AIDS in Africa*

jimi adams
University of Colorado-Denver
jimi.adams@ucdenver.edu

CRITICAL-RETROSPECTIVE ESSAYS

**Introduction**

It would be a vast overgeneralization to suggest that the story of HIV/AIDS in Africa can be told in a single narrative. While the continent accounts for a substantially disproportionate share of the global population living with HIV or AIDS,\(^1\) the contours of the epidemic vary substantially across—and even within—its 54 countries. To make sense of this variation, researchers have devoted considerable attention to identifying the common and differential causal pathways of infection, barriers to treatment, and societal impacts of AIDS within African populations.

Traditional sociological perspectives have contributed to our understanding of the disease from relatively early on—for example, with a focus on social inequalities in HIV

---

* I am especially thankful for feedback from Ryan Light, Rachel Sullivan Robinson, Jenny Trinitapoli, and Sara Yeatman while preparing this manuscript. I also benefited from a very helpful discussion among colleagues on Facebook while identifying and winnowing the list of books to consider.

---

1 To avoid unnecessarily cumbersome writing, I follow the increasingly common convention of using the term “HIV” when explicitly referring to the virus, while using “AIDS” both to indicate the end-stage disease and as the catch-all term in lieu of repeatedly writing “HIV/AIDS.”

---

**References**

infection (for a review, see Heimer 2007) or AIDS-related stigma (Alonzo and Reynolds 1995). As the epidemic has matured, we have increasingly developed a reciprocal relationship between sociology and AIDS. In other words, while applications of existing sociological frameworks continue to shape explanations of AIDS, we have also seen the epidemic extend the boundaries of sociological theory and knowledge.

Here, I will follow a rough chronology to examine that reciprocity through recent developments in the sociology of AIDS. This chronology focuses on how we have come to understand the dynamic progression of HIV infection into AIDS in Africa, an understanding that has matured alongside the epidemic itself. I focus on four primary components: (1) the relatively recent reconstruction of a historical trajectory of AIDS's spread across the continent, (2) the primary explanations for HIV transmission patterns and corresponding prevention efforts, (3) the individual and collective experiences of people living with HIV/AIDS (PLWHA), with a particular focus on changes in population structure and the development and uptake of treatment regimens, and (4) the societal impacts of generalized HIV epidemics. Before getting into each of these elements, some descriptive overview will be helpful.

While initial scientific attention sprung from a cluster of cases in Los Angeles (CDC 1981), sub-Saharan Africa has consistently shown the highest prevalence and exhibited the most rapid growth in new HIV infections, until relatively recent plateaus (Bongaarts et al. 2008). Among the most quoted statistics about the global AIDS pandemic is that, as of 2013, while Africa accounts for only 15 percent of the world’s population, it is home to roughly 71 percent of those who are currently living with HIV or AIDS (UNAIDS 2014). This has led many to question why AIDS is so much more pervasive in sub-Saharan Africa (SSA) than elsewhere in the world.

Scholars from socio-medical, behavioral, and epidemiological perspectives have attempted to answer this question and have engendered frequent, intense debate about each perspective’s explanatory power. Early speculation—often used to motivate information-based intervention campaigns—posed a key role of ignorance (both passive and willful) in sustaining the epidemic. While seemingly plausible and providing a clear first intervention step, evidence in locales with high HIV prevalence demonstrates that “knowledge about the epidemic was not lacking” (Mojola 2014:7) because “there is little silence or denial” (Watkins 2004:674). The content of those conversations varies considerably, particularly between formal and informal contexts (Angotti et al. 2014). Below, I elaborate how these different understandings shape efforts for preventing HIV and caring for PLWHA. Others have claimed stigma is an important epidemic catalyst (Rankin et al. 2005). Here, the evidence is more mixed and will play a key role in the discussion below on societal impacts and treatment. Among highly informed populations, even those exhibiting low levels of stigma, the importance that a populace places on AIDS relative to other issues can shape the responses that they and their governments enact (Dionne, Gerland, and Watkins 2011). However, as will be shown in the sections below on prevention, care, and impacts, governmental actors and NGOs from outside the region heavily shape AIDS interventions across Africa.

### History & Disparities of HIV Infection

The human immunodeficiency virus rapidly replicates. This speed introduces numerous transcription errors into the virus’s genetic code circulating in the body of any infected person, and the “errors” are transmitted between individuals. This variability has generated major strain differences in the population of existing HIV. HIV-1, type M is the predominant strain globally, but consists of many finer distinctions. For example, subtype B prevails in the United States, while C is dominant in southern Africa, and high viral variability—within the M class—is the norm in many central African countries. This rapid replication also supports the occasional recombinant form, incorporating multiple mutations. While this insight is not particularly new, researchers have recently leveraged these strain differences to trace large historical
trends in the global HIV pandemic (Iliffe 2006, Pepin 2011) and have used fine-tuned viral phylogeny for reconstructing infection networks that underlie particular epidemics (Lewis et al. 2008).

In The African AIDS Epidemic and The Origin of AIDS, John Iliffe and Jacques Pepin each draw on historical and current HIV viral strain distributions as a means to date HIV’s transition to a human virus and to recreate the trajectory of its spread through human populations. Most researchers now accept that HIV-1 existed in low prevalence among humans by the 1950s in western equatorial Africa and, perhaps more remarkably, already displayed wide genetic variability. HIV-2 seems to have made the jump earlier; but because it is less virulent, its diffusion remained relatively constrained, keeping it mostly localized. Earlier cases of HIV were likely to have occurred by the 1920s but did not encounter conditions sufficient to produce widespread epidemics.

Among the first confirmed cases of HIV-1—by blood tests conducted decades later—is a case from Kinshasa, identified in a sample taken in 1959. The rapid viral diversification allowed researchers to reconstruct the pathways by which transmission seemed to have evolved from these origins. While Iliffe is a historian and Pepin a clinician and virologist, it is in tracing this historical trajectory that each of their respective works takes an increasingly sociological turn. As they trace the routes of epidemic growth across Africa—spreading mostly east, then south, from the initial human cases and accelerating in prevalence the further it went—the patterns by which HIV spread were markedly shaped by macrostructural characteristics that were distinct to the period. Primary among these were rapid demographic growth and cascading decolonization.

At the time of HIV’s first documented human infections, the population of SSA was continuing to grow more rapidly than anywhere else, at rates unseen in any prior period in recent human history. This rapid population growth coincided with continued and/or increasing levels of (male) cyclical temporary occupational migration. Additionally, this period saw the beginnings of slow but increasing urbanization across the continent. Riding on the backs of these population patterns, HIV traversed transportation and communication corridors across Africa, resulting in early concentration of the epidemic among urban—and to a lesser extent, more affluent—segments of the population. Complicating the effects of these population dynamics was a unique cluster of HIV’s characteristics—its relatively inefficient transmissibility, its slow virologic development, and the fact that many of its subsequent complications mimicked (though amplified) morbidity and mortality events that were common to the area (e.g., tuberculosis in SSA). The combination of these population dynamics and HIV characteristics contributed to a unique feature of the HIV epidemic’s spread across SSA: it was remarkably generalized before it was identified. Iliffe concludes that this unique constellation of factors explains how “the fundamental reason why Africa had the worst AIDS epidemic was because it had the first AIDS epidemic” (2006:58).

These initial conditions provided a baseline from which the virus was able to accelerate its expansion as it spread across Africa; AIDS research began to expand correspondingly. Researchers documented that this period of rapid expansion was increasingly marked by diverging prevalence and incidence patterns—for example, countries such as Niger and Burkina Faso remained relatively unpenetrated (prevalence at or below one percent), while some countries, especially in the south (e.g., South Africa, Swaziland, Botswana), had population prevalence estimates that exceeded 20 percent.

Three primary features were vital to documenting this period of HIV expansion: identifying when prevalence/incidence rates were going to plateau and/or reverse, altering the strategies for generating such estimates, and documenting the (changing) socio-demographic disparities of HIV infection. Incidence captures the rate at which uninfected members of the population

---

2 A “generalized” epidemic indicates one that has spread to more than one percent of the region’s population and typically is not restricted within small subsets of the population (e.g., “high risk” individuals).
become newly infected, while prevalence provides a point estimate of the proportion of a population currently infected with HIV—whether recently or from an old infection. These distinctions matter because their ability to capture epidemic dynamics across the period was changing due to concomitant changes in other features—especially treatment availability. Mathematical estimates of epidemic dynamics often focus on incidence, because the point at which an epidemic will cease growing (or begin to decrease) is identifiable from the reproductive rate of infection ($R_0$), which enumerates how many new cases each existing case is expected to generate (Anderson and May 1991).

Unfortunately, incidence is notoriously hard to estimate for many infectious diseases, especially HIV because of its features mentioned above. As a result, prevalence estimates have been much more common (Ghys, Kufa, and George 2006), despite simultaneously incorporating incidence and survival components. While survival post-infection without treatment is relatively consistently estimated at approximately nine years, as treatment regimens were introduced in this period, they substantially altered survival trajectories—also altering the relationship between incidence and prevalence (Hallett et al. 2008). These measurement difficulties notwithstanding, it appears that HIV incidence in SSA experienced a plateau/decrease during the mid-2000s. Over the same period, prevalence has plateaued, or increased at a slowed rate. These prevalence changes reflect a combination of the incidence trend and increasing availability of treatment—elongating survival possibilities for PLWHA.

A methodological sea change occurred during this period, which further complicated temporal trend interpretation—the transition away from relying on antenatal clinic data as the primary source of epidemic estimates. Early in the period of epidemic growth, the most ready source of testable samples was from women attending clinics at or before giving birth. Because of high fertility, this was thought to provide broad access to the potentially at-risk population—sexually active persons. However, as estimates increasingly relied on population-level estimates, it became clear that antenatal data was overestimating the scope of the epidemic (Mwaluko et al. 2007). This forced re-interpretation of longitudinal trends in the epidemic trajectory, and some modest (delayed) adjustments to estimates of when the plateau in new infections is thought to have occurred (Ghys et al. 2008).

While it was important to document how many people were (newly) infected, as surveillance efforts increased, it became increasingly apparent that socio-demographic subsets of the population varied considerably. I describe some of these differences now and examine their sources in the sections below on transmission/prevention and treatment. Gender provides perhaps the most entrenched HIV disparity in SSA—women have experienced elevated incidence and prevalence rates compared to men since the 1990s, a discrepancy exacerbated for those in the youngest age cohorts (Mojola 2014). While the early period demonstrated some infection preference toward those in advantaged socioeconomic positions, as the epidemic has matured, it has increasingly mirrored the disadvantages of poverty that are observed for most health outcomes (Farmer et al. 2001). Similarly, while genetic evidence points to early urban concentration, as the epidemic spread the urban/rural disparity shrank, and in a few cases even reversed (Iliffe 2006). Research has noted ecological-level differences by religion—for example, countries with more Muslims have lower prevalence (Takyi 2003). Translation of religious disparities into individual-level models has produced mixed evidence (Trinitapoli and Weinreb 2012). Religion is one among several factors that correspond to researchers’ shifting their focus beyond the individual level to identify community-level factors that generate these and other disparities.

### HIV Transmission & Prevention

HIV transmission in SSA occurs predominantly through heterosexual sex. There are two very different approaches for examining why this is so. Social-behavioral work tends to focus on the risk factors associated with potentially contracting HIV, while epidemiological work—especially that concerned
with formal modeling—asks about factors that influence the reproductive rate ($R_0$). While some factors similarly shape both, others influence risk factors or $R_0$ differentially. Take, for example, the fact that women consistently exhibit higher incidence and prevalence rates than men. From a risk-factor perspective this stems from factors that differentially elevate women’s risk; for example, biologically women are approximately two to three times more susceptible to infection than men from a single penile-vaginal sexual encounter (Fox et al. 2011). Thus, if comparing a man and woman who are each currently infected and exhibit exactly the same frequency of sex, with the same number of partners—from an $R_0$ perspective—the woman would be a less efficient transmitter of the virus, generating fewer secondary infections.

Sociological work on HIV gender disparities tends to focus more on gender-based risk factor differences. Sanyu Mojola’s *Love, Money and HIV* and Catherine Campbell’s *Letting Them Die* address several of the most consistently identified factors elevating women’s risk: age-heterophilous partnerships, “transactional sex,” and power differentials that reduce condom usage. In her ethnography of Kenyan women, Mojola argues that age heterophily in the formation of romantic partnerships—and its combination with “transactional sex”—substantially genders HIV risk factors. Age heterophily describes the pattern of women partnering with men who are (sometimes substantially) older. This differentially shapes HIV risk because at almost every age group, men’s HIV prevalence is lower than for corresponding women. Thus, by selecting older partners women are at substantially elevated risk of finding a partner who is already infected.

Romantic partnerships the world over often involve provision of gifts and money—more often from men to women (Poulin 2007). Mojola shows how such relational norms are governed by increasing globalization and commodification of wants among Kenyan women. As such, “transactional” partnerships are simultaneously likely to increase the number of partners women have, exacerbate the age heterophily described above (as age increases men’s capacity to support such relationships), and reduce the woman’s control over the sexual encounter (e.g., ability to require condom use).

Negotiating condom use has been frequently posited as a potential intervention, seeking to elevate women’s control over the risk they encounter within sexual partnerships (Sinding 2005). However, as Campbell notes in her account of a (mostly failed) community-level intervention among commercial sex workers in a mining community in South Africa, commercial sex workers often exhibit higher HIV prevalence than their clientele. In that case, condom promotion is more likely to reduce $R_0$ for women who are currently infected than to shape their risk factors. Despite the potential for protecting the clientele, Campbell echoes others’ findings (e.g., Tavory and Swidler 2009) that condom use is especially resistant to behavior change. The failure in the intervention Campbell documents was especially surprising given both its “harm reduction” orientation and its putative development as an intervention from within the targeted community.

---

5 Two qualifiers are important in using the term “transactional sex.” First, the practice is not unique to Africa. Second, each of the authors who employ the term “transactional sex” explicitly does not equate it with commercial sex work. Relationships involve exchanges, some of which are financial. In these books’ settings, the levels of existing disadvantage and ongoing gender inequalities compound the implications of the transactions. A quote from Decoteau helps illustrate this conceptual distinction from the literature, “It is important not to misconstrue the practice of ‘transactional’ relationships. They are usually enduring, they often involve cohabitation and the sharing of child-rearing responsibilities, and women exercise agency in choosing their partners... greater freedom than [in] marriage” (2013:180). Mojola goes even further to contend that financial exchange is perceived by many of the young women in her study as a vital component of romantic relationships, a perspective summarized as “love = sex provision” (2014:87).

4 This discussion slightly over-assumes the likelihood of individuals’ knowing their HIV status. I will address the implications of HIV testing more explicitly in the section on PLWHA.
Researchers have identified a number of other behavioral, biomedical, and socio-demographic factors that influence HIV’s risk factors and/or $R_0$; the incidence plateau described above is occasionally interpreted as improvements in one or more of these factors. An important behavioral aspect is concurrency, which is having multiple sexual partnerships that overlap in time (Halperin and Epstein 2004). Early modeling efforts demonstrated that small amounts of concurrency in a population could lead to rapid increases in HIV’s $R_0$ (Morris and Kretzschmar 1997). In other words, beyond how many partners one has, $R_0$ estimation also requires knowing how those partners are temporally sequenced. Part of the reason concurrency is thought to be especially important for HIV transmission is its unique virologic development. HIV is most concentrated in the human body—and therefore most likely to be transmitted—in the first several weeks after infection, then again once it has progressed to AIDS; between these, HIV has a long “latent” period where viral load is low and transmission much less likely (Wilson et al. 2008). As such, concurrency increases the likelihood that additional partners overlap with the early period of high infectivity just after infection—increasing the probability of a newly infected individual passing HIV along to their concurrent susceptible partners (Wawer et al. 2005). Concurrency is also one of the more misunderstood concepts in HIV literature; a number of researchers have modeled concurrent relationships as a risk factor for contracting HIV. This is an inaccurate use of the model, however, as concurrency only predicts elevated population-level risk and increases in $R_0$; it has no direct implications as an HIV risk factor beyond those associated with having large numbers of partners (Goodreau 2011).

Important biomedical interventions address reducing mother to child transmission (MTCT), circumcision, reducing other STIs, and identifying/eliminating iatrogenic transmission sources. Nevirapine provides an example of successful interdisciplinary integration that produced the development and increased accessibility of an effective means to prevent MTCT (Adams and Light 2014). Absent treatment, HIV-infected mothers can transmit the virus to their children in utero, during birth, or through breastfeeding (Newell 2001). Mothers’ primary treatment can have the secondary benefit of decreasing transmission probability before and after birth. Nevirapine was developed as a single-dose intervention that halves the likelihood of transmission at birth (Guay et al. 1999). Recent experimental evidence shows that male circumcision reduces both the risk of contracting (Auvert et al. 2005) and transmitting HIV (Wawer et al. 2009), though the subsequent scale-up of targeted circumcision interventions has not been without some controversy. Comorbid sexually transmitted infections other than HIV elevate HIV risk factors and $R_0$ (Pepin 2011), leading to the treatment of other STIs as a key component of community-based HIV interventions (Campbell 2003).

A longstanding biomedical concern has been what role, if any, iatrogenic sources have played in HIV transmission. Pepin contends—though he admits that direct evidence does not exist—that it is likely that early expansion of the virus was accelerated by the use of contaminated needles. Moreover, before a reliable antibody test was developed and put into widespread use, transfusions were an occasional route of infection (most notably among a cluster of orphans in Romania). A small group of researchers contends that the extreme inefficiency of the sexual transmission of HIV (approximately 1:1000 chance per coital act) points to a continued contribution of iatrogenic sources in sustaining the African pandemic (Gisselquist et al. 2003). However, the overwhelming consensus remains that in SSA, HIV is predominantly spread through heterosexual sex, though sexual transmission among men who have sex with men is receiving increased attention (UNAIDS 2014).

Part of the reason sexual transmission remains a difficult point of intervention is continued high (if decreasing) fertility in SSA, which has a complicated relationship to both population-level HIV prevalence and individual HIV status (Trinitapoli and Yeatman 2011). Within marriage, both men and women often share a strong desire for children, removing two primary HIV prevention recommendations (abstinence and
condom use). As Mojola details through some especially poignant quotes from her focus groups, this has led to an increased focus on marriage as a potential risk factor for HIV. Moreover, the cyclical migration patterns described above can lead men to provide bridges between HIV risk pools in their occupational locations and their home villages. Mojola shows how the changing nature of fishing communities around Lake Victoria sustains these dynamics on a somewhat accelerated time-scale.

Intervention efforts have increasingly focused on the community rather than individual level, with varying success. In Religion and AIDS in Africa, Jenny Trinitapoli and Alex Weinreb—recognizing SSA residents’ near-ubiquitous religious involvement—explore how religion influences HIV prevalence rates and associated behaviors. They find that religious organizations can substantially alter behavioral HIV risk factors (e.g., abstinence, fidelity, and condom use) and that increased individual religiosity is itself a mostly protective factor. Perhaps more intriguingly, they find that those community and individual-level protective factors combine in unique ways: in communities that are marked by low aggregate religiosity, there is virtually no individual-level association between religiosity and HIV status; but in highly religious communities, women’s increased religiosity is associated with lower HIV infection probability, while the relationship is inverted for men—that is, higher individual religiosity is associated with higher HIV risk. Religion is but one of several potential community factors that play an important role in shaping the HIV risk environment, and it will also factor into the discussions below of treatment and societal impacts. Another particularly promising community-level intervention coincides with what is known as venue-based sampling (Weir et al. 2003). The aim is to recruit clusters of individuals from places that are associated with higher population-level risk, rather than singling out individuals’ risk behaviors for designing interventions (Yamanis et al. 2010).

Living and Dying with AIDS

While it is not the focus of any of these books, AIDS has significantly reshaped African population dynamics. As with the epidemic itself, those changes vary widely. As mentioned above, without treatment post-infection survival is around 9 years (Ghys, Zaba, and Prins 2007). Since most HIV expansion in Africa occurred before broad treatment availability, life expectancy dropped substantially (approximately 16 years across Africa [UNAIDS 2006]). Those in their prime child-bearing years were the hardest hit (Heuveline 2004), leading to a vast number of AIDS orphans, who, on top of losing a parent, suffered other substantial life disruptions, such as forced residential mobility (Ford and Hosegood 2005).

Treatment offered the potential for ameliorating at least some of these effects. In much of the world, as highly active antiretroviral (ARV) therapy (HAART) became available in the mid-1990s, HIV rapidly transitioned from being viewed as a death sentence to being seen as a manageable chronic condition (Harden 2012). In HIV Exceptionalism and Ancestors and Antiretrovirals, Adia Benton and Claire Laurier Decoteau demonstrate the more complicated story of ARV accessibility in SSA—beginning with the delay of HAART’s availability in SSA by approximately a decade after its development (Crane 2013). While ARV scale-up has vastly improved—approximately 37 percent of PLWHA are now on treatment (UNAIDS 2014), the majority of PLWHA remain without access to treatment.

A person’s HIV status must be known to start therapy. The scale-up of HIV testing was therefore a key intervention point (Angotti et al. 2009), even before widespread ARV availability. The implication was that by knowing their status, people could take steps to protect their partners from infection. Moreover, to accelerate prevention of MTCT, testing became practically mandatory for receiving antenatal care in settings of high prevalence (Angotti, Dionne, and Gaydosh 2010). While HIV testing is now relatively widely available, roughly half of those

---

5 This raises the question of causal direction.

6 For a stand-alone review of Decoteau’s Ancestors and Antiretrovirals, see p. 654 [ed.].
infected still do not know their status (UNAIDS 2014).

Benton’s participant observation among AIDS NGOs in Sierra Leone documents an encouragement to “live positively,” an encouragement that leads to testing not only to provide access to treatment and support mechanisms and to protect one’s partner(s) from infection, but also to encourage (public) status disclosure as an aid to the continued reduction of AIDS-related stigma. However, her encounters within NGOs providing material and social support to PLWHA demonstrate that not all their members—not even necessarily those in leadership—embrace this perspective. Testing has also led to an increasingly common push to make treatment available sooner (i.e., not waiting until CD4 levels are below 200), as a means of reducing viral load in infected individual’s bodies (Crane 2013). This has the dual effect of elongating the symptom-free period for PLWHA and allowing “treatment as prevention” by reducing the probability of secondary infection(s).

Building from the concept of “biomedical citizenship,” Decoteau shows that simply making HAART available is not sufficient to guarantee its benefits for all who need it. While early claims posited African populations’ limited capacity to properly adhere to treatment regimens, this perspective has been resoundingly rejected, with adherence rates in audit studies in SSA having occasionally outpaced those in the United States and elsewhere. Additionally, with the introduction of generic drugs, treatment regimens have been simplified into a single pill combining the multiple drug cocktails initially necessary. Decoteau argues that biomedical citizenship is marked by the individualization of responsibility that has come with neoliberal policies in post-apartheid South Africa. Correspondingly, failures in treatment are often blamed on the individual’s shortcomings. This view ignores the fact that putatively “freely available” treatment still incurs costs in time and money for regularly attending clinics where the drugs are provided. Treatment therefore often reinforces, and exacerbates, SES-based infection disparities. Moreover, stock-outs of drugs are not infrequent at the underresourced facilities many rely on. Decoteau’s critique of saddling primary responsibility for ARV adherence on PLWHA leaves the distinct impression that treatment approaches—at least in South Africa—are not sustainable and, even if able to achieve wider coverage, still would not produce the optimal effects that they might with more comprehensive interventions.

Berkeley biologist Peter Duesberg’s HIV denialism is notorious; he believes that HIV does not cause AIDS, but that AIDS results from a simplex of other conditions which deplete the immune system. Former South African President Thabo Mbeki was heavily influenced by these ideas, leading to strong criticisms of South Africa’s weak response to AIDS during his presidency. This much is well known. But Decoteau also describes how Mbeki’s denialism has deep roots in other, often ignored, factors. In particular, the recent legacy of apartheid generated mistrust of postcolonial interventions from external governments and western NGOs that led Mbeki to view HIV-related “biomedicine as a racialized science masquerading as objective epistemology” (Decoteau 2013:91). In other words, avoiding western biomedical HIV interventions was one element (among many others that were more politically and ethically palatable) of his political strategy to fully establish South African autonomy (sometimes referred to as traditionalism). While critics have often
claimed this prioritized indigenous treatment approaches to HIV in lieu of biomedical ones, Decoteau’s evidence from PLWHA in the two settlements she studied reveals a common “hybridity” wherein individuals draw on both biomedicine and indigenous treatments to improve their health—for HIV and other conditions.

Biomedical and individual orientation to AIDS treatment is common in the literature, but many of these studies ignore a substantial element of AIDS’s consequences and care. The communities within which PLWHA live—disproportionately their kin—also regularly contribute to AIDS treatment. Several common examples: household members nurse PLWHA through opportunistic infections, community acquaintances assist with transport for obtaining medications, extended family members often bear the primary responsibility for caring for AIDS orphans. Jenny Trinitapoli and Alexander Weinreb (2012) also document the common practice of religious leaders simply visiting, spending time with, and offering assistance to members who are ill. In addition to the relevance of this form of care for the treatment of PLWHA, it also speaks to the ongoing discussion of AIDS-related stigma. In regions of high prevalence, estimates suggest that one (or more) in three households have been directly impacted by HIV, and many more engage in these forms of care. This has led some researchers to begin questioning how stigmatized HIV can genuinely be, if people are so regularly engaged with PLWHA—often by choice.12

**Societal Impacts of AIDS**

In addition to AIDS’s individual and population-level impacts, research increasingly documents how HIV has altered organizations and the political economy of the regions hardest hit by the epidemic. While religion’s influence on AIDS has increasingly captured scholars’ interest, especially in Africa, it also provides an example of how AIDS is altering organizations in the regions that are heaviest hit. For example—far from ignoring AIDS—Trinitapoli and Weinreb (2012) find that religious messages have increased their focus on sexual morality. Other common patterns (e.g., individuals switching their religious affiliation) have also changed trajectories over this same time period, though how directly AIDS is shaping those changes is less empirically clear.

The NGO-ification of AIDS-belt countries has been an increasingly documented—and critiqued—phenomenon (Watkins and Swidler 2012). In Benton’s *HIV Exceptionalism* and *Scrambling for Africa* by Johanna Tayloe Crane, the authors provide accounts—and detail the corresponding limitations—of how external donors and researchers have become increasingly central to AIDS responses in Africa. Benton describes how HIV has simultaneously become “the largest vertical public health program in history” (Benton 2015:x) and exceptionally horizontally integrated. Verticality in NGO organization indicates the top-down orientation of program prioritization, which in part stems from the external sources of funding; estimates suggest that as much as 40 percent of Africa’s health funding is earmarked for AIDS, largely because of its reliance on external donors (for example, The Global Fund sponsors approximately 20 percent of all HIV funding worldwide [Benton 2015:45–46, 27]). Benton also argues that this verticality can occasionally lead NGOs to see donors as the primary audience for their intervention efforts, rather than the populations for whom those interventions are developed.

The horizontal “creep” of AIDS interventions derives from well-intentioned holistic approaches to improve AIDS-related outcomes, addressing not just biomedical, but financial, social, and emotional aspects as well. This expansion has had the subsequent impact of making policies beyond medical care (for example, housing and food supplement programs) increasingly subject to the ebbs and flows of HIV donors’ resources and aims. Both Crane and Benton’s data include a prominent example: at times health clinics’ HIV-positive patients have access to common

---

11 Iliffe succinctly summarizes how important caring for AIDS orphans is often perceived to be: “for the family the shame of not caring is worse than the shame of AIDS” (2006:103).

12 By no means should this be read as indicating that AIDS-related stigma has been completely eradicated. I simply raise the issue to point out a recent uptick in scholarship questioning just how pervasive stigmatizing views and behaviors are in practice.
drugs (like pain killers) that are occasionally simultaneously unavailable to other patients in the same clinic who do not have AIDS. A particularly harsh critique of this horizontal expansion is the claim by some that HIV NGOs have become increasingly interested in sustaining their organizations’ funding and infrastructure, independent of the contributions they make. Benton shows that Sierra Leone, despite its relatively low population HIV prevalence of 1.5 percent, is subject to this exceptionalism and expansion, just like regions with much higher AIDS burdens.

Ample funding for AIDS research has had similar far-reaching effects, leading high-prevalence settings to be increasingly heavily researched (Biruk 2012). Crane’s book is embedded in a long-standing research project in Uganda that combines clinical treatment for research participants; she shows how this approach raises two potentially substantial limitations. First, research like this increasingly leads to the postcolonial reality that “laboratories take on new meaning as sites of extraction and exchange” (Crane 2013:106). That is, while research can contribute benefits for patient care, capacity building, and development, each of these elements comes with unique trade-offs that are complicated to negotiate—a tension that, Crane argues, “both generates and relies upon inequalities, even as it seeks to end them” (2013:7). For example, is the research project primarily there to gather data or to treat patients (see the stock-out example mentioned above)? And are the HIV-positive Ugandans involved in the project primarily conceptualized as research subjects, HIV patients, or individual members of a society heavily impacted by HIV (i.e., is their identity as HIV positive prioritized only by the project)? Second, many of the practices employed (clinical or research, technical, and infrastructural), were developed in the United States and other non-African locales.13 As examples, ARV treatment and attempts to develop an HIV vaccine are built on knowledge of HIV subtype B, which only accounts for a small fraction of global HIV cases and only negligibly factors into the distribution of African HIV.14 Moreover, Crane argues that once these humanitarian, postcolonial relationships are established, even if the specific elements are revised over time (e.g., a new focus on ARV development based on other subtypes) it is difficult for those who start out on the receiving end to subsequently attain fully collaborative partnerships.

Moving Forward
What has the reciprocal relationship between sociology and AIDS taught us, and where should these lessons lead us next? It is perhaps obvious, but worth reiterating, both that this review is not comprehensive and that much of what we have learned varies (sometimes considerably) across contexts. Given that caveat, I highlight a few aspects that are particularly common in the literature and/or were important contributions of the books examined here.

Like most health conditions, HIV exhibits a variety of socio-demographic disparities—differentiating both the risk factors for potentially contracting HIV and how many secondary infections each case produces. Women’s elevated prevalence and incidence compared to men is among the most documented—and growing—of these disparities. This contradicts the frequent refrain of focusing intervention efforts on a small number of women—especially commercial sex workers—thought to be catalysts of the epidemic. As generalized epidemics in SSA (and elsewhere) continue to become increasingly female-skewed, it will become even more important to develop broad-scale interventions that engage full populations and not just those presumed to

---

13 One example is recommendations for when to commence treatment that are based on CD4 counts. In settings where the capacity for measuring CD4 levels is rare, this points to a mismatch between existing recommendations and their potential implementation (Benton 2015:125).

14 Not only does this work focus largely on subtype B, but since the samples it relies on are built upon the earliest attempts to isolate the virus, the variants of subtype B that are actually in circulation in the population today are increasingly reflective of the strain being used in these efforts.
be “high risk” (Moody, Morris, and Adams 2014).

Epidemic dynamics across the world, or within any particular context, do not take place in a vacuum; whether a region’s epidemic expands or stagnates can be dependent on sexual behavior and partnering patterns in neighboring locales. NGOs and other foreign funding agencies have shown equally strong influences over the nature of AIDS responses in Africa.15 This provides another example of how postcolonial ties between Africa and western countries foster dependent relationships that externally shape governmental, financial, and health outcomes across SSA. Moreover, consistent with neo-institutional organizational theories, these externally developed strategies frequently encounter difficulties of improper fit when attempting to integrate into African contexts.

Sociological and demographic research has produced numerous models accounting for social influences on health. AIDS provides a compelling case for expanding this view to not only consider health as an outcome, but also as a “right-hand side” variable shifting social norms. The examples in Trinitapoli and Weinreb of AIDS’s impact on religion and Benton’s description of horizontal NGO expansion from HIV services provide exemplary models for future work to consider such effects.

In addition to the important ways sociology and AIDS have each shaped our understanding of the other, these books also point to a number of unanswered questions for each. Scientific research funders, especially for health, have increasingly prioritized interdisciplinary approaches to solving our most pressing problems. HIV/AIDS provides a theoretically rich case for leveraging those benefits, as scholars from across the social, physical, and biological sciences, along with practitioners of many stripes, have perspectives that uniquely inform AIDS understanding and response. However, these efforts have thus far demonstrated mixed results; for example, integration underpins some important developments in PMTCT, while sustained disciplinary segmentation between clinical and basic researchers hampers our reactions to newly developing drug resistance (Adams and Light 2014). This provides an important critique for literature on interdisciplinarity, encouraging a more carefully theorized perspective on how and when we can expect to best reap its benefits.

While some improvements have been observed (like transmission plateaus), SSA is far from out of the woods with respect to AIDS’s societal impacts. This combined with the postcolonial structure of many of the responses documented above raises the question of how “weak” states can (continue to) improve their responses to HIV/AIDS. Even as new cases decline, increasing treatment availability may lead prevalence figures that have plateaued to begin growing again as PLWHA survive longer (Bongaarts et al. 2008). How can the states where this occurs learn from previous responses to build the infrastructure necessary to care for these populations? Their responses must address AIDS-specific aspects such as access to care, but also should ensure—as PLWHA increasingly transition to experiencing AIDS as a manageable chronic condition—that they have the opportunity to remain (or become for the first time) fully participating citizens.

Given the patterns described above, the role of external governments and NGOs will likely need to be reconceptualized to optimize these possibilities.

A final practical consideration is how much of what we have learned about HIV transmission and AIDS responses in SSA will be usefully portable to other contexts. Because of disproportionate AIDS prevalence in SSA, it has also been more thoroughly documented than most epidemics elsewhere. There are other regions (e.g., portions of Eastern Europe and Central Asia) and segments of other populations (e.g., African American women) where epidemic growth has not plateaued, or is even increasing (UNAIDS 2014). It remains to be seen whether these settings can leverage the proximate causes of gender disparities to reduce the rate of epidemic spread, or if the NGO-ification of AIDS responses will be echoed

---

15 This should not be interpreted as minimizing the importance of the many bottom-up HIV intervention strategies (Watkins 2004), but merely indicates the comparative organization and deeper resources of external efforts.
elsewhere—or if it can be improved upon. At minimum, the lessons from SSA seem worth bearing in mind as we continue to develop and scale up responses elsewhere and continue to seek improvements in Africa.

References


“I’m the father of ethnomethodology?” . . . “Yes, and everybody knows the ethnomethodologists are a bunch of bastards. But nobody knows whose bastards they are.” (Garfinkel 2007a:13)

In 1974, a paper by Paul Attewell entitled “Ethnomethodology since Garfinkel” appeared in the journal Theory and Society (Attewell 1974). I was a student of Harold Garfinkel’s at the time, and I recall that he was not at all pleased with Attewell’s article, partly because he read the title to imply that his career was effectively finished and that others were now carrying on with his project. Garfinkel lived for another four decades and continued to write until nearly the very end. When he passed on in 2011, he left a series of unfinished projects for his intellectual heirs to take up. Although much of what he wrote remains unpublished, several collections of his papers have come out in recent years. Of these collections, the one that I shall discuss in this essay (Garfinkel 2002) contains papers drafted after Studies in Ethnomethodology (Garfinkel 1967), while two others (Garfinkel 2005; 2008) contain previously unpublished papers written prior to Garfinkel’s launching of ethnomethodology in the 1960s. I also shall discuss four books that in one way or another take up Garfinkel’s legacy: one (Hutchinson, Read, and Sharrock 2008) is a philosophical essay that takes stock of ethnomethodology’s treatment of social actions, and the other three (Livingston 2008; Liberman 2013; and Sormani 2015) are recent efforts to exemplify Garfinkel’s program.

For reasons of space, I will not discuss recent developments in conversation analysis (CA), a field that once was closely linked to ethnomethodology (Garfinkel and Sacks, 1970) but which has become increasingly autonomous. Current publications in CA often include citations to Garfinkel that acknowledge him as a progenitor of the field, and a degree of overlap and mutual respect continues between those who work more or less exclusively in ethnomethodology or CA; but CA has become a field of its own with links to numerous other lines of research in sociology, linguistics, and communications. Current research in CA is more coherent and professionally visible than the various other lines of work that continue to identify with ethnomethodology; but for social theorists and philosophers who engage with ethnomethodology, as well as scholars in transdisciplinary fields such as science and technology studies...
Garfinkel’s writings continue to be the main focus of discussion and criticism (see, for example, Latour 2007:13). Despite his efforts to emphasize that his own work is bound up with that of a collective (a “company,” as he called it), Garfinkel is often treated as an individual theorist occupying a rather isolated space.

Each book discussed in this essay presents a distinct version of ethnomethodology. Although it is a fool’s errand to take up Garfinkel’s writings without also becoming acquainted with his philosophical sources—particularly those identified with European phenomenology and existentialism—his “followers” do not simply place themselves within a single philosophical or theoretical lineage. In fact, they invoke quite different combinations of sources, and make use of them distinctively. Each of the authors I discuss here, including Garfinkel himself, acknowledges ethnomethodology’s debt to phenomenology, but different authors emphasize other sources as well, such as Durkheim’s sociology (Rawls 2002), semiotics and critical theory (Liberman 2013), and Wittgenstein’s (1958) later philosophy and Peter Winch’s ([1958] 1990) philosophy of social science (Hutchinson et al. 2008). Three of these authors discussed below present detailed investigations in line with Garfinkel’s initiatives (Livingston 2008; Liberman 2013; Sormani 2014), but they also develop them in distinctive ways.

The differences between explicit efforts to take up Garfinkel’s program can lead us to rethink the temporal and epistemic relationship between fundamental ideas and later works in a theoretical lineage. Instead of a line of influence running from founder to followers, we might think of it as something that is constructed backwards, as later works retrofit their arguments and investigations by documenting their relation to prior works. That is, rather than stepping into well-marked historical streams of intellectual influence and carrying them forward, current works retrospectively articulate their literary ancestries. To borrow Garfinkel’s (2007a:13) line quoted in the epigram above about his being “the father of ethnomethodology,” we might view such lineages to consist of bastards all the way down. This way of thinking of philosophical and theoretical influences actually is in line with Garfinkel’s (1967: Ch. 3) conception of the “documentary method of interpretation,” and it is also related to his advice on “misreading” philosophical sources as if they were providing instructions on how to elucidate the practical organization of the mundane, everyday activities that ethnomethodologists investigate (Garfinkel 2002:112; 2007b:17–18). Such retrofitting applies not only to the interpretation of textual sources on an ethnomethodological reading list, but also to an open-ended array of other sources. This, of course, is contrary to the image of an academic literature as a basis for framing empirical observations and analyses, but I would argue that it more accurately describes the practice of finding “sources” in the midst of research.

Two Bibliographies

In seminars during the mid-1970s, Garfinkel announced to his students that there are two distinct ways to make sense of his program, each with its own bibliography. One is to write scholarly books and articles about ethnomethodology, to explain it to readers with a limited acquaintance with research in the field, and to integrate Garfinkel’s ideas with those of others in broader areas of social science research and scholarship. Examples of the former are Anne Rawls’s (2002) introduction to Ethnomethodology’s Program and Dirk vom Lehn’s (2013) recent intellectual biography of Garfinkel. The other way is to conduct studies of what Garfinkel called “naturally organized ordinary activities.” Doing the latter would not be a matter of applying Garfinkel’s ideas, but instead working out what those ideas are (or could possibly be) through a deep, first-hand engagement with specific practices performed outside the library (though organizational practices performed in the library are also available for investigation). The latter alternative involves a reflexive application to Garfinkel’s own writings of his advice to “misread” philosophical sources. Although Garfinkel was generous with his time in giving tutorials to individual students, often lasting for several hours per session, he also suggested that they should seek tutorials from the fields of practice they studied (see Garfinkel 2002, Ch. 4).
not only as a prerequisite for participant observation, but also as a platform for “respecifying” fundamental themes in philosophy and social science methodology (Garfinkel 1991). If such studies were successful, he suggested, they would not only produce singular contributions to sociology (or to ethnomethodology as an academic subfield of that discipline), but they also would create “hybrid” forms of investigation that would be recognized and credited within the relevant practices and pedagogies of mathematics, law, music, or technological design. Although Garfinkel vigorously promoted such studies and expressed indifference toward studies about ethnomethodology, he also recognized that the latter included much of his own writing, and that the balance of risks and credits within the academic profession weighed heavily in favor of erudite explication rather than esoteric investigation. The promise of such hybrids was not that they would expand the domain of professional sociology, or even of ethnomethodology as a subfield of sociology, but that they would create novel fields of investigation within and between a large variety of other academic and practical disciplines.

Like the other collections of Garfinkel’s papers published in 2002 and afterwards, Ethnomethodology’s Program was edited by Anne Rawls1 and included a lengthy editor’s introduction on Garfinkel’s biography and the development of his key ideas. The chapters written by Garfinkel include some autobiographical reflections and programmatic discussions of general themes and concepts, as well as presentations of several studies that he and various collaborators performed during the decades following the publication of Studies in Ethnomethodology. Rawls’s introduction and footnotes provide explanations of some of Garfinkel’s terms and arguments—something he often was sparing about doing in his own writings. She worked actively with him on the book (which must have been no easy task), and so readers can assume that he supplied much of the biographical information and approved what she says in the introduction, though he certainly would place it in the “about ethnomethodology” bibliography.

Durkheim’s Legitimate Heir?

Perhaps the most surprising aspect of Garfinkel’s Program, for readers who consider ethnomethodology as an anti- or non-positivistic perspective on social actions, is the relationship to Durkheim’s ([1895] 1982) rules of method professed by the subtitle Working Out Durkheim’s Aphorism. Garfinkel attributes to Durkheim the “aphorism” that “the objective reality of social facts is sociology’s fundamental principle.”2 Far from distancing his own project from this vision of sociology, Garfinkel appears to endorse it, and to propose further that ethnomethodology has taken up the program that Durkheim initially proposed; a program that had been

---

1 An exception is a special issue of the journal Human Studies, devoted to a long essay by Garfinkel (2007b), edited by George Psathas and introduced by Garfinkel and Ken Liberman. Garfinkel’s essay is a raw and unfinished manuscript that covers some of the same ground as his 2002 book. Garfinkel again invokes Durkheim’s aphorism, but gives much more emphasis to the relation of ethnomethodology to Husserl’s phenomenology. The manuscript is perhaps the most obscurely written of Garfinkel’s publications to date, not only in the way it is written but also in its numerous unsubstantiated claims and allusions to unpublished writings and further inquiries that would substantiate those claims. Garfinkel’s age (90 years old at the time of publication) should be taken into account by readers who venture to wade into the manuscript, and like all of Garfinkel’s writings there are occasional gems to be found in it.

2 This is Garfinkel’s paraphrase of a line from the Preface to the Second Edition of The Rules of Sociological Method, where Durkheim addresses particular criticisms of the earlier edition by arguing that they arise from a refusal to admit “our basic principle, that of the objective reality of social facts” ([1895] 1982:45). Durkheim adds that “social phenomena, although not material things, are nevertheless real ones requiring to be studied.” He adds further that “their mode of existence is constant,” and that “they possess a character independent of individual arbitrariness,” and that the “path to science” is blocked by the “anthropocentric postulate” that resists acknowledgement of the collective forms that subject individuals to their independent force (pp. 45–46).
neglected in the social sciences for the better part of a century. However, Garfinkel makes a slight change to the wording of the aphorism, replacing “principle” with “phenomenon” (2002:66), and this small modification makes all the difference in the world. No longer a general truth for founding a social scientific discipline, the “objective reality of social facts” would become a topic of investigation. In an unpublished paper, Garfinkel (1976:74) presented a more elaborate revision of the aphorism: “The objective reality of social facts, in that it is every society’s local, practical, situated, ongoing achievement, is sociology’s fundamental phenomenon.” The key question, then, is how routine activities such as driving in traffic, queuing for service, or conversing intelligibly in sequentially organized ways produce “the objective reality of social facts.”

When he recites the aphorism, Garfinkel sometimes seems to take seriously that ethnomethodology is Durkheim’s prodigal science—“heir to Durkheim’s neglected legacy” (Garfinkel 2002:94)—but he also makes clear that he is not taking the aphorism at face value, as a proposal to identify objective social facts and explain them scientifically. Without denying the objectivity of social facts—their externality, independence of individual will, and persistence across production cohorts—Garfinkel adds that these facts are treated as facts in everyday life, and that they are so treated is essential to their production. The addendum that Garfinkel supplied to Durkheim’s aphorism not only places Durkheim’s objective facts in the domain of ordinary actions; it subverts Durkheim’s professed ambition to develop an autonomous social scientific discipline whose professional methods would produce certified knowledge that would supersede the partial, ad hoc, and unexamined prenotions of common sense (Lynch 2009).

Garfinkel’s writings and current research in ethnomethodology and conversation analysis remain ambiguous about just how to address the innumerable lay and professional practices that constitute (that is, collectively perform and produce) objective social phenomena. As Hutchinson, Read, and Sharrock (2008:109) observe, “[t]here are at least two conflicting ways in which ethnomethodologists can think of their inquiries,” both of which are suggested, and to an extent exemplified, in Garfinkel’s Program. One is as the legitimate heir to Durkheim’s program, which is to “think of ethnomethodology as a first step in the direction of a genuine social science, [but] one which differentiates itself from sociology at large . . . in being the only branch of sociology that addresses itself directly to actual and observable occurrences in and of the social order” (ibid.). This would be an alternative to existing sociology in the sense that it would get closer to the “data” of actual social life than is possible through surveys, models, and other formal-analytical methods.

The other way to think about ethnomethodology is not to view it as a more rigorous empirical sociology, developing novel findings about the concrete details of actual social practices, but as an “alternate” form of investigation that shadows formal analysis wherever it is found within and beyond the academy. Formal analysis, in Garfinkel’s terms, includes but is not limited to the various theories, methods, and analytic practices in sociology that attempt to develop robust generalizations about the organization and structure of social actions. Formal analysis also includes more workaday examples such as plans and rules, navigation maps and route directions, classroom pedagogies, and recipes and manuals for countless activities. The ethnomethodological alternate is bound up with how such formal analytical tools are composed and deployed in practice. Ethnomethodological investigations can be likened to efforts to delve into tacit knowledge (Polanyi [1966] 2009), or to elucidate the grammar of “knowing how” in contrast to “knowing that” (Ryle 1945), but they do not necessarily confront a coherent domain; instead, Garfinkel’s “alternate” is bound up with the specific formal investigations of practice with which it is paired in any given case. When focused on sociological methods, Garfinkel’s alternate can seem to take on a distinctively subversive character. Alongside studies of the practices of jurors, coroners, and suicide prevention counselors, in Studies in Ethnomethodology Garfinkel presents investigations of sociological practices of coding interview responses and analyzing clinic records. When he focuses on sociological methods, it is difficult not
to detect irony and criticism in his discussion (1967:23) of the ad hoc practices that produce analyzable data. However, Garfinkel denies being critical of social science methods, because cases such as coders’ ad hoc practices simply are instances of “instructed actions” of interest to ethnomethodology, and the analytical stance he takes is no different from the one he takes toward the (often aggravating) difference between, for example, the instructions in an automobile repair manual on how to tune an engine and a specific effort to do so with available tools. The sense of criticism arises from an adherence to the widespread presumption in the social sciences that rigorous procedures should adhere strictly to methodological protocols rather than trusting interviewers, coders, and other low-level functionaries to exercise commonsense judgment when conducting research practices, identifying equivalencies and maintaining stable analytical categories (Suchman and Jordan 1990).

Hutchinson et al. (2008) also make an insightful suggestion about the way Garfinkel tends to “destabilize any settled understanding of his work,” and even to “repudiate some of his own prior stances, and the attachment other ethnomethodologists might show to them.” Viewed in this way, “ethnomethodology has a subversive, rather than constructive role, [and] is ultimately subversive even of its own apparently constructive contributions” (p. 109; also see Liberman 2013:3).

Consequently, it seems fair to say that when Garfinkel proposes to work out Durkheim’s aphorism, he is doing so through a deliberate and subversive misreading. Such a misreading is a matter of understanding Durkheim retrospectively, and even perversely, in light of ethnomethodological projects at hand. Garfinkel (2007b) is explicit about misreading Edmund Husserl retrospectively, in light of a research program that Husserl neither foresaw nor would likely have countenanced, while at the same time drawing deep insight from Husserl’s writings. Among other things, Garfinkel’s misreading of Husserl shifts from a post-Cartesian preoccupation with the ego cogito to a later-Wittgensteinian insistence on public language-use rather than private mentality as the site and source of intelligibility. Although Garfinkel does not explicitly say he is misreading Durkheim’s aphorism, his reading entails a profound shift from treating the objective reality of social facts as a hidden order, the discovery of which awaits the development of a new science, to treating social actions as public endeavors that reflexively and evidently produce objective social orders in a way that eludes the formal procedures of a social or administrative science.

Hutchinson, Read, and Sharrock (2008) mainly discuss Peter Winch’s ([1958] 1990) philosophy of social science, but they devote a chapter to ethnomethodology where they shed light on some puzzling aspects of Garfinkel’s legacy. Their provocative title There is No Such Thing as a Social Science is not meant to imply that the academic disciplines that call themselves social sciences do not exist; instead, following Winch, they suggest that sociology and other social sciences are sciences in name only. They also argue against what has become an accepted position in many of the “qualitative” social sciences today, which is that all observation, and therefore all description, is “theory-laden,” and that “it is impossible to recognise anything save through some theory” (Hutchinson et al. 2008:92). The difficulty they face when attempting to argue against this dogma is that many readers will dismiss the denial of the necessity of theory as itself the expression of an implicit theory. However, what Hutchinson and his colleagues deny is that a competent understanding and engagement in social practices implies the possession of a coherent cognitive framework (whether conscious or not) that theoretically informed analysis can articulate. Typically, any attempt to deny that ordinary action and scientific understanding are theory-laden is ascribed to behaviorism or naïve realism, and some proponents of ethnomethodology and conversation analysis do indeed seem to embrace an atheoretical, inductivist, and realist approach to “actual” or “real world” social activities. However, Hutchinson and his colleagues suggest that, with some ambivalence, Garfinkel foregoes the dream of a science, with its abstract methodological dicta and generalized concepts, in favor of a research program that is grounded in the
intelligibility of the particular practices it studies. Such grounding does not make theoretical or philosophical literatures irrelevant; instead, it treats literary “sources” and their generalized “concepts” as contingent means for articulating insights gained by other means. Hutchinson et al. (2008:94) further acknowledge that their own understanding of ethnomethodology “is not one that would necessarily recommend itself to many ethnomethodologists.” In order to appreciate what such an understanding might involve, let us turn to three books, each of which presents particular studies that exemplify Garfinkel’s program.

Instructed Actions

A starting point for understanding Garfinkel’s approach to social practices is what he calls “instructed actions.” This theme covers an immense range of commonplace and specialized actions performed in accordance with rules, recipes, manuals, directions, maps, and plans. In science studies, dating back at least to Peter Medawar (1964) and Michael Polanyi ([1966] 2009), the point has been made repeatedly that formal accounts of method (such as in textbooks and the methods sections of published articles) do not (and ultimately cannot) give accurate accounts of how science is performed in practice, and therefore cannot give an adequate account of how to replicate experimental results. Medawar provoked interest by suggesting rhetorically that the scientific paper is “fraudulent” because of the way a typical methods account in a scientific publication presents a contingent and uncertain process as though it is an orderly progression of steps leading to a reliable result. Polanyi’s concept of tacit knowledge avoids any suggestion of dishonesty and instead emphasizes the necessity to cultivate skills through apprenticeship—skills that ground the authoritative knowledge of science but which must be accepted on trust because they cannot be conveyed by words or formulae.

Garfinkel sets up his treatment of instructed actions by mentioning familiar discrepancies between formal accounts and actual practices (“actual” in the sense that they are performed on specific occasions). But, unlike many constructivist and post-constructivist theorists, he does not treat such discrepancies as a basis for raising general questions about the role of replication in science, critiquing “positivist” accounts of scientific truth, or arguing that cultural assumptions and political machinations are hidden by objective accounts. Instead, he orients to a distinctive variant of the hermeneutic circle: the embodied use and continual re-reading of instructions in the course of a singular practice. The circle is less of a vicious one, and more of a journey in which initial instructions are informed (and occasionally confused) by unanticipated events, elaborated through interactions among fellow travelers, and made intelligible in relation to unfolding scenic particulars. The aim is not to reveal the limitations, lacunae, and biases “contained” in formal accounts, but to describe how they are read and re-read in relation to scenic particulars on specific occasions.

Livingston (2008) and Liberman (2013), both of whom were students of Garfinkel’s, present his teachings and exercises on instructed actions, and they also present distinctive cases and analyses of their own. Livingston delves into the philosophy of mathematics as well as some of Garfinkel’s favored phenomenologists, and Liberman delves more deeply into phenomenological sources and supplements them with selected writings in semiotics and critical theory. Livingston has a long-standing interest in the practices of mathematicians, and his mathematical interests and abilities inform both the content and methodology of his research. Liberman’s understanding of phenomenology also is inflected by a long-term ethnographic study, in this case of Tibetan monks and their debating practices.

Liberman’s book is partly a presentation of his own notes from Garfinkel’s seminars in 1979–1980, and it includes a number of case studies that develop pedagogical exercises originally assigned by Garfinkel on wayfinding, navigating in traffic, and playing games with rules. In addition, Liberman devotes several chapters to his...
own long-term ethnographic research on debating practices among Tibetan monks, and he also presents a current project on the cultivation of taste and judgment among professional coffee tasters. Livingston also presents a series of exercises, drawing upon Garfinkel’s investigations of driving in traffic, the embodied production of service lines (queues), and games with rules, and supplements these with his own original research on solving mathematical problems, reading poetry, and folding paper in accord with origami instructions.

In his book, Liberman alternates between presenting descriptions of practical exercises and reflecting on the cases in light of Garfinkel’s teachings, as well as the writings of phenomenologists, semioticians, and critical theorists, including Husserl, Merleau-Ponty, Levinas, Derrida, and Adorno. One exercise he assigned to his own students was a matter of following “sketch maps,” akin to what Psathas (1979) and Garfinkel (2002:129) call “occasion maps” (also see Brown and Laurier 2005). These are hand-drawn maps for guiding a driver (or in some cases a pedestrian) from a point of origin to a destination. In the seminars that Liberman attended in the 1970s and ’80s, Garfinkel assigned an exercise to students in which he instructed them to closely describe the practice of following a particular sketch map on a singular occasion. The exercise was a touchstone for elucidating the practical difficulties, contingencies, and improvisations involved in reading and following a map, and it also provided materials for broader reflections on maps and other forms of instructed action. By drawing upon sources in semiotics, as well as upon previous ethnomethodological studies of wayfinding with maps and directions, Liberman focuses closely on the practical difficulties his students encountered when reading the specific features of a sketch map in relation to the local geography of streets and landmarks. He argues that “it is really inaccurate to say one is ‘following a map’ since whatever may be there to follow is necessarily the handiwork of the person who is reading the map” (p. 50). In other words, the details of a sketch map, including its faults, come to life in the course of a journey. Liberman provides numerous examples of exchanges among his journeying students about whether to “count” an alleyway as one of a series of intersections marked on a map, or whether a named street on the map is the “same” as an unsigned street they currently are crossing. Particularly interesting are the ad hoc practices he describes for getting past seemingly intractable difficulties with finding where the current place in the journey is “on the map” (assuming that the journey has not left the map altogether). Liberman (p. 69) quotes Brown and Laurier (2005) on the “solution” to a dilemma: “Ah well. Let’s just drive, see what happens.” Another ad hoc practice—“Wait and see”—is illustrated by a squiggly line on a map that the wayfinders find puzzling, while they nonetheless forge ahead, trusting that it will become clear when they get to the appropriate point in the journey.

Liberman also reflects upon some of the distinctive metrics and geographical features of sketch maps. In one of the sketch maps he discusses, Interstate 5 in California is aligned as a vertical axis (p. 57) along the middle of the map. In another, junctures between a road and a series of intersecting streets provide a metric of distance. These substantive details become part of the “mathematics” of the map, and there is no separate matrix of longitude and latitude, and no separate scale showing distances (also see Vertesi [2008] on the London Underground map).

Livingston’s book is a compendium of exercises that exhibit a distinctive style that has run through his research over the past thirty years, starting with his analysis of proving theorems in mathematics (Livingston 1986). The exercises are distinctive in the way they invite (and even require) readers to follow along and to use their own efforts in distinctive material domains to understand how to characterize constitutive actions in those domains. His “ethnographies of reason” are not descriptions of particular scenes or analyses of transcribed recordings of practitioners performing singular instances of, for example, working

---

4 Garfinkel (2007b:15) asserts that queues “are to sociology what fruit flies are to genetics: a topically rich frequently used pedagogical case.”
out a proof at a blackboard (as compared, for example, to Greiffenhagen and Sharrock 2011). Instead, he presents formal problems, such as a specific position on a checkerboard and a series of moves that follow; a series of diagrams, notations, and commentaries in a geometric proof; a sequence of folds to be made for an origami construction; a step-by-step description of his own efforts to perform Galileo’s experiments with pendulums; and a diagram of a four-way stop together with his own observations on the interactions among the vehicles that encounter one another at the intersection. There is a family resemblance between Livingston’s examples and David Sudnow’s ([1978] 2001) reconstruction of his own mastery of playing improvisational jazz at the piano. In both cases, the author’s written presentation invites the reader to participate in the described performance, at least at an elementary level, in order to understand what the text describes. In a concession to readers who have limited competency with the particular practices he describes, Livingston presents elementary examples, rather than more demanding examples from mathematics and other practices. His writing takes the form of a manual, but it does not aim simply to teach readers how to play checkers or to solve mathematical problems. Instead, performing the exercises opens up the material circumstances for a first-hand explication of the embodied work with checkers on a board in the one case, and with chalk at the blackboard in the other.

In one of his exercises, Livingston (2008, Ch. 27) follows previous efforts by Garfinkel (2002, Ch. 9) and Dusan Bjelic (2003, Ch. 6) to perform experiments (which historians sometimes prefer to call demonstrations) described by Galileo. There are pedagogical advantages to using Galilean experiments, such as those with the inclined plane and pendulum: Galileo’s written accounts of the experiments are compact and open-ended; the equipment is relatively simple and inexpensive (and yet challenging for a novice to set up); and the experiments can be performed fairly quickly and with some success. In these cases, however, the aim is not to replicate Galileo’s original experiments (assuming that he even performed them), or to gain insight into Galileo’s practices in the context of early seventeenth-century Florentine court society. Instead, the aim is to use the present-day performance—regardless of how anachronistic it might be—in an effort to bring under examination how a physical “law” (not an eternal truth, but a particular mathematical formulation of a physical regularity) can be made observable through an embodied practice. Galileo’s law of the pendulum, as Livingston expresses it, is that “the period of a pendulum is proportional to the square root of the length of its cord” (p. 229). Livingston, with his interest and background in mathematics, takes a very different approach to the pendulum experiment than Bjelic had done earlier. Bjelic focused on the embodied details of setting up three pendulums in an effort to approximate the result that Galileo describes in Two New Sciences. Bjelic elaborates at length upon the practical difficulties he ran into with finding weights of appropriate size and length, working out suitable lengths for the space available, tying knots that would maintain adequately precise and stable ratios among the three strings, releasing the three weights simultaneously and along parallel paths, and counting swings of the three pendulums to work out their ratios. His fascination with such embodied practices expands into a Lacanian exegesis of equipment as an extension of a sexualized body. Livingston goes at it quite differently, noting that the experiment with the three pendulums was part of a “suite” of experiments, each of which illustrates the Galilean law. Rather than treating the claimed ratio of length to period as an occasional and emergent “discovery” founded on singular arrangements of materials and embodied practices, Livingston treats it as a basis for an indefinite series of variants in number of pendulums and methods of measurement. The law thus acts as a reference point, independent of any given experimental performance, and it is approximated through averaged measures from a series of runs; but such independence is itself a contingent and performative feature of the suite of experiments and series of runs. Where Bjelic’s personal discovery of pendulum synchronicity is grounded in the experimental body, Livingston’s exercise is grounded in actions organized in relation to the mathematical
expression they demonstrate. From Bjelic’s vantage point, Livingston verges on a Platonic regard for the mathematical law, whereas for Livingston, Bjelic’s fascination with embodiment misses the point of the experiment.

The divergent lessons taken from the pendulum exercise can lead us to wonder about just what would exemplify Garfinkel’s (2002:175–176) “unique adequacy requirement” of methods. There are two ways to understand this requirement: as a demand to gain a thoroughgoing mastery of a practice as a condition for characterizing how competent practitioners perform it; and/or as a requirement to utilize material performances at some level of competency in order to bring under examination the endogenous details of the practice. As Livingston (2008:243) expresses it, “our research isn’t oriented to disciplinary sociology; we aren’t engaged in a sociology of the hidden order. The impetus for these studies lies in a curiosity about how some activity, as a practical matter, is done.” He adds that there is “no way to do this other than by ‘going native.’”

Going native typically is treated as a textbook mistake in ethnographic research—a failure to maintain the “distance” necessary for making sociological sense out of routine practices—but in this case it is a methodological condition rather than an end. And since, for Livingston, routine communicative practices are the primary source of sociological sense, the task is not a matter of “stepping back” but instead is a matter of getting to the heart of the practice. Moreover, the practices Livingston addresses are far from alien to Western “reason”—as his book title suggests, they epitomize “reason” (not as a universal, but as the requisite understandings for making competent moves in one or another language game). His methodology has admirable integrity, as a way to exhibit and understand the way particular formal moves in a game or other circumscribed activity are enacted in lived situations. It also is limited by the scope of what can be conveyed in writing to readers with limited, if any, preparation to perform the practices in question. As with Sudnow’s ([1978] 2001) presentation of improvisational jazz, which provides a rich phenomenological account of the author’s mastery of a series of maneuvers at the keyboard, many of Livingston’s examples tend to be solo performances: an initial position of checkers on a board followed by a series of moves that force the opponent to lose; an initial diagram and a series of steps that make up a proof, and so forth. Livingston insists, and rightly so, that the performances he explicates are “social,” though the interactions he describes are matters of reasoned action with initial materials, rather than contingency-laden courses of social interaction.

Sormani (2014) presents a more familiar style of ethnography, utilizing first-hand observations, transcripts of tape recordings, and photographs of practitioners (including himself) using probe microscopes in a physics laboratory. He critically situates his study in reference to a series of ethnographies of laboratories that had an important role in the development of science and technology studies (STS). The best known of those studies was Latour and Woolgar’s (1979) ethnography of a laboratory in the Salk Institute in La Jolla, California. Many in the STS community nowadays acknowledge that ethnographies of scientific laboratories marked an important moment in the development of STS. The lesson usually taken from such studies is that such ethnographies demonstrated that “facts” in even the hardest sciences are constructed all the way down to the laboratory bench. Although the heyday of laboratory studies appears to have passed long ago, and it is no longer fashionable to speak of the “construction of scientific facts,” laboratory studies remain rhetorically important for setting up current claims in STS that science is thoroughly pervaded by culture and politics. Sormani questions the assumption that laboratory studies did (or even could) achieve such a demonstration (see also Doing 2008), and, guided by Garfinkel’s unique adequacy requirement, he describes his own arduous effort to master experimental techniques using scanning tunneling microscopy to measure electronic properties of complex superconducting compounds. His book elaborates upon experiments in which he participated, including one that his main informant and collaborator claimed was a discovery. Although Sormani describes the technical practices and illustrates them with transcripts and photographs, he does not (nor
could he possibly) expect his readers to perform the practices.

Sormani’s book is quite challenging to read for those of us who have limited experience with the physics research he describes, and his critical lessons for ethnomethodological and STS research are embedded in lengthy descriptions of the equipment, experiments, and practices of performing them. Compared with exercises for performing a mocked-up Galilean experiment, he offers a far more complex account of how he struggled to master actual practices, though without the invitation to his readers (other than a few with the requisite backgrounds and skills) to travel with him on his journey. His main lesson for STS is a deflationary one that questions both the methodological basis (in practical understanding) and precise meaning of the claim that physical facts are constructed or somehow inflected with culture and politics. Sormani himself wonders if this lesson was worth the arduous task he undertook to engage more deeply with the performative details of a contemporary physical science than had been done in any prior STS or ethnomethodological study.

Conclusion

Taken together, the books discussed in this essay point to an alternative way to think about the relationship between current research and theoretical and philosophical traditions. That alternative way of thinking is encouraged, though unevenly perhaps, in some of Garfinkel’s writings and in the writings of his would-be followers. Garfinkel’s “company of bastards” remark suggests that ethnomethodology is an anarchic lineage in which participants misread foundational writings and take earlier initiatives (including his own) in unanticipated directions. Such directions include efforts to “mainstream” or “normalize” ethnomethodology by linking it to classic sociological theory and established social science interests and methods. However, Garfinkel’s more radical suggestion, which the books discussed here attempt to follow up and exemplify, is to position ethnomethodology not within the existing social sciences, but to develop it in partnership with the various explicit methodologies of the sciences (and non-sciences) of practical action. Viewed in this way, ethnomethodology offers to faithfully articulate implicit (sometimes embarrassing) aspects of the practice of a social science, natural science, or other activity. It is a vulnerable and hazardous undertaking, with uncertain prospects in academic organizations, corporate environments, and other places where investigations of practice are administered. If Garfinkel is correct, however, a niche for ethnomethodology or something like it reappears wherever and whenever formal analysis is done.

References


---

5 Graham Button (personal communication, July 2014) suggested the theme of “normalizing Garfinkel.”


Following a long tradition of scholars in the international migration field, Elizabeth Aranda, Sallie Hughes, and Elena Sabogal offer a methodologically rich and theoretically sophisticated study that vividly contextualizes modern processes of migration and immigrant incorporation. In *Making a Life in Multiethnic Miami*, they portray causes and consequences of migration from Latin America and problematize the incorporation of Latino and Caribbean immigrants into the social and economic fabric of Miami, which is the American city with the highest proportion of Spanish-speaking population (63.7 percent) and home to 1.3 million immigrants—equivalent to 51 percent of the population of the city—of whom 93 percent originate from Latin America. Detailed descriptions of immigrants’ accounts through stages of departure, arrival, and settlement, and, in a few cases, return are examined through the concepts of ontological security and translocal social citizenship.

I

The book opens with the story of Alejandra, not a Mexican or Cuban immigrant, but an upper-middle-class Colombian immigrant with a college degree and a successful professional life. Alejandra exemplifies a professional woman in search of higher income and gender emancipation who leaves her family behind. Although Alejandra, like many other immigrants, came to the United States in search of the American Dream, she regrets the consequences of having done so. Through the exploration and interpretation of immigrants’ voices, the authors juxtapose the image of Miami as a safe haven for Latino immigrants in search of greater human security with their exclusionary experiences in the city. Immigrants’ experiences are marked by processes of racialization, occupational segmentation, absence or limits of their legal status, and barriers due to national-origin, class, and gender hierarchies.

The authors introduce two novel analytical elements: subjective self-placements on social ladders and racial structures, and emotions. In the analysis of social mobility and race relations, the authors describe and categorize how immigrants amalgamate and internalize their social positions. The data show how immigrants subjectively use class hierarchies they are confronted with in Miami in combination with class structures from their countries of origin, resulting in a new self-identification in a joint hierarchy—leading to anxiety and contradictions for the majority of immigrants who do not perceive upward mobility. The second novel analytical element is the incorporation of a sense of loss through immigrants’ voiced emotions. Migration inevitably carries the loss of stable and affirmative relationships for immigrants and the loss of their unconscious sense of trust, which the authors conceptualize by using Giddens’s concept of ontological insecurity. Immigration is described then as an ambivalent experience. Immigrants’ new destinations offer economic, physical, and in some instances psychic security at an emotional cost.
In the search for ontological security, immigrants engage in translocal practices to cope with anxiety generated by the ambivalence of their experience and maintain co-presence with their families and communities in their countries of origin by visiting relatives, calling regularly, and consuming homeland media. The authors adopt principles from the literature on transnationalism to analyze transborder practices, the broad concept of citizenship from the feminist perspective, and the local experiences of immigrants in Miami’s familiar cultural landscape to coin the phrase “translocal social citizenship.”

In essence, Making a Life in Multiethnic Miami uncovers—through Latino immigrants’ voices and emotions—the exchange between forms of security and vulnerability embedded in human mobility and analyzes how these are modulated in the most global, Latin city in the Americas.

II

Aranda, Hughes, and Sabogal skillfully navigate the life experiences of a large and diverse sample of immigrants and their processes of economic, social, and political incorporation in a rising global city. Their analysis provides contributions to the fields of international migration, transnational studies, urban sociology, race and ethnic relations, women’s studies, social psychology, and human geography. The book contains nine chapters, which are loosely grouped into three parts: contexts of migration, structural and institutional barriers to immigrants’ integration, and immigrants’ ways of coping. The first three chapters provide a rationale for the theoretical layout and describe the negative effects of neoliberal policies on economic, social, and physical security in Latin America, as well as post-9/11 immigrant policies that have resulted in the criminalization of immigrants. These first three chapters show how the global neoliberal project requires the participation of the state, but in very distinct and contradictory roles: as the promoter of capital flows and as the enforcer of national borders. State actions in the United States and in Latin America and the Caribbean have led to higher levels of insecurity in the region, which results in increased pressures to migrate, growth of the immigrant flow, and greater vulnerability for immigrants.

The second part of the book centers on how class, power, and racial relations and immigrants’ membership and degree of political representation shape immigrants’ strategies and modes of integration. Chapter Four offers an analysis of immigrants’ self-perceptions of social mobility. For many, their perceptions of downward social mobility upon migration, experiences of immobility in a secondary labor market, and, for some, their success stories reveal nuanced variations in the well-known U-pattern of social and economic mobility over time observed by Chiswick and colleagues (2005) when immigrants’ self-perceived social locations are introduced in the analysis.

Furthermore, the rich qualitative data incorporated in the analysis allows the reader to observe how immigrants’ subjective notions of class and class belonging in Latin America and their experiences in Miami’s labor market shape immigrants’ self-esteem and social security in an economically bifurcated context that “caters to the rich and disadvantages the poor and middle classes” (p. 112). The authors identified variables associated with subjective upward mobility as well as barriers for social mobility. Those in search of educational opportunities, those of rural origin, and women are more likely to perceive upward mobility. For women, and particularly for women from rural origins, migration is an opportunity for emancipation, work outside the home, increased education, and a new lifestyle. The labor incorporation of professionals from Latin America is limited due to language barriers, difficulties in the transference of professional credentials or acknowledgement of past labor experience, and lack of or limited legal status. Many immigrants with college degrees and professional experience occupy contradictory class locations (one according to their human capital and another according to their current income and occupation). Moreover, those who experienced working conditions of exploitation, declines in occupational status, and loss of control over their work life expressed enhanced social and psychic insecurity as a result of migration.
Although immigrants’ sense of security is primarily shaped by their experiences in the labor market, the lack of voice and political representation for non-Cuban Latino immigrants in Miami is a source of insecurity that erodes their sense of belonging. The Cuban model of political incorporation—based on automatic legal status and government financial support, ethnic solidarity, spatial concentration, interest in domestic policy for the exile community, and possession of key positions in the political arena—reinforces and reproduces differences due to national origin, race, class, and gender. As Cubans symbolically represent Latinos in Miami, only limited political channels remain for emergent policies that can benefit other immigrant groups. With the exception of Haitians, no other Latino or Caribbean national origin group has successfully incorporated into South Florida politics. Unfortunately, the Nicaraguan community is geographically dispersed and has low rates of naturalization; Colombians are divided along political lines; Mexicans are polarized by their class and ethnic origins; and the Venezuelan community, although growing rapidly, is still in the process of establishing itself in the local political landscape.

As in the analysis of social mobility, the analysis of race in multiracial Miami is informed by a combination of immigrants’ understandings of racial hierarchies derived from their countries of origin, usually associated with class and indigenous roots, and the U.S. racial structure. The authors identified, as has been observed in other studies, variations of the traditional black and white color line (Lee and Bean 2010). These new categories conflate national origin, class, race, and legal status (p. 212) and reinforce institutional racism—evidenced in their analysis of the Cuban-Haitian divide. Cubans experience privilege as “honorary whites,” and blackness is denigrated (p. 228). Although the pro-immigrant environment created by Cubans can be welcoming for young immigrants (Stepick and Stepick 2009), according to the authors, these emerging racial categories are experienced among immigrants through diverse forms of discrimination-provoking anxieties of being perceived in “suspicious ways,” which can be identified as an additional source of immigrants’ psychic insecurity.

The last part of the book contains the analysis of immigrants’ responses to sources of their ontological insecurity in Miami and how Miami’s cultural context and geographical location facilitate their simultaneous engagement in translocal and placemaking practices. Immigrants who have lived in other areas of the United States perceive Miami as a culturally familiar and inclusive environment. The use of Spanish or Creole is the most important cultural element in the achievement of immigrants’ goals and is also perceived as a source of comfort. However, the analysis of Spanish use suggests that the use of a common language cannot overcome differences in national origin (p. 301). Culturally disadvantaged immigrants tend to seek membership in their conational group and find the racial and ethnic hierarchies exclusionary. Stereotypes, accents, meanings of words, and even humor styles in Spanish media dominated by Cuban and Mexican companies reinforced their subordinate status. The critical analysis of racial hierarchies, immigrant social integration, and identity formation leads to a paradox: even in Miami, “a Latino foreign city” to many Americans, home of the Cuban enclave—where Spanish use is predominant and Latinos hold economic and political power—a pan-ethnic Latino community is not identified; what prevail are intra- and interethnic hierarchies that are rooted in social structures of origin and destination and shaped by state policies. This paradox opens new questions on the formation of ethnic solidarity and pan-ethnic consciousness among immigrants from regional blocs concentrated in global cities.

III

Several books have contributed to the place-specific literature on Miami and the social make-up of the city. Making a Life in Multiracial Miami is an important addition to this literature. Chronologically, City on the Edge (Portes and Stepick 1993) offers an account of the rise of Miami as an important urban center, identifies the elements contributing to the emergence and sustainability of the Cuban enclave, and compares how other

Contemporary Sociology 44, 5

Downloaded from osx.sagepub.com at ASA - American Sociological Association on September 2, 2015
racial (African Americans) and immigrant groups (Haitians and Nicaraguans) fared in the city in the 1980s and were confronted with the changing landscape resulting from the Mariel boatlift and the Liberty City riots. In *Miami: Mistress of the Americas*, Jan Nijman (2011) describes the transformation of Miami from one the most inaccessible parts of the country to the first hemispheric city—the most centrally connected city in the Americas: post-industrial, global, multicultural, segregated, and transient. *Making a Life in Multiethnic Miami* explores immigrant livelihoods in a rising global city, challenges the role of the Cuban enclave in the contemporary Latino immigrant community, and critically assesses the neoliberal and racial projects of the state as a numerous and increasingly diverse flow of Latino immigrants arrive in the city and make it their home.

In short, Aranda, Hughes, and Sabogal offer a conceptually sophisticated and rich analysis of immigration in a segregated, unique, and rising global city; they provide new methods for the inclusion of emotions and subjective perceptions needed in the literature on international migration; and they pave the way for the incorporation of more nuanced analysis on the confluence of class, racial, and ethnic rivalries and legal status in urban areas with high concentrations of immigrants.

**References**


The Path Not Taken? Culture, Materials, and Pleasure in Action

**Claudio E. Benzecry**

University of Connecticut

Claudio.benzecry@uconn.edu

Looking at the blurbs on the back cover of *Profane Culture*, you get a first glimpse of why this book deserves to be discussed almost 40 years after the date of its first publication. The celebratory testimonies by Jeffrey Alexander, Mitchell Duneier, and three European scholars of cultural studies (Mats Trondman, Anna Lund, and Stefan Lund) point to the confluence of three intellectual schools that have grown apart as the study of culture has become institutionalized: cultural sociology, with its attention to cultural structures in the generation of practices, selfhood, and meaning-making; careful ethnographies of working-class and deviant subgroups and their idiolects; and the normative study of cultural autonomy among dominated subcultures and their creative use of dominant culture. Part of the explanation for this happy convergence is the attention Paul Willis dedicates to two subjects that have become central to the sociological study of culture in recent years: processes of embodiment and the role of materiality in everyday life. From this vantage point, the book develops a particular theory of how social groups and cultural objects interact and the potential avenues through which this happens. Far from the one-to-one attribution of meaning of the Durkheimian school, Willis masterfully shows how objects and
racial (African Americans) and immigrant groups (Haitians and Nicaraguans) fared in the city in the 1980s and were confronted with the changing landscape resulting from the Mariel boatlift and the Liberty City riots. In *Miami: Mistress of the Americas*, Jan Nijman (2011) describes the transformation of Miami from one the most inaccessible parts of the country to the first hemispheric city—the most centrally connected city in the Americas: post-industrial, global, multicultural, segregated, and transient. *Making a Life in Multiethnic Miami* explores immigrant livelihoods in a rising global city, challenges the role of the Cuban enclave in the contemporary Latino immigrant community, and critically assesses the neoliberal and racial projects of the state as a numerous and increasingly diverse flow of Latino immigrants arrive in the city and make it their home.

In short, Aranda, Huges, and Sabogal offer a conceptually sophisticated and rich analysis of immigration in a segregated, unique, and rising global city; they provide new methods for the inclusion of emotions and subjective perceptions needed in the literature on international migration; and they pave the way for the incorporation of more nuanced analysis on the confluence of class, racial, and ethnic rivalries and legal status in urban areas with high concentrations of immigrants.

References


The Path Not Taken? Culture, Materials, and Pleasure in Action

Claudio E. Benzecry
University of Connecticut
Claudio.benzecry@uconn.edu

Looking at the blurbs on the back cover of *Profane Culture*, you get a first glimpse of why this book deserves to be discussed almost 40 years after the date of its first publication. The celebratory testimonies by Jeffrey Alexander, Mitchell Duneier, and three European scholars of cultural studies (Mats Trondman, Anna Lund, and Stefan Lund) point to the confluence of three intellectual schools that have grown apart as the study of culture has become institutionalized: cultural sociology, with its attention to cultural structures in the generation of practices, selfhood, and meaning-making; careful ethnographies of working-class and deviant subgroups and their idiolects; and the normative study of cultural autonomy among dominated subcultures and their creative use of dominant culture. Part of the explanation for this happy convergence is the attention Paul Willis dedicates to two subjects that have become central to the sociological study of culture in recent years: processes of embodiment and the role of materiality in everyday life. From this vantage point, the book develops a particular theory of how social groups and cultural objects interact and the potential avenues through which this happens. Far from the one-to-one attribution of meaning of the Durkheimian school, Willis masterfully shows how objects and

selves interact, indicating to one another the available paths to follow.

Willis is part of the second generation of British cultural studies, having worked with both Richard Hoggart and Stuart Hall. His work can be read as concerned with some of the classic coordinates of this school, emphasizing the change in focus in the study of cultural practices from the realm of highbrow to lowbrow and concerned with a dialectical version of culture that shows what those from below do first with the culture produced by the elite and then with mass culture. His work also signals a second movement in the study of culture, leaving behind literary analysis and interpretation in favor of ethnographic experimentation. In that displacement, Willis applied and extended the close reading techniques he learned in Cambridge and combined them with a humanist interest in the creativity of subaltern groups to constitute themselves within capitalist power relations. To do so, he adopted the anti-intellectual posture of subordinate groups with the hopes of developing concepts sensitive to the lives of the populations under study. His thick-layered interpretation of the cultures of hippies and motor bikers is not about the investigator “reading” and saying “here we have a cultural form and there is society; this is how they relate,” but rather about what practitioners themselves report in terms of which cultural materials play a role in determining how subjects constitute themselves.

The book is set up as a comparison between two cases, one working class (the motor bikers) and the other mostly middle class (the hippies). In exploring culture from the sixties, with its promise of heralding a new set of open and democratic human relations, Willis shows that what both groups share is the quest to achieve “authenticity,” though they differ greatly in the meanings they attach to that concept, the styles of selfhood they envision, and the cultural devices they mobilize. Willis shows, through which versions of selfhood they achieve, that the politics of style allow subjects to construct variations on the mainstream via the use of mass culture, and he explores the role of cultural items in constituting these lifestyles.

For the biker boys it was a “masculine” style based on simulated versions of aggression, on moving quickly and with confidence, and in opposition to qualities perceived as feminine, like delicacy and artificiality. They aimed to achieve ontological security in the muscular style of early rock ‘n’ roll. Despite the potentially anachronistic character of the attachment, biker boys made the music resonate with their lives. Being active and affirmative was so important that even linguistic choices—as when negative opinions are expressed as actions, or descriptions are carried in the form of onomatopoeia—follow consequently.

On the other hand, for the hippies authenticity is achieved through the rejection of traditional middle-class career-meritocratic values: meaningful living is based on the search for experiences. The self was organized around spirituality, via fuller states of awareness, emphasizing the “now” of temporality over long-term planning. In line with the romantic roots of British Marxism, Willis describes the hippie lifestyle as confronting the devaluation of everyday life because of technological rationalism. Hippies achieved a powerless omniscience through identification with underprivileged groups and the pursuit of organic and natural styles, carefully crafted through the disorganization of fashion as seen in their long hair, the music they listened to (progressive rock), their drug use, and the randomness of their object choices. To summarize, for Willis, in symbolizing their conditions, bikers and hippies create forms of culture made out of particular objects and ways of acting and carrying themselves in the world.

The evidentiary pillar that Willis’s research is based on is long-term ethnographic immersion in these groups, emphasizing the sensuous and lived character of cultural production with a focus on the body, both in how it operates in the production of sensibility and in how the ethnographer’s own body is a tool for producing data, a research instrument for exposing himself to the same conditions and issues experienced by the people he is studying. Willis’s ethnographic perspective also anticipates technical and rhetorical devices common nowadays in some of our best-known qualitative studies: the role of reflexivity, using participant perceptions of the ethnographer as a source of data; and the role of...
recordings. He used the latter in two different ways. First, he used tape more as a way to capture the elusive quality of time as it passes than as a tool to establish the one-on-one correspondence between fieldwork and its inscription as data. Second, Willis played his own music records for the hippies, hoping to record what kinds of reactions they elicited.

So, what is in this book for sociology? Or, more pointedly, how does the book contribute to our contemporary understanding of culture? In *Learning to Labour* we gathered lessons about the autonomous character of working-class culture and its complicit and paradoxical role in class reproduction. Here we learn about how cultural materials participate in the production of selves and groups. Willis’s theory of culture provides additional lessons to the new sociological debates on the culture of poverty; instead of imagining the cultural and social autonomy of the world of subaltern populations as a given, the book shows how the “feeling of us,” of groupness, is always precarious, something to be achieved. It is constructed both through relationships with others (for example, the police, schoolteachers, migrants) as much as with other members of the same social class who choose other cultural materials to make sense of who they are (in the case of the bikers, the mods, for instance). The relationships also include gendered associations with women: in one case, women constitute an inverted mirror of what is to be achieved (an assertive masculinity and an action-oriented perspective on the world for the biker boys), and in the other women are an organic component of the culture to be aspired to (as when hippies consider women closer to nature).

Groups are always thought of as relationally constituted. The links between groups and objects, and between some cultural items and other cultural items, are built as homologies. But instead of an arbitrary character of meaning attribution, and of a one-to-one relationship between social positions and the items that are used to express them imagined by Durkheimians and structuralists, Willis proposes to study what it is that those materials actually afford. How do they lend themselves to be used to express the forms of masculinity, authenticity, and spirituality I have described? How do they help people to feel that they are taking part in a group and to convert their structural conditions into everyday concrete practices?

Combining the analysis of cultural structures via homologies with a Marxist focus on experiences through the notion of style, *Profane Culture* advances its argument in two parts: first, how the “feeling of us” is distinctly achieved for hippies and bikers; and second, the indexical, homological, and polyvalent role of objects and the meaning possibilities they hold for those who use them. Regarding the first, the book highlights the ontological security and integrity provided to bikers by the beat of their music, their way of handling themselves, the use of words as actions, and the restless movement that is a constant in their lives. If the bikers were able to subvert industrial and bourgeois time thanks to their steady state of being, hippies, on the other hand, subverted time by avoiding plans and schedules as much as they could, as well as by aiming to have “experiences,” dissolving the diachronical movement of the clock. Hippies found a confirmation of themselves in the exploration of what made each and every one of them unique, as well as in their common work toward achieving community, which was considered something to be accomplished, always precarious, not taken for granted. Uniqueness was achieved through big gestures; being a “head” was all about speaking in a way that involved surprise and movement. It was also about using drugs and listening to music that disregarded the classical bar structure. Now, let me expand on this last point further.

Willis studies the use of cultural items like music and drugs, paying close attention to the polyvalent relationship between groups and objects. While the meanings are culturally constructed, the construction, nevertheless, derives from the “objective” possibilities of the cultural items themselves. This phenomenon can be studied at the level of the homological—as I have already discussed—but also at the “indexical” level, by examining the frequency with which certain cultural materials are “indexed” into a group. If drugs offered to hippies the opening up of time and the paradoxical achievement of freedom through a lack of autonomy, the music—artists and groups
like Frank Zappa, Pink Floyd, or Van Morrison—resonated with them by allowing for logocentrism to be kept at bay by the unobjectified complexity of the music’s sonic explorations. The subversion of the tonal structure, the overthrowing of harmony as the basic musical skeleton, and the advent of the concept album further allowed for explorations with time and consciousness that were closely in line with what hippies wanted to feel. Music, being an “experience,” also allowed hippies to escape the determinations of their class.

For biker boys, music provided an avenue to authenticity and masculinity. The key was the muscularity and physicality of the beat in early rock ‘n’ roll (e.g., Chuck Berry and Buddy Holly), but the format also mattered, with 78s preferred to 45s. Biker boys also entered into a symbiotic relationship with another material: the motorbike. Willis shows how the boys adapted and subverted a technological device, emphasizing the anthropomorphization of the machine and how its strength and movement matched the secure nature of the boys’ world. Handlebars, to point to another cultural item, provided a sense of style but also increased the ability to handle the bike with ease, giving the feeling of being one with the motorcycle, almost like a modern centaur.

Willis shows how a group cannot choose just a random cultural item, and neither can a cultural item attach itself to just any group. And, to show how cultural relationships are deepened over time, he explores the processes of "substantiation" and "accumulation," underscoring how items become increasingly and extensively transparent in their meaning. In respecting the materiality of cultural objects, the book is, to a certain extent, an investigation of popular music and its uses from a sociological perspective as much as an aesthetic analysis of popular music and its sociological consequences, making sure not to let one perspective supersede the other. The book showed for the first time music in action and had the subjects “themselves establish the connection between music and social life” (DeNora 2000:6).

Unfortunately, after a start where cultural materials were ethnographically presented as enablers of lifestyles and forms of consciousness (beyond Willis, think of the work of Dick Hebdige [1979]), cultural studies has slowly moved into a combination of first-person testimonies, analysis of literary texts as representative of social groups, and content analysis of lyrics and texts instead of interrogating consumers about what different works mean to them and about the performances through which consumers have built a relationship with these works. Cultural sociologists, on the other hand, have smartly moved away from this cultural studies tradition in which the process of signification is erroneously equated with and reduced to content analysis. Unfortunately, some other subdisciplines have adopted the cultural-studies legacy, moving further and further away from the insights of books like Profane Culture. Instead of exploring the actual work and how it produces engagement, like Willis, many have moved away from studying what people do with things and more into content analyses where they assume class/race/gender as the taken-for-granted explanation for the cultural object and its effects. The real promise of reissuing this book for cultural sociologists is in helping us to move beyond the reduction of culture to a pre-structured intersection (or its resistance) and to refocus on the symbolic work people perform on commodities in ordinary life.

In his just-published memoir, anthropologist Vincent Crapanzano wonders, “Must we punctuate the past only with pain? Can we organize it around pleasurable moments? . . . Would a pleasure punctuated life be less vibrant?” (2015:96). Profane Culture has opened up an avenue to explore how we can study what is pleasurable in life, even among those who might seem, at first glance, the most subject to domination and suffering. I hope a new generation of scholars reads this updated edition and aims to follow its path.

References