill are able to gain social status and recruit human followers, believers or other desirable "objects." His analysis hinges on viewing culture itself as a process; in this way, expertise and authoritative knowledge emerge out of practice, conversation and unpredictable confluences of people, things and contexts. His conceptual focus on the performance rather than the inherent, stable content of knowledge or knowledge traditions informs the focus of this section.

Genre plays a central translational role in Callon's (1986) now-famous study of the construction of "scientific knowledge" at a 1970s conference held to brainstorm strategies to increase the production of scallops by controlling their cultivation. At this conference, researchers discussed diagrams and deployed tables with numbers in a closed

room. But, as Callon points out, these discussions enlisted uncountable populations of silent actors: scallops, fishermen and specialists who are all represented by a few spokesmen. They have been mobilized—brought to the conference room through a series of transformations: "The scallops are transformed into larvae, the larvae into numbers, the numbers into tables and curves which represent easily transportable, reproducible, and diffusible sheets of paper" (1986:210).[353] For Callon, translation emphasizes: "continuity of displacements and transformations which occur in the story: displacements of goals and interests, and also, displacements of devices, human beings, larvae, and inscriptions" (214). But to translate is also to express in one's own language what others say and want, why they act in the way they do and how they associate it with each other: it is to establish oneself as a spokesman. "The three researchers translated the fishermen, the scallops and the scientific community" (222). Callon's framing of his actors as spokespersons or translators is helpful in considering the role of researchers who present their findings at conference venues. These spokespersons enlist countless actors (the research participants and communities their data originate in) when they present miniaturized versions of reality—statistics, images or findings—to diverse audiences.

Buzzwords and Presentational Conventions

In what follows, I explore the function of two forms of genre that are central to the translation of research findings and that determine whether findings are legitimated or challenged: presentational conventions and buzzwords. My definition of genre in this section centers on its role in the performance of knowledge; I view genre as a vehicle for

[353] Cf. Latour 1987.

knowledge that facilitates translation by containing the "new" in familiar casing. As in the case studies that comprise Chapter Four, it is clear that performances of knowledge that deviate too far from conventions of presentation or performance or fail to "package" evidence in familiar and comfortable casing, often fail. For instance, at many conferences and other expert forums, conventionalized style played a key role in carrying messages from sender to receiver; research conference culture elevates style over content. In many settings, Powerpoint has become an almost ubiquitous fixture that follows a set script: The title, presenter's institution, name, conference title; the presentation outline; the statistical summary of findings. Presenters who failed to use Powerpoint were often dismissed or devalued.

Buzzwords and categories that circulate widely at conferences were a *lingua franca* that encased evidence and propelled it forward. These words and phrases are not confined only to the forums this chapter is interested in; instead, they promiscuously cross borders and circulate the globe, utilized by actors ranging from villagers to aid partners. In their familiarity and wide recognition, buzzwords serve as heuristics or sign posts for multiple audiences. These terms travel easily across borders. At the District AIDS Office in central Malawi, the District AIDS Coordinator (DAC) had assembled a list of over two hundred community based organizations (CBOs) operating in the district ("about 175 of which are active"). A quick glance at this list indicated that almost all of the CBOs included "orphan care" or "orphan support" in their names and objectives. The DAC explained that in order to be official recognized by the National AIDS Commission (and, therefore, eligible for national funding), CBOs were required to register themselves at the Social Welfare Office of district assemblies. In order to register, NAC also

mandated that local groups focus on an issue pertinent to social welfare; at the time that these community groups registered, "orphans" were the "big thing," so most of them "took that up in their names" even though they "do not even actually focus on orphans."[354] In a rural district in Malawi, then, we can trace larger national AIDS priorities in layers of local responses—formation of groups, prevention activities and targeting of resources—that fit themselves into a more institutionalized AIDS response. However, the proliferation of connections, committees and groups around certain objects or problems does not necessarily equate to efficacy or enhanced problem-solving.

Buzzwords also served to frame the social interactions and proceedings of conferences. Often, these buzzwords were so common and well-known that they were abbreviated into familiar and recognizable acronyms: OVC, MSM, MCP, NGO, CBO, GAD, WID, SWAp, NAC, PLWHAs, HBC…[355] These acronyms are generic because they are germane *to a particular audience*; they point to things in the real world that are pertinent to their discussions. They direct and channel attention toward groups, organizations or practices that are assumed to have important meaning for the spread and mitigation of AIDS. Acronyms, like data itself, are an example of the miniature form discussed in Chapter Three. Amid a diverse array of audience members, these acronyms create a sense of community, shared interests and belonging. They construct boundaries around those who are not *inside*.[356]

[354]Interview, DAC, central Malawi; March 19, 2008

[355] In order, these acronyms stand in for: Orphans and vulnerable children, men who have sex with men, multiple concurrent partnerships, non-governmental organization, community based organization, gender and development, women in development, sector-wide approaches, National AIDS Commission, people living with HIV/AIDS and home based care.

[356] In 2004, researchers for a case study project allowed Malawian fieldwork supervisors to design the employment "test" for new interviewers. They studded it with acronyms such as the ones mentioned here; this was likely a form of boundary work whereby they attempted to "distance" themselves from potential interviewers who would, presumably, be less likely to know the acronyms' meanings and have less familiarity with the world of AIDS research and policy (Research notes, correspondence with American researcher; March 23, 2011).

Coda: Evidence as Policy Base

Conferences, workshops, academic papers and other downstream sites are places where diverse actors and experts come into contact to validate or challenge knowledge claims about the epidemic. These venues examine and evaluate the underlying evidence for such claims. Conceiving of these social interactions and exchanges as performances on "stages" and for audiences brings into relief the component pieces of knowledge: presenting evidence AND communicating it. Evidence never stands alone, but must accumulate authority by means of performances and acceptance of those performances.

I have formulated three main ways in which evidence is evaluated and gains momentum: measuring new claims against shared "facts," assessing a speaker's network or credibility and determining whether a presentation is convincing. Recent changes to the infrastructure of global health and other knowledge production projects have involved an increasing diversity of actors and expanded conceptions of expertise and experts. These shifts ensure that knowledge-making is no longer a top down endeavor and incorporates not only researchers, but also policy makers, villagers, activists, community groups and other stakeholders. Further, the imperative to make AIDS research "useful"—especially in impoverished and high-incidence contexts such as Malawi—has brought new attention to strategies and mechanisms that can effectively translate research findings into policy and practice.

Each of the evaluative processes elaborated above, in fact, necessitates translation. First, audiences assess knowledge based on whether it accords with past, previously accepted claims or implicit knowledge—i.e. cultural scripts. Thus, producers of knowledge (performers) must consider and anticipate the shared knowledge of their

audience(s) when presenting new knowledge. This can determine whether or not a certain claim is propelled forward or fails (as did Gift Trapence's claims about MSM and AIDS in Chapter Four). Secondly, performers of knowledge must be credible; one way in which they attain credibility is by networking. For example, recall Blessings Chimanda's claim that cultural practices in Malawi were a key driver of the epidemic. Because he was recommended to the expatriate consultant who was his audience—that is, considered a respectable local expert—his claim was given legitimacy over others. As a credible speaker who occupied this particular role in a new network, he communicated knowledge between "Malawi" and an expatriate consultant. Similarly, Malawians who gossip to determine neighbors' HIV status often cite a social chain of known and credible speakers, and demographers cite a chain of previous scholarly studies and "speakers" Finally, a performer must utilize props such as Powerpoint in convincing his audience of a knowledge claim's veracity. These props also carry messages to audiences and if a performer inaccurately chooses her "props," her knowledge is unlikely to attain a truth threshold. Here, consider Kate Brown's decision to include photographs and interview excerpts in her presentation of findings about couples voluntary counseling and testing (VCT) to an audience primarily of demographers. This "packaging" was slightly misaligned with the presentational conventions by which evidence is usually translated and contributed to the audience's assessment of Kate's findings as "merely interesting."

The incorporation of diverse actors into knowledge production and circulation in the AIDS policy-research nexus means that performers and audiences must adopt new sorts of flexibility as they are expected not only to make but *translate* evidence. As the link between practice and knowledge, policy is a proxy for social action. In a moment of

global health crisis and massive global inequality, social science research in contexts like Malawi is increasingly expected to be policy-relevant and "useful." This emphasis has entailed numerous rearrangements of the social infrastructure and roles inherent to this regime. First, knowledge production projects in Malawi must incorporate local experts and local collaborators. I showed in Chapter One how this mandate takes form in the recruitment of "elite" and everyday experts to expatriate-led projects. This creates a legitimate human infrastructure for production of AIDS knowledge: it is presumed to guarantee participation, incorporate local knowledge, facilitate sharing of knowledge and resources and ensure more "relevant" research questions. However, producers of knowledge must also consider how to make their findings useful to potential consumers assumed to be on the front lines of the epidemic: policy makers, NGOs, activists, organizations "on the ground" and the general public. While the first rearrangement occurs before and during research fieldwork, the second necessitates encounters between increasingly diverse actors. This and the previous chapter, in their focus on performances of knowledge in venues including conferences, workshops, journals, media sources and meetings, have elaborated the details of some of these encounters.

Filling the Policy-Research "Gap"

Before an audience can evaluate evidence it must be made accessible through translation. In what follows, I elaborate on two explicit translational strategies meant to fill specific knowledge gaps: 1) between AIDS researchers and research subjects; and 2) between AIDS researchers and policy makers. First, I describe how people try to "fill" these gaps—by networking and dissemination—and then I show how these strategies

work not only to carry evidence across boundaries, but also to reinforce them. Finally, incorporating insights from analysis of these two strategies for "closing the gap" and from the case studies discussed above, I show how the prioritization of translation and communication in the policy-research nexus might, paradoxically, contribute to the production and validation of non-innovative knowledge and disallow for critical, meaningful engagement among different actors and experts.

Disseminating Findings to Those on the Front Lines

During interviews I conducted with not only the principal investigators of my case study projects but also the expatriate researchers leading biomedical and social scientific AIDS research projects across Malawi, the topic of how to, first, effectively make research accessible to policy makers and, second, how to "close the gap" between policy and practice was central. Often, these researchers crafted proposals for dissemination activities alongside their actual research proposals; these initiatives were unified in their mission to disseminate evidence in a "comprehensible," "relevant," "abbreviated" or "simpler" form. Both national and international research bodies in Malawi have recently faced increased pressure to disseminate their research findings and results back to the local people they collected their information from or to front line NGOs and CBOs who could use the findings to better structure AIDS programming. For example, in response to circulating complaints among research participants that researchers just take information without ever telling them the findings,[357] a case study project designed a one-page sheet

[357] One district commissioner (DC) of a district where researchers were collecting data suggested they bring him some reports of publications so he could see what they did. "You are always researching in my area, but we hear nothing," he said. Projects did try to hold dissemination meetings at the district office for district health staff and interested others. However, because of the widespread assumption that attending a meeting or workshop entails receiving a *per diem* and other benefits (transport money, food, perhaps lodging), projects often lacked the budget for such endeavors (Field notes; May 15, 2008).

that summarized findings in Chichewa. At a larger scale, the National AIDS Commission (NAC) in Malawi decided in 2007 to begin holding what they called "NAC Zonal Quarterly Review and Dissemination Workshops."[358] NAC described the objectives of these meetings: "to discuss the key findings of surveys we conducted in the country." During the 2005 Research Council Meeting, there was significant recommendation to replicate the conferences done at the national level to the district level so community based organizations (CBOs) could benefit from hearing the information. As a result of that recommendation, "We would like to give a chance to people who have not attended the national meetings to hear what was said there."[359]

The pioneering zonal meeting in October 2008[360] in northern Malawi attracted a wide audience, which had been invited by NAC.[361] Most of the participants were CBO members (usually the chairpersons), members of District AIDS Coordinating Committees (DACCs), District HIV Programming Officers and so on who came from districts in northern Malawi. In order to ensure that financial barriers did not prevent them from attending, NAC paid for participants' accommodation and transport.[362]

The premium placed on "translation" of research findings was evident in the introduction to the workshop given by the conveners. Translation had a two-pronged goal: 1) Linguistic intelligibility; and 2) Making findings useful or relevant to the audience. On the first day, it was announced that some of the papers presented at the annual national level research dissemination meeting in Lilongwe earlier that year would not be presented at the local level because, "[they] were felt not to be relevant to the

[358] Interview, NAC; April 28, 2008. Other zonal Meetings were held in southern and central Malawi between September-November 2008.
[359] Ibid.
[360] Field notes; October 22-24, 2008.
[361] NAC sent invitations to district assemblies and CBOs they funded in the region to send a representative to the zonal workshops.
[362] They were also provided with a 2500 *kwach*a ($18) *per diem* (for a total of 7500 *kwacha* ($54) over the three days).

needs of people at the district assemblies" and "some others touch on the policy level only."[363] The form, tone and organization (the interactional context) of the workshop itself was different than national level meetings. For example, on day one, it was announced that participants should feel comfortable using the language of their choice ("Chichewa, Chitumbuka, Chizungu…"); "We can always translate!" This drew appreciative nods from those assembled, a sort of validation of the local participants. Furthermore, the organizers emphasized that part of the reason they had chosen to allow fewer researchers to present their findings was because: "We want the CBOs to really grasp what the articles are giving us, so we have given a lot of time to the presenters and left room and time for good discussions and questions." The overall tone set by the organizers was not paternalistic but spoke to a self-conscious circumscription of what it was that local people could be expected to appreciate. Yet the comment about the CBO members' ability to grasp the research papers was a way of distancing the local participants in the workshop by explicitly treating them differently than the presenters and conveners. In fact, the very mission of the zonal workshop itself relied on the geographic, cultural and educational distance between experts and the local people to whom they were speaking. Even as this meeting's purpose was to close the "gap" between research and those on the front lines of the AIDS epidemic, it assumed there were "gaps" in socioeconomic status, education levels or proximity to urban centers between those present. After all, "dissemination" strategies are formulated and mobilized to *fill gaps* in the first place. "Closing the gap," in this instance, is a form of boundary

[363] A NAC consultant was responsible for determining which research findings would be presented at the meetings.

work whereby producers of knowledge reinforce, perform and protect their "expertise" even as they disseminate it.

Sharing Findings/Networking with Policy Makers

Gaps cannot simply be closed through networking or dissemination. This gap are conceived of as a space of non-translation between policy makers and researchers; it stands between these two groups of actors as a sort of chasm and prevents the enlistment of research findings or evidence into policy. This gap erodes the utopian ideal of evidence-based policy. Consequently, closing this gap has been prioritized in both international and national research and development agendas. For example, the Malawi National AIDS Framework (NAF) allocates 6 percent (about $22 million) of its budget to research and development activities: dissemination of research findings, coordination between research and policy and collaborations between researchers and policy makers. This investment translates into "building bridges" between the two sides via dialogue through networking to foster collaboration. One might imagine these initiatives as a sort of safety net built of humans that is thrown across the chasm, laden with high hopes that it will not only bridge the gap but benefit the two sides it links.

"Dialogue" between policy makers and researchers is framed as a key solution to the gap. Often, this calls for forums such as conferences, advisory boards, partnerships or workshops where both parties can not only communicate effectively and easily but also "network" I now briefly analyze some of the strategies to enhance networking implemented by a large biomedical research project in a major city in Malawi, paying special attention to the ways in which these strategies aim to "close the gap." A principal

investigator (PI) of a major research project had this to say about the connection between policy and research:

> Malawi's no different to the UK 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* [my emphasis].[364]

As for communicating research findings to policy makers, he said:

> *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 try to send representatives [to relevant conferences] whenever possible. I hope our science communication officer we just hired will *help open some of those channels*. [my emphasis].[365]

The PI's comments on the links between policy and research serve two functions. First, they reveal gaps between policy and practice and policy and research. As he explains, the needs, interests and orientations of policy makers and researchers are quite different. The former are "affected by the winds of change" and require quick information to "shoot from the hip;" The latter "lack a perspective on real life." Ostensibly, these factors contribute to the gap identified by this PI: "a lack of good, frequent dialogue between ourselves and policy makers." Secondly, his comments provide some insight into the ways in which his project is attempting to close this gap. The project, for example, is helping to fund an initiative that seeks to "improve the back and forth between policy makers and researchers" and recently hired a science communication officer who, the PI anticipates, will "help open the channels" between the two groups. In first framing the gap as a result of some seeming incommensurabilities

[364] Interview, expatriate biomedical researcher; April 1, 2008.
[365] Ibid.

between two groups with different interests, the PI believes dialogue or open communication channels (characterized by "personal contact and trust" between researchers and decision makers) are potential solutions.

How has this project operationalized this ideal of open dialogue? They have created a number of spaces where communication can occur: a technical working group, a health research capacity-strengthening initiative and an HIV/AIDS research partnership.[366] The PI was most hopeful about the potential strides to be made by the science communication officer they had just hired; this promised to "make our research evidence more usable… and strengthen the capacity of policy and clinical decision makers to use the evidence." He explained further that his project would begin generating short summaries and key point/recommendations to policy makers.

On the other side of the "gap," policy makers also saw a communication problem. One policy maker whose task involves the compilation and synthesis of research studies informative to policy concerns said:

> [It is my opinion] *that [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 [my emphasis].[367]

[366] Ibid.
[367] Interview, Malawian policy maker; April 28, 2008.

On the kinds of data he uses to make HIV policy:

> This afternoon we are leaving for Mangochi [a town near Lake 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.[368]

This policy maker constructs the gap in a similar way to the British PI; researchers "stand aloof" as "academicians," creating some "antagonism" with more practically minded policy makers—an antagonism that is decreasing. He described the ways that he and other policy makers use the findings collected by research projects. Although "research" plays a central role in his characterization, *researchers* are notably absent from the groups who attend the think tank meeting. Here, research findings circulate as disembodied digitized PDF files on flash drives, in email inboxes or as printed copies of reports.[369] These files and reports overcome the geographic (or other) distance between researchers abroad and policy makers in Malawi. As the PI described above, there is a lack of frequent dialogue between the two groups.

Within the research itself, "dialogue" is ongoing, if one-sided. Expatriate academic researchers publish findings from data collection and fieldwork conducted in Malawi and often address Malawian policy makers at the conclusion of papers they publish in peer-reviewed journals. This gesture is one attempt to correct the complaint that policy implications or recommended interventions are often absent from research outputs (Hennink and Stephenson 2005:173, Walt 1994). For example, Angotti *et al* suggest a number of ways in which policy and program efforts could increase rural HIV

[368] Ibid.

[369] Research studies and executive summaries are visible at the National AIDS Commission (NAC), National Research Council (NRC) and also district health offices but many informants on the policy side indicated that they missed out on studies because they lacked access to, for example, JSTOR or even to a reliable Internet connection that would enable them to download files. Case study projects increasingly make their articles and findings accessible via open access websites or databases.

testing, based on results drawn from a long term longitudinal study of testing acceptance; they indicate that confidential, convenient (door-to-door) and credible testing should be widely implemented (2009:6). Another of the case study projects made gestures toward policy makers in a report on their findings. It calls for "an increased focus, from policy-makers and researchers alike, on examining a broader set of outcomes that might be plausibly affected by these programs that are pertinent for policy design" (Baird *et al* 2010:2). Documents, then, attempt to communicate between researchers and policy makers—both as objects that address policy makers and that circulate among them.

Closing the Gap?: The Paradox of Translation

I suggest that gaps such as the "policy-research gap" are not just an empty chasm between researchers and policy makers, for example. Rather it results from constructions of cultural, social and political factors. As another scholar put it, they are "crowded spaces of moral practices and biases, so to say 'full' already of pre-, con-, and mis-conceptions—without which… life and policy would be unwriteable (and unreadable)" (Apthorpe 1997:55). How can these gaps exist between research and policy when the two fields are linked and share a common language, common concerns and overlapping interests? Policy, as an instrument for both representing and fostering change in practices has its own specific culturally constructed categories and priorities—its own language and its own constraints and rules. However, as is the case with AIDS risk groups, these priorities are not neutral categories for investigation, but ones that have been constructed through social, political and historical exchanges and whose attributes are settled before evidence is collected. While research is presumed to be a neutral device that collects data

to fill gaps or to make unknowns known (to describe reality), it is a political process that emerges from a simulated reality already authoritatively represented as really real.

Further, despite massive efforts to "close the gap," attention to this issue might serve to exacerbate it. Namely, formalized and standardized initiatives such as increased opportunities for networking, addressing policy makers in articles on findings, dissemination and multiple efforts at better translation serve convincingly to "fill" a seemingly shrinking gap without addressing the larger structural problems that maintain it. Even in the few cases where policy makers and researchers interact directly with one another, disparities in their training, interests, economic position and structural positions limit the critical dialogue that is possible. These instances of so-called "knowledge sharing" are limited by time constraints, overextension of Malawian policy makers and differences in interests and skill sets. Often, the premiums placed on "networking" are so high that quantity, as opposed to quality of such human connection are emphasized.[370] While evaluations of interventions or projects often examine the impact of their response, and their ability to attract, reach or involve large numbers of collaborators, stakeholders or participants,[371] they rarely address or consider the impact of network structures themselves on research outputs. What inequalities persist in the distribution of scientific research funds in a policy-research network? What growth in capacity[372] of individuals occurs over the duration of an individual's belonging to the same (or multiple) networks? What factors limit the meaningful contribution of, for example, Malawian partners to a

[370] Klenk and colleagues consider the constraints experienced by certain members of a large research network in Canada. In subjecting this network to social network analysis (SNA), they found that the benefits of belonging were unevenly distributed among kinds of collaborators (2010:954). These findings accord with the sentiments of stagnancy the local experts in Chapter One, long time members of research networks, expressed.

[371] For example, both NAC and research projects commonly track their improvements in the area of research/policy translation with indicators such as "*number* of interactive fora engaging policy makers and researchers" or "*number* of policies and programmes [sic] informed by health research evidence."

[372] The term "capacity building" is a common descriptor for activities or initiatives that seek to enhance human resource development, institutions or infrastructures through education or distribution of resources, usually in developing nations.

network? Without meaningfully addressing these and similar questions, elongating networks do not, as is suggested, increase dialogue or enhance translation of findings. Expatriate researchers continue to be first author on publications, to attract funds for research proposals and to accumulate new skills and knowledge. Malawian researchers, conversely, continue to remain second (or third…) authors, to flit from project to project, to lack time or skills to write competitive proposals of their own and, therefore, they follow ready-made donor-driven priorities and research.[373] These structural inequalities mean that the dialogue that researchers and policy makers enter into is lopsided.

Anthropologists and others have been increasingly concerned with understanding the processes of translation from research to policy and practice. However, many of these accounts consider translation in a limited sense—as a linguistic or localizing process which makes the outside intelligible or relevant within a specific context. As this chapter has shown, the evidence produced by research and enlisted into evidence-based policy is "made" through processes that are not only linguistic but social, political and interested. Audiences do not come to evidence as blank slates; individuals and groups encounter knowledge claims and their underlying evidence from a specific social position or location. The AIDS policy-research nexus is a social field where multiple actors make knowledge claims in order to further their own interests. In terms of making and evaluating knowledge claims, it is central to display authority and legitimacy: to align new knowledge with pre-validated cultural scripts, to be a credible speaker and to package evidence in familiar and convincing dress.

[373] Booth et al (2006), in an insightful critique and analysis of the aid relationship in Malawi, comment on the great multiplication of policy documents that are usually very short term and "do" little, and the way in which donor driven policy making leads to stunted, discontinuous and eclectic policy.

I suggest that an increased emphasis on the permeability of boundaries between policymakers and researchers covers over the gaps it unwittingly maintains and reproduces. The AIDS epidemic, as a pressing and exceptional global social problem is an ideal site in which to examine policy as a form of social action that translates and mobilizes not only evidence and discourses or myths about the AIDS problem, but also has significant ability to reconfigure social realities. Namely, the tragic human and medical consequences of the spread of the HIV virus place great emphasis on quick action, persistent calls for more data and rapidly conceived and implemented interventions. Often, the tragedy and gravity of the situation mean that little attention is given to the ways in which evidence is made, enlisted into AIDS narratives and policy and impacts the social worlds it enters. Evidence is an artifact of an accumulative series of ongoing performances or enactments of expertise that are evaluated by shared criteria for their validity and authority. Knowledge claims are not made by, nor received by, blank slates; instead, they are mobilized on a stage. Many suggest that the same pieces of conventional wisdom or narratives are validated again and again and bemoan the lack of innovation in the fight against AIDS. This chapter explored some of the reasons behind this reproduction of "facts." The triumverate of criteria by which evidence is evaluated acts as a sort of limit on the novelty and innovation of claims. A claim must not deviate too far from accepted knowledge; a claim must not risk or too egregiously misrepresent knowledge shared by respectable members of one's network; and knowledge must be presented in recognizable and conventional genres. Yet, non-innovation seems paradoxical to the massive emphasis on translation in the policy-research nexus.

I have shown that the discourse of translation is itself a performance—a spectacle—that distracts audiences from the content of claims and, instead, draws their attention to the claims' packaging. Ritualized spaces, shared AIDS-language, common buzzwords, tightly-knit networks and an increased call for translation across epistemological and social boundaries—each of these is presumed to enhance communication among increasingly diverse actors. However, even as forums for communication proliferate and encompass more kinds of expertise and experts, the difference among them necessitates more kinds of translation: researchers make their findings more "digestible" to policy makers; Malawian researchers pick and choose from the findings or reinvent them entirely when they translate them to in-country audiences; research projects seek to "disseminate" their findings to villagers. Each of these processes relies on socially and politically constructed "gaps" even as they claim to close the gap between research and policy. I suggest that even as evidence reaches wider audiences and is carried across a larger number of boundaries, it remains encased and unaddressed; a "culture" of AIDS research and policy has served to ritualize and standardize how evidence is evaluated and performed.

In this way, the "forms" of knowledge production—networking, conferences, committees—"effectively generate the effect of effectiveness" (Riles 2006:172). They work to condense and produce an "inside" from which actors solve social problems that appear to be necessarily "outside" of their forms. Further, the increasingly diverse actors who comprise these "insides" rely on translational strategies such as dissemination or networking to share and make knowledge intelligible across boundaries. However, although these strategies that are meant to close "gaps" appear to achieve successful

outcomes (more networking, more inclusive knowledge-making practices, more output of reports and other documents), they distract from the practices that take hold within the form.

This chapter, then, begins to explain the generally convergent "rhetoric" of AIDS that is shared by the actors described in this study. Amid a "virtually industrial production of massive numbers of position papers, conferences, diplomatic overtures, and assessments," we note the doctrinal reiteration of basic assumptions about AIDS, even if they change every few years (Falk Moore 2001:183). Evidence circulates and is translated, but it attains credibility primarily according to audience reception of its performance. Just as categories, risk groups and policy mandates create social worlds and identities among beneficiaries, so too do categories and terms such as evidence-based, policy relevance and networking create and enclose the worlds and roles of those in the policy-research nexus. The interests of people who comprise the social field of AIDS research are, in fact, embedded in the "forms" they construct around the problem of AIDS; as these forms attain legitimacy, so too do the kinds of knowledge produced within them.

Conclusion

The Production of Knowledge, Selves and Socialities in AIDS Research

In a context marked by widespread poverty,[374] few prospects for employment,[375] and increased economic inequality following failed structural adjustment programs, the suffering of the Malawian people has been exacerbated by the AIDS epidemic. In global terms, Malawi is not only one of the poorest and least developed nations (ranking 153rd out of 182 countries on the Human Development Index),[376] but also one of the most highly AIDS-infected countries in the world. Dire statistics such as these have brought about intensive interest in research and intervention that can improve the quality of life and mitigate the spread of AIDS.

Following democratization in 1994, AIDS has been an important contributor to the "NGOization" and "projectification" of the Malawian state (Rottenburg 2009). Expatriate-led academic social science research projects that administer household-based surveys and HIV tests are one cog in this system of knowledge production about the AIDS epidemic. However, while the first world actors involved in both colonial and post-WWII international health presumed an entitled right to enact bureaucratic surveillance of developing world health problems, contemporary global health norms place great rhetorical value on establishing meaningful partnerships between wealthy and poor nations that are unified in their production of policy-relevant knowledge that will improve the health of the poor (Mulenga 1999, Costello and Zumla 2000, Cooke and

[374] In 2009, 40 percent of Malawians were living in poverty (Vandermoortele and Bird 2011).
[375] 85 percent of Malawians depend on small-scale agriculture and 97 percent of farmers grow maize, with more than half of them growing no other crop (World Bank 2009).
[376] 25 percent of Malawians over 15 are not literate; 2.1 out of 100 people are Internet users; and life expectancy is around 54 years of age (UNDP 2010).

Kothari 2001).[377] These collaborative norms and the entrenched, if episodic, presence of research institutions in Malawi have opened definitions of expertise and enlisted a diverse set of actors into knowledge production projects.

Under the banner of global health, thousands of American and European medical students undertake "placements" in developing countries, philanthrocapitalists donate billions of dollars for good causes, pharmaceutical companies conduct clinical trials and research projects collect data. All of this well-intentioned involvement in places like Malawi, however, raises questions. To what extent should the global South become a "sample" for studying disease burdens in order to satisfy the needs of science to find new subjects and explore new problems?[378] As this study has shown, knowledge creation entails competing interests, exploitations and exchanges.

While anthropologists have drawn attention to the social and economic consequences of AIDS suffering, few have documented the everyday practices, contradictions and politics of producing AIDS-related knowledge in impoverished contexts. In the preceding chapters, I described the social relations, exchanges, practices and tensions that comprise international social science research in Malawi. Without diminishing the tragic suffering brought by HIV to Malawi nor the rearrangements of kinship networks and family structures it has necessitated, this study examined the ways in which research projects in Africa produce new socialities, new mobilities, new exclusions and inclusions and generate new configurations of expertise and evaluations of knowledge. AIDS research and other projects do not simply "impact" local social

[377] Though funding for research, including social scientific research in Africa, comes primarily from the global North, African researchers and contexts are not mere "clients" but have significant negotiating power and are "in demand." American HIV researchers are increasingly aware that funding bodies "look for" or mandate African partners on grant applications (Crane 2010).
[378] Janes and Corbett 2009.

worlds; they also rearrange and reconfigure them. How does knowledge, as a social and cultural product, interact with the local places and people in which it is transformed and transforms?

As a historical crossroads of health knowledge and healing practices, Malawi is a fitting site for exploring the intersections of contemporary knowledge production projects with existing knowledge, beliefs and practices. For example, resistance to AIDS research projects (captured in Chapter Two) is not a spontaneous reaction by research participants who do not understand the importance of giving researchers information, but rather it is rooted in a series of encounters between them and outsiders that have informed their expectations of reciprocity (cf. Chabal 2009). Blood and knowledge have long been conceived of as indices of wealth and health in Africa. What implications does this have for research projects? Chapter Three illustrates how valuation of rapidly collected data and measurements overlook important components of Malawian health as linked to the social body (missed by random sampling) or agricultural productivity. Researching or treating "AIDS" means de-emphasizing that which is not "AIDS." Further, the discussion of evidence-making and validation across sites and by diverse actors in Chapter Four and Chapter Five brings into focus the simultaneous rhetorical breaking down of bounded notions of expertise and the reinforcement of boundaries to maintain ownership over knowledge and status. Conceiving of the policy-research nexus as a dynamic social entity comprised of ever-evolving and overlapping epistemic virtues is useful for future considerations of the integration of health knowledge held by demographers with that held by traditional healers, chiefs, rural Malawians and physicians. Certainly, the knowledge assembled by the research projects discussed here does not just rise to the

surface to be "taken up" by villagers or policy makers; it is constructed through social interactions and practices that accumulatively measure it against what is already known and unknown, assess its moral, economic and social conditions of production and manipulate it to fit certain pragmatic needs and interests.

Further, it is my intention to consider health knowledge not only in the form of explicit claims about the AIDS epidemic, but also in terms of the claims made in informal spaces which also enlist evidence and seek validation by audiences: "I am a local expert." "We are over-researched." "We deserve more compensation for giving this information." "Villagers believe the silliest things!" Each of these claims materializes out of the crucible of AIDS research projects in local contexts and has some bearing on the organization, production, circulation and validation of AIDS knowledge. Finally, a performative approach to knowledge-making combined with multi-sited ethnographic study has enabled me to begin to trace how knowledge travels with (or in absence of) actors and, in turn, may be transformed or instrumentalized "downstream:" Consider the validation of Gift's claims about MSM and AIDS by transnational audiences and its deligitimation by Malawian audiences or recall Blessings' Powerpoint presentation that contained, in the opinion of his expatriate collaborator, "misinformation").

"Socializing" the Scientific

The episodic, yet increasing, presence of international research institutions is a growing trend across sub-Saharan Africa. While scholars have taken note of this "medicalization" of Africa and explored how global health categories and knowledge have become a resource for identity and citizenship projects (Nguyen 2010; Bornstein

Forthcoming), the everyday interactions of the large research community is less studied. While many people paint research practices as exploitative and view researchers as an autonomous epistemic community, I have shown how research is a social field rife with complicated and historically informed interactions and negotiations by a diversity of interested actors ranging from AIDS researchers to poor rural villagers. I focus on this social field's main sites—the "messy" interactions of survey research fieldwork, the sanitized space of the office and the downstream ritualized space of AIDS conferences—to illustrate that science travels not only from the first world to the global South but also between and within mundane sites in the latter. The recent proliferation of multiple kinds of global biopolitical projects—human rights, development, global health and food aid—indicates that social production of AIDS knowledge can apply to a range of knowledge formations across postcolonial contexts. Analyses such as this one can play an important role in demystifying the relations, for example, between stratification and hierarchy (material resources, geography, networks, power, control, prestige, influence) and evidence that people use to make sense of the world (culture, data, meanings, maps, models, numbers).

Global biopolitical projects offer a fruitful site for reconsidering some of the analytics that anthropologists of knowledge have long concerned themselves with—expertise, exchange, circulation and validation. The preceding chapters have outlined and reinvented—through the lens of a social constructionist and performative approach to knowledge—the meanings of expertise, the contradictions and asymmetries of exchange, the mechanisms of circulation and the role of translation in evaluating the evidence upon which policy is based. These are my goals in this study: 1) To nuance accounts of outside

actors and institutions in sub-Saharan Africa that traffic in dichotomous categories such as global/local, powerful/powerless and expert/lay by illustrating how these categories and the boundaries between them are artifacts of everyday knowledge-making practices; 2) To dissect and give color to "knowledge" by situating it as a performance within and across social contexts; 3) To elaborate some of the contradictions inherent in features of the new global health that are often uncritically taken to be "good"—collaboration, multiple definitions of expertise, international ethics standards and policy-relevant research; 4) To show how the so-called medicalization or projectification of sub-Saharan Africa in the wake of AIDS is a fruitful ethnographic site for exploring the emergence of particular socialities and subjectivities around and within sites of management, measurement and knowledge production.

Expertise

Calls for collaborative and participatory research and demands for rapid data collection "in the field" require expatriate researchers increasingly to rely on local experts to be their eyes and ears on the ground—to translate consent forms, to orchestrate logistical plans and to act as key informants who can quickly bring local knowledge to a global table. African intermediaries and translators have long occupied an important space between the local and the global in roles ranging from colonial-era enumerators to chiefs with roles in colonial indirect rule to translators for gentlemen travelers. Today, the entanglements of neoliberalism,[379] humanitarianism and the political economy of global health continue to produce a demand for labor in the form of knowledge brokers. In

[379] Harvey (2005) provides a brief history of neoliberalism; Lave *et al* 2010 discuss the effects of a commercial imperative on scientific research.

contexts of high unemployment, this demand attracts large numbers of "local experts" who play instrumental, if often muted or invisible, roles in knowledge production. These actors maximize their own (social and financial) capital gains by employing an array of tactics within the space of research jobs that may influence the kinds of data collected "in the field" or the uses of this data in the sites to which it circulates. Importantly, it is precisely through and within encounters between these local experts and researchers that "local knowledge" is defined and takes form. Chapter One focuses on local experts to expose the performances and practices that create this category of researchers and set its boundaries.

Exchange and Reciprocity

Since the colonial period, outsiders have been preoccupied with the question of how to appropriately compensate local people who surrender time, information or bodily samples to projects that claim to improve the public good. Whereas exchanges for information in African and other contexts were historically haphazard,[380] the rise of universal standards and guidelines for ethical human subjects research demand research that is consensual, non-coercive and compensatory. In a global capitalist context where data extracted from African bodies has become a sort of commodity, scholars, ethics boards and policy makers have begun to reconsider the ethics of exchange. What returns should human subjects expect from projects? What are the contours of obligation and reciprocity in exchanges between actors in asymmetrical positions? Mauss' writings on gift exchange in archaic societies apply equally to contemporary questions. How do

[380] Cf. Anderson 2008

research participants' expectations of exchange emerge from a long history of exchanges with outsiders? What are the consequences when outsiders create speculative economies of hope for healing or improvement among research participants? I have attempted to show how the soap-for-information exchanges of survey research are sites in which researchers and research subjects define and negotiate the meanings of giving, receiving and obligation.

Figure 6.1: Children chase after a research project minibus (Photo: Author).

Circulation

In a neoliberal moment when health is increasingly a commodity, what is accepted as good data is contested; often, debates and negotiations rely not only on traditional mechanisms of science but also on issues of what data are most amenable to

enlistment into the market.[381] Chapter Three illustrates how researchers in the policy-research nexus produce high quality data that achieves validity according to their epistemic virtues. Chapter Four and Chapter Five discuss what kinds of evidence count and exposes how the policy-research nexus is an entanglement of interests variously classified as humanitarian, market-based, transnational and national. These overlapping interests dictate that data must be mobile, consistent and trustworthy. Chapter Three focuses on showing how these standards for evidence operate on everyday social processes and people engaged in data collection. This Chapter serves as a platform for further exploration of how the intensified interlinking of numbers or measures and life or health may sideline broader, structural problems in favor of problems that are easier to measure and fix.[382] The privileging of the fieldwork supervisors' call for closed survey questions that make data collection more efficient over researchers' own preference, in some cases, for open questions, is a small example of the kinds of tensions that arise amid an urgent need for rapidly produced data that can circulate widely. Further, calls for policy-relevant research seem, at first glance, benevolent and imperative in impoverished contexts; yet, we might also consider the strictures of this demand and the way in which it places researchers in a straitjacket of recycled conventional approaches. I have tried to move away from simplistic critiques of researchers and other powerful actors in Africa to question instead the conditions and structures that limit their decisions, commitments and practices.

[381] Cf. Lave *et al* 2010.
[382] Kalofonos (2010), for example, illustrates how "feeding" malnutritioned or hungry bodies in Mozambique with ARVs can transform them into AIDS success stories that mute persistent and difficult structural problems. Biehl (2009), writing on the first universal rollout of ARVs in Brazil, depicts some of the inequalities and exclusions enfolded into this success story.

Validation

Anthropologists and others have widely documented concerns about first world partners overpowering or exploiting African collaborators or local participants in their projects; likewise, many have explored the ways in which well-intentioned projects can reproduce existing inequalities in local settings (Bornstein 2003, Hodžić 2006, Elyachar 2006, Englund 2006).[383] However, the preceding chapters suggest that "expertise" be rooted in everyday practices in order to show how categories such as "local knowledge," "good evidence" or the "grassroots" are not solid or stable but produced only through the interactions of outsiders such as survey research projects with actors in local contexts.

The preceding chapters also consider how evidence is validated or contested so as to bring together increasingly diverse actors with multiple and sometimes competing interests. Knowledge-making and exchange necessitate dissemination to or translation between different groups. The last chapter examines how mechanisms put in place to translate knowledge between researchers and policy-makers (collaboration and networking) and researchers and local people (dissemination) may serve to distract attention away from enduring structural conditions that limit dialogue. In attempting to "close the policy-research gap," these mechanisms for translation may reproduce pre-existing inequalities, solidify boundaries between actors and contribute to the validation of non-knowledge or non-innovative knowledge about the epidemic.

[383] Van de Ruit (Forthcoming) explores the production of inequalities by South African NGOs practices of recruiting local volunteers to implement their projects.

Cultures of Research

An emphasis on knowledge-sharing, partnership and participation amid rising trends of well-intentioned investment in global health by the donor community means that more and more postcolonial technoscientific projects and knowledge formations have taken up residence in sub-Saharan Africa. Dire statistics about AIDS infection amid tragic poverty create a state of exception that justifies humanitarian and scientific intervention and produces an enduring need for timely and high quality data. Yet, this exceptionalism and crisis often distract us from viewing the dynamic and evolving cultures of research as objects for social study. The term "research" has multiple definitions that act as entry points for further investigation. Rural Malawians claim that they are "over-researched." African scholars wish they had more chance to do "innovative, interesting" research. The state and international governance dictate that all research in impoverished contexts must be "policy-relevant." Young educated Malawians see research as an opportunity for employment. This study considered how social relations, rituals, roles and exchanges of and around research projects affect the production of knowledge about AIDS and reconfigure local social worlds and subjectivities. In the words of a research participant in southern Malawi: "Without research, everything is hiding."[384]

[384] Interview, Enock K., Chopi (Balaka District); August 25, 2008.

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