

## Editorial

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Epidemic events, as the great American historian of medicine Charles E. Rosenberg once observed in a programmatic statement, constitute an “extraordinary useful sampling device” for the social scientist (Rosenberg 1992, 279). [1] The sudden spread of an infectious disease across a territory, Rosenberg remarked, is at once a “found object” and a “natural experiment,” capable of illuminating “fundamental patterns of social value and institutional practice” (Rosenberg 1992, 279). Epidemics of plaque and cholera, smallpox and yellow fever, AIDS and SARS enable scholars to see, at one particular moment in time, “the configuration of values and attitudes that [...] are so fragmented or so taken for granted that they are not easily visible” (Rosenberg 2008, 6). Due to their episodic nature and their public character, epidemics seem to constitute particularly generative objects of scientific research, allowing scholars to explore in detail how a society mobilizes itself in a time of crisis, acting out, as Rosenberg put it, “propitiatory rituals that incorporate and reaffirm fundamental social values and modes of understanding” (Rosenberg 1992, 279). According to this view, the unexpected emergence of an infectious disease thus constitutes a critical moment of truth, forcing societies to represent themselves to themselves, as Durkheim might have phrased it, in forms that are culturally specific and historically singular.

Today, of course, “society” is not necessarily figured anymore as the ultimate instance of the real in scholarly work. Following a suggestion proposed by French philosopher and historian Michel Foucault, a growing body of critical research has in fact questioned “the principle, unexamined and taken for granted, that the only reality to which history must attend, is society itself” (Foucault 2001, 834). In the past two decades, anthropologists and other affiliated scholars have put the presumably self-evident category under critical scrutiny, arguing that the concept of society is “theoretically

[1] See as well Rosenberg’s acclaimed history of cholera in the United States, originally published in 1962. (Rosenberg 1987)



obsolete” (Strathern 1996). With Rosenberg’s account of epidemic events in mind, a question begins to formulate itself: What kind of relations might such events illuminate today? What kind of work might epidemic events be able to accomplish beyond the elucidation of “society”? What is their contemporary analytic potential? A key aim of this special issue of *Behemoth* devoted to the timely theme of epidemic events is to re-function Rosenberg’s modernist sampling device for current circumstances.

## Emerging Viruses

Over the past two decades, a powerful biomedical framework has appeared that has fundamentally reshaped the way epidemic events are approached by a growing number of actors. In the late 1980s and early 1990s, as historian of medicine Nicholas B. King pointed out, a group of influential American scientists and public health specialists argued that new infectious diseases are likely to surface in the near future. Coined by epidemiologist Stephen S. Morse, the seminal concept of “emerging viruses” was officially launched in early May of 1989 at a high-profile conference in Washington, D.C., sponsored by Rockefeller University, the National Institute of Allergy and Infectious Diseases, and the Fogarty International Center (Altman 1989). Morse and his colleague, Nobel Prize-winning virologist Joshua Lederberg, invited more than two hundred participants to the scientific meeting in order to discuss their concerns about the relentless evolution of viral agents that seemed to account for the growing number of infectious disease outbreaks around the world, including, most importantly, the spread of AIDS/HIV. These concerns, as it turned out, were shared by many a scientist, and over the next few years they were frequently repeated by journalists and politicians, solidifying into what King identified as an “emerging diseases worldview” (King 2002; see as well King 2004). This worldview eventually laid to rest “comfortable theories of an epidemiological transition” and necessitated the reconsideration of traditional strategies of infectious disease management, as anthropologists Veena Das and Abhijit Dasgupta remarked (Das/Dasgupta 2000, 636).

As King rightly underscored in his critical account, the tremendous success and popularity of the emerging diseases worldview was largely due to two American journalists, Laurie Garrett and Richard Preston. Garrett, a former National Public Radio and Newsday correspondent, conducted extensive research for a book project on the appearance of a set of known and unknown infectious diseases while she was a fellow at the Harvard School of Public Health in the early 1990s. Upon learning that her colleague Richard Preston was working on a similar publication, Garrett intensified work on her

rapidly growing manuscript in order to release it at the same time (King 2002). In 1994, Garrett's *The Coming Plague* and Preston's *The Hot Zone* were published almost simultaneously to great public acclaim (Garrett 1994; Preston 1994). Both publications instantly became bestsellers, effectively establishing the frightening imaginary of a vulnerable nation threatened by an obscure world of dangerous microbes lurking in the rain forests of far away countries. As King pointed out, the discourse of emerging infectious diseases has been so powerful in the past few years because it is "tremendously flexible, allowing a wide variety of actors to adopt it, moulding small parts or emphasizing particular elements and downplaying others to suit their own purposes" (King 2002). The discourse furnishes actors "with a consistent, self-contained ontology of epidemic disease: its causes and consequences, its patterns and prospects, the constellation of risks that it presents, and the most appropriate methods of preventing and managing those risks. It comes equipped with a moral economy and historical narrative, explaining how and why we find ourselves in the situation that we do now, identifying villains and heroes, ascribing blame for failures and credit for triumphs" (King 2002, 768). The discourse of emerging infectious diseases is not only and simply animated by a scientific understanding of the relentless evolution of microbial organisms, but also by a deeply moral perception of the harmful consequences of modernity.

## Global Orders

A broad range of factors, as biomedical scientists, public health experts and journalists underscore, is responsible for the spread of known and unknown infectious agents across the world (Culliton 1990; Krause 1992; Krause 1998; Morse 1993; Satcher 1995). An influential report on *Microbial Threats to Health in the United States*, published by the Institute of Medicine in 1992, foregrounds changes in human demographics and behavior, technology and industry, economic development and land use, international travel and commerce as crucial elements that contribute to the emergence and re-emergence of infectious diseases (Lederberg/Shope/Oaks 1992). Unlike the much-celebrated germ theory of disease and its narrow etiological narrative of the normal and the pathological (though see Mendelsohn 1998), the discourse of emerging infectious diseases actively promotes a "holistic" or "ecological" or "biosocial" perspective. With its focus firmly on the unpredictable interactions of social, biological, political and economic factors, the textual productions around emerging infections formulates a critical account of modernity which deeply resonates with contemporary environmental con-

cerns as they are expressed by a number of actors, both governmental and non-governmental. The discourse of emerging infectious diseases thus articulates with a broad and diverse range of discourses, practices, and affects, providing existing and emerging concerns a powerful language that effectively contributes to the production of urgency and the apparent necessity of immediate intervention.

Clearly, as King suggests, the flexible discourse of emerging infectious diseases also deeply resonates with the world-historical narrative of “globalization” and the distinctive practices, meanings, and affects that are attached to it. Indeed, the troubling notion of a relentless traffic of microbial organisms falls on fertile ground in a world that increasingly finds itself in the grip of global flows and their unintended consequences for human health and well-being. As the above-mentioned report by the Institute of Medicine soberly notes, infectious diseases that “affect people in other parts of the world represent potential threats to the United States because of global interdependence, modern transportation, trade, and changing social and cultural patterns” (Lederberg et al. 1992). The discourse of emerging infectious diseases thus constitutes a version of what anthropologist Anna Tsing identified as “globalism,” the explicit endorsement “of the importance of the global” (Tsing 2000, 330). Here, in the realm of the pathological, the global comes to matter in very particular ways. The “global village,” and the processes that have led to its formation, are seen as a dangerous “breeding ground” for deadly diseases. Pathogenic agents circle around the planet in the shadow of a growing international traffic of people, things, and goods, and thus index “emerging biopolitical insecurities: unrecognizable aliens capable of disrupting existing immunities, penetrating once-secure boundaries at a time of deregulated exchange,” as Jean Comaroff recently phrased it (Comaroff 2007, 198). To bring the rapid diffusion of these dangerous germs under control, the report calls for the construction of a worldwide network of surveillance. In the context of “globalization,” infectious diseases thus take shape as an ambivalent symbolic figure articulating the hopes and fears associated with an intensifying global exchange of commercial goods. As experts underscore, the global proliferation of pathogenic matter calls for a global counter-proliferation of pathologic information. Emerging infections are thus both a sign and a vector of a new global order – and with it, “a new sense of the nature and possibilities of the political” (Comaroff 2007, 198). In the aftermath of the Cold War and its historically distinctive institutions of international order, the specter of emerging diseases has thus become a prominent site of thought, action, and passion where crucial meanings of health, safety, and security are articulated and negotiated. It is perhaps primarily these multiple kinds of ambivalent resonances that make the discourse and ideology of emerging infectious diseases so suggestive and thus so powerful and pervasive today.

## Anthropologies of the Contemporary

The aim of this special issue of Behemoth is to explore critical issues arising within the new problem space of emerging infectious diseases. The contributions to this issue identify some of the domains in which fundamental shifts have occurred over the past few years and they examine the underlying logics animating these consequential shifts in public health, medical care, and scientific research. In her contribution to this issue, *Katherine A. Mason* traces the recent institutional impact of the “emerging diseases worldview” in China. Following the work of Lakoff and others (Lakoff 2008), she argues that a new model of public health, which is focused less on prevention and more on preparedness, has increasingly taken hold in China, especially in the aftermath of the sudden emergence of SARS. Her article examines in great ethnographic detail a series of infectious disease control strategies that were considered excessive, unscientific, and unsophisticated in other parts of the world. Mason thus poses an important question in her article. It is the question of how certain forms of representation and intervention eventually gain paramount rationality in a given historical context. This question is also at the heart of *Lyle Fearnley’s* contribution. His concern is especially with the development of surveillance systems for the detection of disease outbreaks in the United States. In his article, Fearnley shows how a specific set of techniques and infrastructures were assembled into a system of “epidemic intelligence,” which gradually transformed epidemic events as a scientific object. What epidemic events are and what they mean, Fearnley suggests, has fundamentally changed in the second half of the twentieth century. Significantly, this transformation of epidemic events in the context of epidemic intelligence has made surveillance a “dominant solution to emerging infectious disease problems,” Fearnley argues. Returning to the “swine flu” pandemic, *Theresa MacPhail* examines a more recent shift within U.S. public health discourse and practice. It is the gradual shift from the deployment of scientific “certainty” to the use of biological “uncertainty.” MacPhail’s article offers an important and interesting perspective on contemporary practices in the biopolitical management of populations. As the author suggests, scientific authority “persists not despite uncertainty, but because of it. Uncertainty is the fertile ground for further scientific research and funding. Sustaining this uncertainty, while managing it, maintains the current or reigning research paradigm.” The last contribution to this special issue addresses recent concerns with “global health,” which have taken the institutional form of departments, centers, and programs at a rapidly growing number of research universities in the United States and elsewhere. At the center of these concerns with “global health,” *Johanna Crane* argues, is an ethic of “partnership,” which distinguishes it “from older, more paternalistic traditions of international health and tropical

medicine.” The paradox of this ethic of partnership is that inequality is perceived as both a condition to be redressed and a professional opportunity to be exploited. Crane concludes that “despite the frequent invocation of the idea of ‘partnership’ in global health, the field has given little consideration to what partnership actually entails in practice or to the wide variety of relationships that currently exist between Northern and Southern entities.” Common to all articles in this special issue is a critical sense that the social, political, ethical, and scientific stakes involved in our evolving engagements with epidemic events have significantly changed and that it is essential to investigate the consequences of these changes for the worlds in which we live.

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## Becoming Modern after SARS.

### Battling the H1N1 Pandemic and the Politics of Backwardness in China's Pearl River Delta

Katherine A. Mason

#### Abstract:

This article traces the early evolution of the H1N1 pandemic as it played out in China's Pearl River Delta in the spring and summer of 2009, as local public health professionals there tried to contain the virus when their American counterparts did not do so. My informants' difficulties in escaping their perceived status as a source, rather than a victim, of dangerous viruses; their use of disease control tactics that were portrayed abroad as excessive, unscientific, and unsophisticated; and their fatalism about reforming their local system of governance; all frustrated their ambitions to show off their pandemic preparedness prowess. At the same time, the gulf between their reactions to H1N1 and the reactions across the Pacific suggests the need for a more serious global debate about what local places in all parts of the globe should and should not be prepared to do in the name of pandemic preparedness.

**Keywords:** SARS; H1N1; pandemic; China; preparedness

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*It is 9:00pm, and Huang Qing and I are in the eerily quiet H1N1 isolation ward of the main infectious disease hospital in the southeastern Chinese city of Tianmai. [1] We are here to meet the latest suspected H1N1 case, a foreign traveler that the city Quarantine Bureau has brought in from the Hong Kong border. Following Huang's instructions and what I can remember of the emergency training we all attended two months previously, I climb into my protective gear: first the biohazard suit, zipped up, then the N95 mask, surgical hat, and shoe coverings. It is hot and I instantly begin sweating and feeling short of breath – the mask limits the amount of oxygen I can take into my lungs.*

*The patient, perhaps 16 years old, has gently browned skin and a mess of dark curly hair, and is lying on a hospital bed, talking animatedly into his cell phone while nurses in biohazard suits take his blood pressure. The nurses report that he does not actually have a fever – his temperature is only 36.9 degrees. "I've been trying to tell them, it's just a headache!" the boy tells me as he puts down the phone. He says he attends a school in Hong Kong, and that he had gone on holiday to France – recently designated as an outbreak zone – but had returned a full two weeks earlier. He does not feel sick, he says.*

*Huang Qing, speaking in English, says, "Well, you might have to stay here a little while anyway." He asks how long, and she says a few hours or a few days. "A few days?? I really don't want to stay here a few days!" Huang Qing replies by paraphrasing the quarantine notice I had helped her to translate the day before, explaining that for the good of his own health and the health of others and according to international law and the law of the People's Republic of China, he might need to stay for "awhile." He becomes quiet, and tells me that he is afraid. The nurse is trying to shoo us away so that she can take nasal samples. I don't know what to say, so I just pat him on the hand, tell him not to worry, and we rush out the door. "He obviously doesn't have the virus," Huang Qing says with a sigh, and she rips off her protective gear, throws it in the trash, and goes to the doctor's station to write her report.*

In this article, I describe the attempts of local public health professionals in China's Pearl River Delta region to prevent the H1N1 pandemic influenza virus from spreading into Mainland China in the spring of 2009. [2] I argue that the origins of the controversial control tactics that my informants used can be located in a profound reconfiguration of public health in the Pearl River Delta region that took place in response to the 2003 SARS epidemic. Replacing a model of sanitation with model of preparedness, my informants were determined to prove to the world that they had shed the

[1] Tianmai is a pseudonym. All names, locations, and certain identifying details have been changed to protect the anonymity of my informants.

[2] All of the incidents I describe in this article occurred while I was conducting one year of ethnographic fieldwork at several local CDCs in the Pearl River Delta, from September 2008 through August 2009, as part of my doctoral dissertation project on public health reform in this part of China. I also conducted approximately 100 semi-structured, open-ended interviews and 10 life history interviews with members of city, district and community level public health institutions in two Pearl River Delta cities, as well as at city and national level institutions in Beijing. In addition, I visited a city public health institution in Hong Kong and two Hong Kong universities that collaborated with public health institutions in Tianmai. Preliminary research was conducted from June to August 2007, and follow-up research was conducted in August 2010.

backwardness associated with SARS and were deserving of a place in a “modern” global health community. What happened during the early days of the pandemic, however, instead revealed the ways in which their full admittance into this community remained elusive. My informants’ difficulties in escaping their perceived status as a source, rather than a victim, of dangerous viruses; their use of disease control tactics that were portrayed abroad as excessive, unscientific, and unsophisticated; and their fatalism about reforming a local system of governance that they viewed as irrational; all frustrated their ambitions to show off their pandemic preparedness prowess. At the same time, the gulf between their reactions to H1N1 and the reactions across the Pacific suggests the need for a more serious global debate about what local places in all parts of the globe should and should not be prepared to do in the name of pandemic preparedness.

## Preparedness and the Emerging Diseases Worldview

Historian Nicholas King argues that with the rise of HIV/AIDS in the 1980s, the “emerging diseases worldview” – the idea that the greatest new threats to the health of the globe would come from new and seemingly exotic infectious diseases – became a dominant paradigm in global health, and indeed in a sense gave birth to the concept of “global health” itself (2002). The idea that diseases are by their very nature global – that we are all affected and all “in it together” – had to do with the realization that “international” and “tropical” diseases could come to “us” (the developed world), and therefore we, as citizens of developed countries, had to care about the spread of such diseases among “them” (the developing world). Priscilla Wald describes how the emerging diseases worldview holds that “microbial traffic” flows “*from* the primordial rainforests of the impoverished developing world *to* the metropolitan centers of commerce and capital. [...] An infection may be endemic to an impoverished area, but it *emerges* when it appears – or threatens to appear – in a metropolitan center of the North” (2008, 34). As Nobel Laureate Joshua Lederberg famously warned, “the microbe that felled one child in a distant continent yesterday can reach your child today and seed a global pandemic tomorrow” (CDC 1994).

It is in response to this scenario that pandemic preparedness began to take hold in the 1990s and 2000s as a model for organizing public health systems around the world (Lakoff 2008). As Lyle Fearnley (2005) has described, preparedness differed from prevention models of public health in that the focus was less on trying to improve overall health indicators through sanitary and other interventions, and more on preparing for a future catastrophic disease event (Lakoff 2008). With an

outbreak of the highly fatal H5N1 strain of avian influenza in Hong Kong in 1997, which killed six people and sickened 18, the focus of global preparedness began to turn to pandemic influenza, and to southeastern China's Pearl River Delta. This region had for decades been implicated as a global source of influenza viruses – but H5N1 was different. One local flu specialist told me in an interview, "That shook up the whole world. WHO sent U.S. CDC people [to the Pearl River Delta, including Hong Kong and Tianmai] especially to investigate. [...] At the time we had never done avian flu, we could barely do human flu. Afterwards we went to Hong Kong for two months, and had an exchange. And from that moment on, the central government started to emphasize flu." Money flowed into flu surveillance and control from international and federal coffers. According to this informant, it was when SARS appeared, however, that the real obsession with pandemic flu began.

In February 2003, a Pearl River Delta physician crossed into Hong Kong and spread the SARS virus to over a dozen hotel guests, who then carried it to Toronto, Singapore, and Hanoi. In an unprecedented move, WHO issued a global health alert and urged the cessation of all non-urgent travel to Mainland China and Hong Kong and later to Toronto and other cities. SARS went on to kill about 800 people worldwide and sicken 8,000.

After initially denying the scope of SARS within China, the central Chinese government admitted error on April 20, following a whistleblower's report. Chinese leaders discharged the Minister of Health and the mayor of Beijing, promised to cooperate with all international disease control efforts, and began aggressively instituting control measures. These measures included: involuntarily quarantining thousands of people, even sealing off entire hospitals, schools, and apartment buildings; rapidly building SARS facilities, including an entire SARS hospital in one week; closing down movie theaters, Internet cafes, and other public spaces; setting up neighborhood watch systems to root out potential carriers of the disease; and drowning thousands of civets – suspected reservoirs of the SARS virus – in disinfectant. WHO praised China's actions and credited them with the success of the global SARS containment effort (Kaufman 2008; Fidler 2004; Wynia 2007).

The city that I call Tianmai is a large city located near the border between Mainland China and Hong Kong in the Pearl River Delta. [3] With SARS, this border took on outsized significance as the site where frightening diseases were likely to pass from Mainland China to the rest of the world, and Tianmai's public health professionals soon found themselves on the front lines of the global pandemic preparedness apparatus (Kleinman/Watson 2006). After SARS, the budget for flu surveillance in Tianmai shot up by a factor of 10, the size of the flu control team doubled, and the rest of the public

[3] Hong Kong, a former British colony, rejoined the People's Republic of China in 1997 as a Special Administrative Region – but a border with passport control still separates it from Mainland China.

health system reorganized to focus on preparedness. Thus, by the time H1N1 appeared in April 2009, Tianmai's Center for Disease Control and Prevention (TM CDC) – where I was conducting ethnographic fieldwork – had been preparing for such an event for six years, and was determined to prove that it had absorbed the lessons of SARS and could keep another potentially deadly bug at bay.

To the surprise of most flu specialists, however, the new H1N1 virus first appeared in North America, not the Pearl River Delta. “You know, we did all this stuff on H5N1 [avian influenza] as the next pandemic, and here we are and it's from North America [...] it's not what we thought,” one U.S. CDC official told me. An important reason that this was so surprising was that North America was not supposed to have the conditions that fostered the emergence of new flu-like viruses. Although the origins of the SARS virus to this day have never been determined (Janies et al. 2009), scientists immediately implicated “wet markets” – markets selling fresh produce, meat, and live animals common in the Pearl River Delta. They attributed the virus first to civets (a raccoon-like mammal) and then to bats, both “traditional” animals sold in some wet markets for consumption (Kan et al. 2005; Lau et al. 2005; Li et al. 2005; Tu et al. 2004). **[4]** By eating these wild animals, city residents in this part of China were portrayed as transgressing the line between nature and culture, and thus launching SARS (Shortridge 2005; Goudsmit 2004; Brookes 2005; Zhan 2005). As Mei Zhan argues in her analysis of SARS, “the story of ‘zoonotic origin’ did not blame nature itself for the SARS outbreak; what went wrong was the Chinese people's uncanny affinity with the nonhuman and the wild” (37).

Western media and Western scholars also pointed to the juxtaposition of China's many “backyard farms” with modern cityscapes as presenting another kind of viral danger (Bingham/Hinchcliffe 2008; Kaufman 2008; Lockerbie/Herring 2009). Backyard farms – small-scale family holdings of animals – were ubiquitous throughout Asia, and observers commonly described them in derisive terms familiar to any historian of epidemics (c.f. Shah 2001; Markel 1997). They were portrayed as filthy, backwards, and uncontrollable; they allowed for an unholy mixing of animals, humans, and waste; they were contrary to modern agricultural techniques; and they were inevitable breeders of disease (Bingham/Hinchcliffe 2008; Douglas 2002). Having been blamed for repeated outbreaks of H5N1 avian influenza since 1997, backyard farms joined wet markets as emblematic symbols of China's backwardness. Of particular significance was that the “farm-to-fork chain may be as short as a few meters” (WHO 2004) – implying an anti-modern failure to separate the meat one eats from the live animal from which it derives – or to separate nature (live animal) from culture (human food) (Douglas 2002; Levi-Strauss 1969). As with SARS, such backward transgressions were seen as dangerously

**[4]** The index case was said to have had contact with civets in a wet market prior to getting ill. Civets were later shown to be victims rather than perpetrators; apparently the virus passed from humans to civets rather than vice versa (Janies et al. 2008).

intermingling with the modern world in Chinese cities. According to Wald, “The ‘primitive farms’ of [the Pearl River Delta], like the ‘primordial’ spaces of African rainforests, temporalize the threat of emerging infections, proclaiming the danger of putting the past in (geographical) proximity to the present” (2008, 7–8).

Johannes Fabian has argued that “denial of coevalness is a political act, not just a discursive fact. The absence of the Other from our Time has been his mode of presence in our discourse” (1983, 153–54). Though other discourses about the origins of avian flu existed – some scientists eventually began to blame factory farms, for example (Otte et al. 2006) – Tianmai’s public health professionals interpreted the ubiquity of the backwardness discourse as a political challenge. The desire to transition from backward to modern and join the ranks of powerful nations had been an overarching goal for China’s leadership since the early days of Communist rule (Greenhalgh 2008), and locating the source of backward diseases outside of China’s borders had been a key part of this project since the time of the Patriotic Health Campaigns in the 1950s (Rogaski 2004). For my informants, the arrival of H1N1 in 2009 proved that this mission, at least in Tianmai, was finally complete: the Pearl River Delta was no longer the source of backward diseases, it was a modern destination.

From my Tianmai informants’ point of view, the next move, then, should have been clear. This time, rather than the Pearl River Delta, it was the U.S. and Mexico that were charged with containing the virus and preventing it from spreading beyond their borders. WHO and U.S. preparedness plans drafted in the mid-2000s stated that at phase 5 (declared on April 29), exit screening and other strict containment measures inside affected countries should be taken to stem the spread of the virus to other countries and delay the onset of a pandemic (WHO 2005a; U.S. HSC 2006). [5] The WHO plan suggested that at phase 5 affected countries should attempt to “exclude spread to other countries/regions” and “make massive efforts to contain or delay human-to-human transmission and the onset of a pandemic” (WHO 2005a, 32). My informants took these plans extremely seriously. As one Hong Kong flu specialist told me, “WHO is only a platform for communication. But on this side, in China, Hong Kong, Taiwan, they treat the WHO’s suggestion as gold. And this is what WHO said.”

But what the WHO plan said and what the organization did turned out not to be quite the same thing. By the time H1N1 was identified, the U.S. CDC declared that it was already too late to contain it (McNeil 2009). WHO concurred: although it did not declare a pandemic until June 11, the agency decided almost immediately in April that “geographical containment was not feasible, leading the [WHO] to call for mitigation” (Gostin 2009, 2376). The virus rapidly spread beyond North America,

[5] An updated WHO plan published in April 2009, at the same time that the H1N1 outbreak was occurring, changed the meaning of phase 5 to be essentially the same as the pandemic phase, and removed the recommendation for efforts to delay transmission during this phase, though it retained recommendations for exit and entry screening (see below, WHO 2009). My informants did not seem to be familiar with this new plan.

and on May 1, the first case of H1N1 was confirmed in Hong Kong. And with that, Tianmai's post-SARS preparedness apparatus kicked into gear.

## Creating a Public Health System of Preparedness

By 2003, the nationwide disease control apparatus that Mao had built during the height of Chinese Communism – a low-cost, prevention-based system credited with rapid, radical improvements in the health of Chinese – had been disintegrating quietly for years, a result of economic reforms that discouraged government investment in public health (Hsiao 1995; Liu et al. 1995; Liu/Mills 2002; Wang 2004). From the time of Mao's death in 1976 until the appearance of SARS, Mao's system of Anti-Epidemic Stations (AES), had been morphing into semi-private enterprises that supported themselves through paid sanitation inspections of restaurants, hotels, and factories. With the arrival of SARS, the AESs were given a new purpose, as a retooled national network of Centers for Disease Control and Prevention (CDCs).

SARS did not, technically, create the Chinese CDC system. But it did, largely as an accident of timing, co-evolve with it – or, as Jasanoff has put it, SARS and the CDC system were “co-produced” (Jasanoff 2004). When SARS appeared in 2003, the country was in the midst of undergoing a transition from a “Soviet system” to an “American system” (Peng et al. 2003; Lu/Li 2007). This process involved splitting each AES into a “Health Inspection Institute” (*weisheng jiandusuo*) and a “Center for Disease Control and Prevention” (*jibing yufang kongzhi zhongxin*) (CDC) (Lu/Li 2006). In Tianmai, this split took place during the height of the SARS outbreak, with the new Tianmai CDC opening its doors in May 2003. The health inspection institutes were charged with taking over the bulk of the sanitation inspections. The CDCs, on the other hand, were to focus on laboratory and field research, disease prevention and surveillance, and epidemiological investigations.

Prior to 2003, money for the high-tech labs, surveillance systems, and well-trained personnel that local public health professionals hoped to build into this system was lacking. The SARS response provided all of these things, but it also provided a very particular way of doing public health: the paradigm of pandemic preparedness as a model for public health praxis was built directly into the fundamental mandate of the CDC system. This emphasis was felt especially acutely in the Pearl River Delta. One senior TM CDC member explained:

“At the time, to tell the truth, people thought the CDC’s job was giving out vaccines, and disinfection, that’s all. [6] The whole society knew the CDC [after SARS], knew that we were primarily here for disease control, for acute infectious disease, we have this important position. [...] After SARS, that made clear what the goal of the TM CDC is. What are the goals of the CDC? What kind of work do we do? This is where the fundamental change came in. [...] Now primarily what we do is [...] gradually building preparations, contingency for sudden public health incidents, as well as some infectious disease response.” [7]

The name “CDC,” an explicit reference to the U.S. CDC in Atlanta, was meant to evoke the kind of highly modern, scientific ethos that Susan Greenhalgh (2008) has described in the context of Chinese reproductive policy. Indeed Chinese public health professionals from the local to the national level displayed an admiration for the U.S. CDC that bordered on worshipful. Numerous informants cited the American institution’s “technology,” “hardware,” “speed,” and skill in responding to outbreaks, as models for what they hoped to become. Visits to or from the U.S. CDC became instant status symbols that far exceeded the prestige of a similar visit to or from the national-level Chinese CDC. TM CDC workers clamored for collaborations with the U.S. CDC, which were most easily obtained for projects relating to influenza or to HIV/AIDS. Finally, almost everyone I talked with told me that their greatest public health hero was a U.S. CDC scientist in Beijing who ran a program called the Field Epidemiology Training Program (FETP), which has since 2003 trained hundreds of Chinese public health professionals in outbreak response. Those in the TM CDC who graduated from the program were promoted to top posts.

It is no accident that my informants looked to the U.S. CDC for guidance – the American organization had taken an active role in developing China’s preparedness and disease control capacities. Aside from the FETP program, dozens of U.S. CDC representatives reside in China and run a variety of programs including the China-U.S. Collaborative Program on Emerging and Re-emerging Infectious Diseases, the Global AIDS Program, as well as influenza surveillance and laboratory safety training programs. U.S. CDC employees living in China whom I interviewed embraced their positions as role models for the new Chinese public health system. One told me, “They do admire the [U.S.] CDC but really it’s rightfully so, because it really is the best in technology, best in personnel too. And [we] are the first ones on top of all the disease outbreaks, including AIDS, SARS, swine flu – right from the beginning, we are always doing the best work and the best investigations.”

[6] These were the primary tasks of the AES in Tianmai at the time that the CDCs opened.

[7] A separate Center for Chronic Disease Prevention and Control (CCDC) dealt with chronic disease response in Tianmai. The CCDC received less attention, less funding, and provided less status to its leaders and workers than the CDC, due to the greater perceived importance of infectious disease control.

My informants lacked any similar reverence for their own national-level CDC. The TM CDC was officially supposed to receive “direction” from the CDCs above it at the provincial and central levels, but such direction was rarely sought due to a general understanding among all parties that the technical capacities and strength of the TM CDC’s personnel exceeded that of the Guangdong provincial CDC and to some extent the central CDC as well. One Guangdong CDC department head, for example, told me that his Tianmai counterparts had many times the funding that he had, and “to be honest, they are better equipped and more highly trained – we couldn’t oversee them if we wanted to.” One TM CDC informant told me more diplomatically, “The provincial CDC, it’s not that we are very proud or look down on them. But [...] actually the quality of the personnel at TM CDC, and the extent to which we understand Tianmai’s situation, is deeper than theirs.” Even WHO had a particularly high regard for the quality of the work produced by the CDC in Tianmai, partly due to its positioning near Hong Kong. One WHO informant told me that it was the desire of Tianmai’s leaders to appear “international” which made Tianmai – along with other Pearl River Delta cities – a particularly strong site for international cooperation. She told me that due to the commitment of Tianmai’s leaders, several WHO projects had been piloted there.

Further strengthening the relative independence of the TM CDC was the fact that the national-level CDC had no codified power to enforce public health directives at the lower levels. Rather, at each administrative level in China (national, provincial, city, district, community), the Bureau of Health associated with that level exercised control over the corresponding level of CDC (a “*kuai-kuai*” horizontal structure – see Lieberthal 1995; Zhong 2003). At the same time, the CDCs had a “*tiao-tiao*” (vertical) structure of “technical direction” that was supposed to flow downward. The Bureaus of Health (BOH) also had a *tiao-tiao* structure of more formalized power that flowed from the Ministry of Health downwards. The TM CDC itself, meanwhile, was at the nexus of a citywide system of district- and “street”-level CDCs, each of which was under the jurisdiction of the corresponding BOH. Because the TM CDC was one of perhaps a dozen city institutions with public health-related functions, some of them under the jurisdiction of the BOH, some under other city Bureaus, and some under central or provincial control, it also had ad hoc “cooperation” (*hezuo*) relationships with these other institutions on a project-by-project basis. This limited the power of both the BOH and the TM CDC, as most projects required the cooperation of institutions not under the jurisdiction of the BOH.

Hong Kong also acted as an important partner. After SARS, Hong Kong’s Centre for Health Protection established official relations with the Guangdong provincial BOH, which involved meeting



monthly, agreeing to report outbreaks to each other without having to go through Beijing first, and establishing a 24-hour phone connection. The CHP also developed closer relations with the provincial and city CDCs in Guangdong province, but these connections remained informal. As a courtesy, the CHP notified the TM CDC about any changes in its emergency response policies that it made, “because this affects them, if we stop quarantining people, for example,” one CHP official told me. In the early days of H1N1 it sent faxes to the TM CDC whenever a suspected case or contact crossed the border. Still, as the CHP official explained. “Often Guangdong might want to follow us, but they have to wait for Beijing, they still have to do what Beijing says, so that’s frustrating for them.” TM CDC leaders were most responsible of all, however, to local government officials. TM CDC employees, meanwhile, answered only to their TM CDC leaders.

What all of this means for our discussion of H1N1 is that even in the case of a national pandemic control campaign like that launched to address H1N1, disease containment efforts were highly localized. The Ministry of Health put out general directives, but provincial and city leaders tended to make their own decisions about how to respond to those directives. The success of any national effort was ultimately reliant upon the cooperation of the local CDCs, and the leaders of those CDCs had a considerable amount of latitude as to how they wished to carry out any particular project. One Beijing-based WHO representative told me, “The guidelines [the national level] gives are the minimal to do. But that doesn’t mean the provinces and cities can’t do more.”

By displaying powerful pandemic preparedness capabilities that exceeded the demands of Beijing, my informants thought that they would show the world that the TM CDC was deserving of the CDC name. They would live up to the reputation they had built as one of the most advanced public health systems in China, on par with the best in the world. They would succeed where even the U.S. CDC had failed. They would stop a pandemic in its tracks.

## A Foreign Pandemic Arrives

In late April 2009, small sidebars began appearing in local Tianmai newspapers reporting that a new flu had been identified among sick children in California. By the time the cases in California had been traced to a larger outbreak in Mexico, assistants to the TM CDC directors had begun flitting breathlessly around the center, delivering notices to all department heads alerting them that “*lingdao gaodu zhongshi zhu liugan*” or “the leaders have made swine flu a top priority.” Local newspapers and television stations reported that President Hu Jintao himself had declared the prevention of swine flu

to be of vital importance. On the day the first Hong Kong case – in a Mexican tourist – was confirmed, flights were halted between Mexico and China, and the Health Minister gave directives for every level of CDC to track down and quarantine passengers who were on the same flight with the patient, to report and isolate all people with symptoms who had recently returned from Mexico or the U.S., and to quarantine all contacts of such people for seven days – all with the goal of preventing or at least slowing the virus from entering China (c.f. Cankao Xiaoxi 2009). As one Chinese national CDC official later told me, “Once [WHO] raised the [preparedness] level from 3 to 5, China immediately change[d] H1N1 flu from category B to A. That means they are more restrictive, you have to quarantine all the patients and also all the contacts.”

In Tianmai, the BOH issued a notice declaring that the city CDC, in conjunction with the city’s Quarantine Inspection and Control Bureau, had the responsibility of keeping the virus from crossing the border between Tianmai and Hong Kong – that is, of “defending the first line (*dì’yī xiàn*).” TM CDC leaders in turn issued a notice to all departments declaring that swine flu prevention and containment was now the center’s greatest priority, and called a rare center-wide meeting to review the initial steps to be taken. “Whether it’s bird flu or swine flu, the same principles apply,” the director of the center said at this meeting. He made it clear that this response was to be the culmination of the TM CDC’s many years of preparation, that everything they were going to do was entirely in keeping with “international regulations” – especially WHO pandemic preparedness schemes – and that the continued good name of the TM CDC and its members depended on success in preventing swine flu from taking hold in Tianmai.

The Tianmai media took up the cause, referring constantly to the SARS experience in doing so. Interviews with SARS hero Zhong Nanshan, a Pearl River Delta native and perhaps the most famous doctor in China, rehashed stories from Zhong’s heroics in identifying and stopping SARS, and offered up lessons for H1N1. A spread on the H1N1 threat in a local magazine declared, “From this H1N1 that was brought by North Americans, we can easily think back to the SARS panic six years ago. Actually today Chinese people remain in combative readiness for H1N1, and benefited from the life and death practice of six years ago” (Hong 2009, 35).

As initial measures, TM CDC leaders barred all those assigned to the emergency response team from leaving Tianmai until further notice. A 24-hour hotline was established to answer questions from concerned citizens. Disinfection equipment was prepared. The flu surveillance mechanisms that had been put in place after SARS were tightened, and lower level CDCs were told to report any suspected

cases immediately – though my informants complained that even in this instance assuring cooperation from lower level CDCs was difficult, as no district or community wanted to be the first one to report a case.

One of the first measures that the CDC and Quarantine Bureau jointly implemented was to require each person who crossed the border from Hong Kong to Tianmai to complete a health report attesting to his lack of flu symptoms and reporting where else he had traveled during the previous seven days. The Quarantine Bureau reassigned much of its personnel to man flu prevention booths, where they examined health reports, pulled aside suspect travelers for interviews or exams, and pointed laser thermometers shaped like guns at the foreheads of anyone who had transited through Hong Kong from an “epidemic region” – at first defined as Texas, California, or Mexico, and later Japan, the rest of the U.S., Canada, and Hong Kong itself. In keeping with Tianmai BOH guidelines, anyone reporting recent travel to an epidemic region and showing a fever of at least 37.5 degrees or any other flu-like symptoms would be taken to the designated swine flu hospital – an infectious disease hospital that was also the receiving hospital for SARS patients – to be evaluated by TM CDC workers. Anyone still exhibiting symptoms at that point would be isolated until swine flu was ruled out through laboratory tests, or for seven days after cessation of flu symptoms. In addition, TM CDC workers detained anyone reporting contact with a swine flu patient, or seated on an airplane with a suspected case, in a quarantine facility on the outskirts of Tianmai, where they were monitored and treated with Tamiflu and Traditional Chinese Medicine for seven days. **[8]**

In the early days of the pandemic, the TM CDC sent investigators to the hospital whenever a suspected case was brought in. Later it sent workers to stay in dormitories at the hospital for two-week shifts, where they were on call 24 hours a day. The vast majority of the educated young people hired since SARS to build up the TM CDC’s technical capabilities were pulled from their positions in other departments and assigned to carry out these tasks. All other programs, including surveillance for other infectious diseases common in the spring and summer, were effectively put on hold. Similar steps were taken across China, as well as in Hong Kong.

At the beginning of this campaign, a wave of excitement rushed through the TM CDC. Midnight calls to don full-body biohazard suits in the searing heat and investigate the steady stream of suspected cases were met with enthusiasm. Eager young workers who had spent their entire short careers training for a moment like this volunteered to take up residence in the quarantine camp. Quarantine notices were issued with a sense of importance, and incensed foreign travelers were calmed with

**[8]** This rule was later softened to include only passengers seated within three rows of affected passengers, and, as we shall see later, was eventually dropped entirely at the end of the summer. I was never allowed to visit the quarantine facility.

appeals to a moral high ground based in, as was explained to me, “the laws of our country and of the international community, internationally accepted regulations, and a responsibility to society and the world.” When moral appeals did not work, promises of laptop computers, free mobile phones, and Western-style meals helped to soothe unsteady nerves. The whole TM CDC bustled with excitement, a sense of purpose, and a feeling of pride that the people there were carrying out a rigorous response worthy of the CDC name. The media reinforced an aura of heroism. The front pages of newspapers and magazines throughout the month of May were splashed with photographs of public health professionals in biohazard suits and grateful patients, and articles recounted dramatic quarantine efforts all over China.

But within a couple of weeks, the tone at the TM CDC quickly soured. During the SARS epidemic, China’s leaders and public health professionals were internationally praised for implementing harsh but apparently successful control measures (Kaufman 2006; Saich 2006). But when they began to institute similar measures in response to swine flu – this time focusing on foreign travelers in an effort to contain the virus *outside* rather than *inside* China’s borders – the same organizations that had praised China six years earlier instead either offered only tepid support or even criticism, calling the response an overreaction. Meanwhile, Western news outlets flung accusations of xenophobia, and published harrowing accounts of tourists’ quarantine experiences in backward conditions at the hands of a draconian state (Metzl 2009; Stolberg/Robinson 2009). The Mexican government evacuated its citizens from China, accusing the public health establishment of human rights abuses after Mexicans in many cities, including Tianmai, were quarantined and subjected to medical tests without reason to suspect that they were infected (Singer 2009; Telegraph Online 2009). Scientists even began deflecting blame for the virus’ emergence away from North America, suggesting that some key components of H1N1 DNA had actually originated in East Asia (Trifonov et al. 2009). This move infuriated my informants: one young CDC worker confronted me and demanded an explanation for why the U.S. media was spreading lies that the virus was Chinese and not American.

The efforts that my informants thought would solidify their place in the international community instead seemed to them to be jeopardizing it. Their bureaucracy had been built exactly for this purpose, people had been trained, money had been invested, infrastructure had been built, and over and over again they had been warned that they were responsible for taming the next pandemic. But now that they were doing exactly what they thought they were supposed to be doing, they were being criticized rather than praised. Meanwhile, the U.S. CDC that they so admired as a role model seemed to them to

be standing by and doing nothing, allowing the disease to invade China. To many of those working so hard to institute Tianmai's swine flu measures, this felt like a betrayal. "The international community should support us. This is both in accordance with our own laws and with the [WHO's] International Health Regulations. It's the U.S. they should criticize, they are the ones who did not do anything to stop this," one epidemiologist told me.

## One Step Forward, One Step Back

In describing his experience in a Chinese quarantine in July 2009, Jonathan Metzl drew upon the trope of the backyard farm when he wrote in the *Los Angeles Times*, "The Chinese media have reported that travelers placed in quarantine are being held at five-star hotels, but if this is true, then the star system is in need of revision. Imagine a Motel 6 next to a chicken farm in the middle of a field. Then imagine that it had been left abandoned for a year before receiving a quick cleaning and sanitizing and a lot of new security features" (2009). Metzl went on to charge that by putting him and other healthy travelers in quarantine, the Chinese had acted out of xenophobia and fear. He concluded by declaring that his captors had done just the opposite of what my informants told me they thought they were doing: complying with international standards. "Chinese health authorities need to wake up to this lesson and develop China's ongoing H1N1 response in concert with, rather than in rejection of, international norms" (2009).

My informants at first vigorously defended themselves against these sorts of complaints, variations of which they told me they sometimes faced in their own quarantine facilities and also read about in local and international media reports. Repeatedly they insisted to me that their actions were not rejecting international norms, but were perfectly aligned with them. They cited WHO plans that presented the SARS response as an example of the kind of global action that should be repeated during an influenza outbreak. They cited the fact that no one knew at first how mild the virus would be, that judgments of severity in any case were not part of WHO preparedness plans – a fact that led to later international criticisms that the WHO itself had overreacted to what was essentially an ordinary flu (Reuters 2010) – and that WHO had warned that H1N1 could mutate into something more like H5N1, or even might mix with H5N1 in China (Fox 2009). They cited the training that the U.S. CDC and others had given them since SARS, and the expectation that they felt came along with this training that they should mount an aggressive response against the next influenza-like threat. They cited pandemic preparedness materials from the Ministry of Health that suggested that by following the successful

quarantine and containment activities, surveillance, disinfection, and Chinese medicine treatment methods used during SARS, similar results might be obtained for influenza (PRC MOH 2006, 2007a, 2007b). Local media also stated that WHO endorsed quarantine as a “long established principle in dealing with infectious diseases” and a “means that can be taken under special circumstances” (Nanfang Dushi Bao 2009).

Public health officials in the U.S. clearly did not think that the relatively mild H1N1 outbreak qualified as one of these special circumstances. What is less clear, however, is what virus *would* qualify. This is hard to surmise because though the U.S. preparedness plan acknowledged that a pandemic could begin in North America, the strategies that it laid out almost exclusively started with the premise that it would begin overseas, most likely in Asia, and that containment measures would be implemented overseas with the goal of preventing or slowing the spread of the disease from Asia to the U.S. (U.S. HSC 2006; U.S. HHS 2005). The plan for federal government response incorporated this assumption into the U.S.’ own phase system: phase 1 of pandemic response would be declared when a suspected human outbreak occurred overseas, while phase 4 indicated the arrival of the virus in the U.S. (U.S. HSC 2006). In a magazine interview, U.S. CDC acting director-general Richard Besser reinforced this position when defending his decision not to implement any border controls for H1N1: “So at the time that the outbreak was first diagnosed, it was already in the U.S. Our pandemic planning, overarching planning that was done largely around avian flu, had approached or looked at [an outbreak that] would originate off our shores” (Walsh 2009).

The investment they had made in training and infrastructure in China, my U.S. CDC and WHO contacts acknowledged, was an important part of their efforts to heed WHO’s call to “stop the spread of highly pathogenic bird flu at its source” (WHO et al. 2005). But though the U.S. CDC and WHO officials with whom I spoke seemed unsurprised by the Chinese decision to use large-scale quarantine for H1N1, they, along with many scholars, agreed that such large-scale involuntary measures would most likely never be implemented in the U.S. or any other “liberal democracy” – except perhaps as a last resort in the face of an extreme threat (Wynia 2007; Gostin et al. 2003). The U.S. plan, when describing the conditions under which stringent control measures might be taken domestically, indicated that only if the most basic functions of (American) society itself were at risk would one undertake wide-scale coercive restrictions (HSC 2006; HHS 2005). [9] Interviews with several U.S. CDC workers supported this view. “At the beginning I got messages asking, ‘how is this different from U.S. quarantine?’ And I said, ‘the U.S. doesn’t quarantine!’” one U.S. CDC informant told me. I asked

[9] Quarantines for suspected SARS cases were instituted in Toronto, Canada after the virus reached Canada in 2003, but they were for the most part voluntary and limited, and strict border quarantines like those in China were not imposed.

how high the bar would have to be for the U.S. to do something along the lines of what he had witnessed in China. Would cholera be reason enough? Ebola? “Yeah, I think there would have to be, like, blood coming out of your eyes!” he replied with a laugh. He went on to describe a speech that a colleague from the U.S. CDC gave at the China CDC during the initial outbreak, in which he told Chinese colleagues, “You know quarantine is about risks, and risks that society is willing to take – it’s an intervention that’s partly determined by your cultural values.”

An ethical guidance issued by the WHO in 2007 supported this cultural relativist stance, suggesting that though all measures that restrict liberties must be implemented only when “strictly necessary in a democratic society,” (WHO 2007, 9) latitude in terms of specific approaches “will depend on local circumstances and community values” (WHO 2007, 2). As Gostin et al note, “coercive strategies reflect conceptions of individual rights, the legitimacy of state intrusions, and the appropriate balance between security and liberty. Measures tolerable in an authoritarian regime would not be tolerated in a liberal democratic state” (2003, 3231–2). He and others concluded that as members of an authoritarian society, Chinese people would find it more acceptable to be subject to coercive practices than people of democratic societies.

One implication that came along with this type of analysis was that in addition to being better suited for non-democratic societies, quarantine was also better suited for non-technological and non-modern societies. Large-scale quarantine was presented as a blunt instrument associated with the past. Many scholars and journalists have noted with amazement that SARS apparently was stopped “with essentially nineteenth-century public health instruments” (Fidler 2004; see also Bayer/Fairchild 2004). Fidler noted, “The public health instruments at the forefront of the SARS battle were surveillance, isolation and quarantine, which were the main tools of infectious disease control in the historical era before the development of the arsenal of vaccines and antibiotics” (2004, 106). He went on to argue that for a public health response to be sustainable, diagnostics, therapies and vaccines must eventually replace quarantine (167). U.S. government scientists agreed; in a December 2009 article, U.S. Department of Health and Human Services Assistant Secretary for Preparedness and Response Nicole Lurie argued that though quarantine might sometimes be effective, “[t]he ultimate way to protect individual persons and populations from disease is with vaccination, and the rapid development and manufacture of the H1N1 vaccine represent a triumph of modern science” (2572). Quarantine, like backyard farming, was presented as a pre-modern practice that preceded the teleological development of superior technology. While still sometimes useful, it was not emblematic of

the sort of scientific, modern disease control apparatus that my TM CDC informants thought they were demonstrating.

Reinforcing this association, worst-case pandemic catastrophe scenarios outlined in scientific journals (c.f. Osterholm 2005; Belshe 2005), popular science writing (Garrett 1994; Davis 2005) and WHO pandemic preparedness materials (WHO 2005b) almost always conjured up the 1918 Influenza as the dark vision that could finally justify similar measures in a modern democratic society. Black-and-white images of makeshift hospitals and morgues circa 1918 are scattered throughout WHO literature on pandemic preparedness, for example, and the preface to the U.S. Pandemic Influenza Implementation Plan begins with a description of the 1918 flu (2006). Alfred Crosby, in arguing that SARS presaged the return of a 1918-like event, concluded, “There is a bitter little pill of a joke currently circulating among infectious disease experts. It is short: The nineteenth century was followed by the twentieth century, which was followed by the [...] nineteenth century” (2003, xiii).

The people of the TM CDC did not want to a symbol of the nineteenth century. They instead argued forcefully that they had not violated international norms – they had improved upon them. They consistently told me that the lack of democracy in China, as well as what they described as a persistent collectivist spirit, provided the structural environment needed to implement necessary control measures. They blamed a democratic system and an emphasis on individualism, on the other hand, for the inability of the U.S. to follow suit. Democratic governments would have done the same things China was doing if they were able, they told me – because it was the correct, scientific thing to do according to the principles of preparedness, they said.

In making this argument, TM CDC leaders departed from their praise of the U.S. CDC and presented U.S. inaction as a sign of weakness. One told me:

“In this area, when it comes to infectious diseases, I think that China has better administrative means than the U.S. – stronger and more effective. If the U.S. wants to do this sort of thing, it’s not easy. [...] A lot of our measures, maybe Americans say it’s human rights. For example, the current quarantines, they’ll say, I’m not going, you’re violating my human rights. Our country, in this area, is clear about having sense. It can take forcible measures.”

It was not just the leaders who made this argument. A young worker in the infectious disease department told me of the quarantine and other harsh measures, “Some people have been complaining



to their embassy, not understanding the situation. But really it's because Chinese leaders are actually ahead, trying to keep one step ahead [*qian yi bu*] of other countries." She pointed to the successful quarantines as an example of China's ability to "walk in front" of the international community by garnering widespread popular support for a more thorough, effective response. Similarly, informants at the Guangdong CDC claimed that 80 percent of Guangdong province's residents that they surveyed said they thought the virus was being effectively controlled through the local CDCs' aggressive measures.

## Science and Leaders

Support among TM CDC workers, however, quickly began to falter. As the pandemic response dragged on, the U.S. stopped reporting cases, and Hong Kong stopped tracking and quarantining contacts, the younger TM CDC workers began distancing themselves from the rhetoric about democracy that their leaders were espousing and that they had at first supported. They instead quietly began suggesting that their political system was perhaps not helping them to respond effectively to H1N1, but hindering them.

Foreign patients' complaints and demands had begun to wear on my young informants' patience and on their confidence that they – or rather their superiors – held the moral high ground. Yet their leaders, ever more eager to show the breadth and depth of the TM CDC's capabilities and make sure they were not blamed for letting in the virus via their city, announced almost daily additions to the set of tasks allocated to the increasingly unenthusiastic cadre of young people. The 24-hour telephone and laboratory shifts carried on long after the sense of urgency faded. Dashes to the hospital faded into weeks living in hospital dormitories. The weekend overtime shifts to call and check up on every single traveler who had crossed the border from Hong Kong into Tianmai, carried on even as Hong Kong stopped isolating those who felt sick. The burden of all of these activities fell on the newest, youngest, and most well-educated members of the CDC, who had trained in the shadow of SARS, organized outbreak simulation exercises, attended emergency management trainings, and chased after false alarm after false alarm of H5N1 avian flu, only to suddenly wonder what the point of it all was. They began to feel silly, frustrated, and finally bitter. They wondered why they were spending so much time on H1N1 while they were more or less ignoring more dangerous diseases that circulated in Tianmai every summer – such as hand, foot and mouth disease, which had been on the rise for years. As more and more countries abandoned any measures that even approached what they were doing, they no longer felt that Tianmai was "walking ahead" of the rest of the world.

They charged their leaders with failing to follow scientific norms that those in other countries were following. If the U.S. CDC was not responding to H1N1 in the way that the TM CDC was, then it must have its reasons – it was, after all, they asserted, the epitome of scientific modernity. Because of the leaders' failure to be scientific enough, and their excessive concern with losing face or losing their positions, they had in fact lost face with the international community and left them all in the current predicament, these young workers told me.

"The leaders need to change their strategy," said one. "You can see they're not paying attention [to H1N1 in the U.S.]. In China everyday they have a count, say there are so many in the U.S. and so many in China. But in the U.S. papers I look at online, they have nothing!" But when I asked her how such a change might occur, she shook her head. "There's nothing we can do about it – it's all determined by the leaders" [*meiyou banfa, dou shi lingdao anpai de*]. Another young informant told me that the problem was not so much with the leaders themselves as with what he called their "irrational" system. A system too focused on local prestige and on pleasing local leaders was unscientific and incompatible with a modern disease response, he told me:

"The defect with our system is that there's no way to change something once it's no longer useful. It's fine if you need to adjust a policy, there's nothing wrong with it – back then maybe it made sense [to do all these things we are still doing]. But now it doesn't, but no one's going to stop, because there's no way for anyone to stop, everyone just listens to the leaders, and the leaders are only worried about losing face. There's no process for anyone to adjust, it's completely irrational. That's why it's better in the U.S. to at least have a process, if something no longer makes sense, you change it, you follow scientific research, get experts to research this problem and decide what is the rational [*heli*] way of proceeding, and then change the policy. [...] And everyone here is only worried about pleasing the next higher level, that is, the next level above them. Because that's the one that's going to judge them. They don't care about the actual effect that it has, or rationality or science – as long as they make the next higher leaders happy, they're satisfied. So the leaders just say something based on what they think the next higher up will like, and then we have to do it."

Later, however, another colleague told me that the problem could really be traced back to a problem of national leadership:

“In China, everything is decided by the Party, by the national government, and right now because WHO made a big deal of it, and because of SARS and not wanting to have something like that, the national government is doing this. And until the national government owns up to the fact that this is not worth it and calls it off, no one region is going to let up.”

As the virus slowly made inroads all over China, however, the national CDC began to abandon quarantine as a strategy and to loosen its surveillance requirements, acknowledging by July that the disease had spread too far to be stopped. But Tianmai’s leaders continued to require detailed reporting and quarantining of contacts even after this national shift began. An informant who worked for the national Chinese CDC told me: “We keep telling [local CDCs] that they don’t need to chase every single contact anymore. They don’t need to test every single person who has any symptoms either. Actually they never needed to do that. So we need to explain this to them, but at the local level, it’s hard for them to understand, they are worried about face.” The TM CDC’s efforts to show just how modern and scientific its pandemic response system was ended up instead making many of its members feel that they were showing just the opposite. Late in the summer of 2009, the TM CDC leaders quietly closed Tianmai’s quarantine facility, the hospital teams were sent home, and the 24-hour hotline was shut off. The containment effort was over.

## **Conclusion: Toward a Global Debate on Pandemic Responsibilities**

In November and December 2009, as H1N1 became commoner than the common cold throughout the U.S., the same commentators who had declared China’s measures to be inappropriate began to suggest that perhaps they had been wrong. In a November 11 *New York Times* article entitled, “China’s Tough Flu Measures Appear to be Effective,” journalist Edward Wong noted that, apparently as a result of the measures, China’s total case count was dwarfed by that in the U.S. Wong went on to quote WHO Beijing office director Michael O’Leary as asserting, “I think there were a variety of measures put in place by different countries, and it’s difficult to say what worked best and what didn’t, but China’s has worked very well.” Aggressive containment measures, it seemed, were perhaps “rational” after all. In a more guarded assessment Lurie argued, “Many observers think that China’s isolation and quarantine policy, like the school closures in the United States, was disruptive. Unfortunately, we do not yet have adequate data to help us understand whether any of these measures worked, nor do we have a good

understanding of the levels of individual or social disruption that are acceptable to different people, communities, and countries.” (2009, 2)

Lurie raises a question that is critical to the development of a more meaningful global debate about pandemic preparedness measures. WHO stated in its ethics document that specific approaches to pandemic control “will depend on local circumstances and community values” (2007, 2). But whose values are “community values,” and to whom must social disruption be considered “acceptable” before it is to be used? If quarantine did work after all in slowing the spread of H1N1 and minimizing its effects, should the U.S. have attempted to implement it also? Could it ever have succeeded in doing so? If we are “all in it together,” when it comes to global pandemic control, should we all be required to sacrifice equally? And if not, who should decide for “the people” of a particular city or community what is acceptable and what is not?

The people of the TM CDC felt that they had to take the most aggressive measures possible against H1N1 – this was what they thought would boost their reputations and ingratiate themselves with WHO and the U.S. CDC. But that did not necessarily mean that they found quarantine acceptable when it came to restricting their own liberties. TM CDC workers seemed no more willing than the average American to sacrifice themselves for the good of the collective. One informant who had been working long shifts in the H1N1 ward, for example, admitted to lying about her contact with sick patients when she crossed the border into and out of Hong Kong, in order to avoid being caught in her own or her Hong Kong colleagues’ quarantine. On the other hand, as I have shown elsewhere (Mason n.d.), my informants did find quarantine and other severe tactics acceptable for others, especially those considered to be outside of society. Tianmai’s large internal rural-to-urban migrant population, made up of liminal outsiders whom my informants considered to be natural carriers of infectious diseases, is vulnerable to becoming the target of coercive public health measures in the future. And as my informants pointed out, one can never presume that a debate over the usefulness of adopting such measures would precede or follow their adoption.

My point here is not to suggest the U.S. acted correctly and China did not, or vice versa. Rather, I suggest that a more rigorous debate on the goals and tactics of pandemic preparedness is called for. This debate should acknowledge that any global disease response is composed of thousands of local responses, that global plans and global discourses have local consequences, that new diseases do not only emerge in “backward” places, and that both diseases and potential solutions flow in more than one direction (see Briggs and Nichter’s 2009 discussion of “biocommunicability,” for example). The U.S.

CDC, WHO, and other international organizations that have set up shop in China and invested heavily in promoting national and local public health reforms that prioritize influenza preparedness, must take more responsibility for the priorities they set, the models they put forth, the plans they issue, and the training they give. This is true not just in terms of what actions are taken, but also in terms of how systems are designed and what other priorities are being left out as a result. As we saw in the case of H1N1, priorities and plans – even if they are meant only as suggestions – carry real weight in local places, and real consequences. And, as the U.S. CDC and WHO are well aware, they are sometimes interpreted in the context of political procedures and structures that differ markedly from the places that first conceived them. This does not mean that the U.S. CDC and others should not be involved in China, but it does call for much more careful review of who is promoting what measures where, and whom the measures will really benefit. We must, as Atlani-Duault and Kendall suggest, first “map the varied truths constructed around the influenza epidemics and then [...] participate, together, in constructing new ones” (2009, 210).

Rather than hand-waving what happened in China as a case either of excessive overreaction or of a “culturally acceptable” measure not subject to outside judgment, the U.S. CDC and WHO – as leaders of global disease response and idols of public health professionals in China and perhaps elsewhere – need to take the lead in conducting this review. As scandals involving HIV/AIDS research conducted in Africa have born out (c.f. Petryna 2009), the use of ethical relativism in global health practice has usually served to protect those living in liberal democratic nations who are most likely to benefit from more permissive “values” overseas – generally in places marked as backward – without obliging anyone to take responsibility for those who are at the losing end of this equation. International organizations should avoid such quagmires by more seriously considering not just what countries blamed for emerging diseases are obligated to do in order to protect the rest of the world, but what the U.S. and others are obligated to do to protect those countries, and what everyone is obligated to do to protect individual people and the best interests of various populations. Local – and not just national – public health professionals must be part of this debate, for it is they who are familiar with the needs of communities and who are carrying out preparedness measures. And they should do so not just as students learning from the supposedly superior science and technology of the U.S., but as partners in deciding what form a truly global response can and should take.

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# Epidemic Intelligence.

## Langmuir and the Birth of Disease Surveillance

Lyle Fearnley

### Abstract:

In the wake of the SARS and influenza epidemics of the past decade, one public health solution has become a refrain: surveillance systems for detection of disease outbreaks. This paper is an effort to understand how disease surveillance for outbreak detection gained such paramount rationality in contemporary public health. The epidemiologist Alexander Langmuir is well known as the creator of modern disease surveillance. But less well known is how he imagined disease surveillance as one part of what he called “epidemic intelligence.” Langmuir developed the practice of disease surveillance during an unprecedented moment in which the threat of biological warfare brought civil defense experts and epidemiologists together around a common problem. In this paper, I describe how Langmuir navigated this world, experimenting with new techniques and rationales of epidemic control. Ultimately, I argue, Langmuir’s experiments resulted in a set of techniques and infrastructures – a system of epidemic intelligence – that transformed the epidemic as an object of human art.

**Keywords:** disease surveillance; social determinants of disease; epidemiology; biological warfare; preparedness

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*“[...] so great a plague and mortality of men was never remembered to have happened in any place before. For at first neither were the physicians able to cure it through ignorance of what it was but died fastest themselves, as being the men that most approached the sick, nor any other art of man availed whatsoever. All supplications to the gods and enquiries of oracles and whatsoever other means they used of that kind proved all unprofitable; insomuch as subdued with the greatness of the evil, they gave them all over.”*

Thucydides, *The Peloponnesian War*, Book II, (115)

The Greek historian Thucydides described the plague at Athens as a revelation of the vulnerability of human institutions. The failure of rites, knowledge and faith under the duress of death and disease overturned the order of the city itself. To this day, epidemics continue to provide the occasion for a reckoning of human knowledge and political order. Among international health experts and policymakers, the outbreak of Severe Acute Respiratory Syndrome (SARS) prompted such a critical occasion. These critiques moved swiftly from a recognition of the fatal danger of the virus to a condemnation of human failures – both scientific and political. Above all, these critiques focused on delays in recognizing the new virus and poor communication between health organizations.

SARS critiques singled out China as an opaque space, hidden from the view of international health institutions. This opacity had two dimensions. First, local health authorities did not immediately identify or report the existence of a new disease to the central government in Beijing. In addition, the Beijing authorities belatedly confirmed the existence of the epidemic to the World Health Organization (WHO), enabling the virus to spread to Hong Kong and around the world (see Fidler 2004; Kleinman and Watson 2006; Greenfeld 2006). As David Fidler (2004) concludes, SARS revealed the weakness of the disease detection and response capabilities of the international health regime. WHO quickly passed a resolution demanding the prompt and transparent reporting of all cases, as well as the sharing of information that might help prevent international transmission. In the 2003 *World Health Report*, WHO called for the extension of surveillance systems around the world in order to detect the appearance of new disease outbreaks. The *Report* declared that “across the board strengthening of systems for outbreak alert and response was *the only rational way* to defend public health security against not only SARS but also all future infectious disease threats, including those that might be deliberately caused” (WHO 2003).

This paper is an effort to understand how disease surveillance for outbreak detection gained such paramount rationality in contemporary public health. Paul Farmer (1999), Mike Davis (2005) and Nicholas King (2001, 2002, 2003), among others, have criticized this reliance on the detection of disease emergence. Epidemic detection, they argue, is a post-hoc technical solution, which ignores the correlation of disease outcomes with social inequality and the role of social conditions in the production of disease. [1] This paper returns to the birth of modern disease surveillance in order to identify precisely how the practice of epidemic control moved beyond the analysis of social conditions of disease. Alexander Langmuir, lead epidemiologist at the U.S. Centers for Disease Control and Prevention (CDC) [2] from 1949 to 1970, is credited by global health institutions with conceiving and refining the modern practice of disease surveillance (Declich/Carter 1994, 287). But less well known is how he imagined disease surveillance as one part of what he called “epidemic intelligence”.

Langmuir developed the practice of disease surveillance in the post-Second World War United States, an unprecedented moment in which the threat of biological warfare brought civil defense experts and epidemiologists together around a common problem. In this paper, I describe how Langmuir navigated this world, experimenting with new techniques and rationales of epidemic control. Ultimately, I argue, Langmuir’s experiments resulted in a set of techniques and infrastructures – a system of epidemic intelligence – that transformed the epidemic as an object of human art.

## A legacy of grandeur or malice?

In 1996, the *American Journal of Epidemiology* devoted an entire issue to remembering the life and work of Alexander Langmuir. The articles enumerated Langmuir’s manifold contributions to the sciences of air-borne pathogens, population control, veterinary health, and of course, the invention of disease surveillance. The narrative structure of this hagiography, however, relied on writing the history of public health as what Thomas Kuhn once criticized as a “development-by-accumulation” (Kuhn 1996). The authors situated Langmuir within a linear progressive development; standing between the nineteenth century hygienists and ourselves, he is described as “passing the epidemiologic torch from [William] Farr to the world” (Foster/Gangarossa 1996, 65). As one author declaimed, “we do indeed stand on the shoulders of those who came before us, and in public health, we are grateful for the shoulders of Alexander Langmuir” (Foege 1996, 15).

A few years after the honorary volume, however, Langmuir’s legacy and influence in American public health became the object of historical criticism. In an article published in the *American Journal*

[1] See especially Nicholas King (2001, 2002, 2003) for the history of the “emerging infections worldview” and its paramount demand for surveillance and information.

[2] When founded after World War II (see below), today’s Centers for Disease Control and Prevention was known as the Communicable Disease Center. But the acronym has always been CDC.

of *Public Health*, Theodore Brown and Elizabeth Fee (2001a) revealed Langmuir's historical relationship to the biological weapons defense establishment and warnings about biological sabotage. Langmuir, they wrote, exploited popular fears of biological warfare in order to build the CDC and disease surveillance infrastructure. Brown and Fee identified two specific harmful effects of this exploitation. First, by giving credibility to these fears, he "added legitimacy to one dimension of the fear-driven mentality of the Cold War era" (2001a, 725). Second, and more importantly, Langmuir's emphasis on the "exotic threat" of biological warfare "channeled the energy and narrowed the scope of epidemiological research to an infectious disease focus" (2001a, 725).

Brown and Fee questioned the idea that all investment in public health, no matter what the motive or objective, is a good. "Choices must be made," they wrote. "Funding towards one set of problems can be – and in our experience often is – diverted from others" (2001a, 725). In this case, while funding for biological warfare research and defense increased, funding for local public health departments decreased. They concluded by drawing a comparison to our own time, in which fears of biological attack are once again used to promote certain forms of public health preparedness.

D.A. Henderson, a former student of Langmuir's most known for his leadership of the global smallpox eradication program, disputed Brown and Fee's causal arguments in a subsequent letter to the editor (Henderson 2001). A member of the Epidemic Intelligence Service in the 1950s, he pointed out that only one out of fifty or so EIS officers worked on biological weapons threats at the time. In addition, while the decline of local health departments is indisputable, Henderson questioned whether Langmuir's biopreparedness efforts could be held responsible for this decline.

As Brown and Fee subsequently acknowledged, these two historical perspectives "do not see eye to eye" (Brown/Fee 2001). Amidst these crossed gazes that do not recognize each other, I believe the truth of Langmuir's work remains unseen. Langmuir's life and work reveal a transformation that is neither a "strengthening" nor a "narrowing" of public health. Instead, the articulation of civil defense planning with epidemiology – what he called "epidemic intelligence" – changed the nature of the epidemic as a scientific problem.

In order to make this transformation visible, I depict Langmuir's world as what Paul Rabinow calls a "problem-space" (see Rabinow 2003). Rabinow draws on Michel Foucault's concept of problematization as a moment of "uncertainty, a loss of familiarity [...] the result of difficulties in our previous way of understanding, acting, relating." The analysis of a problem-space does not see historical situations as givens of an existing narrative, but as *milieux* with both constraints and

possibilities. The world in which the defense against biological warfare became the task of epidemiologists forced Langmuir to develop and experiment with new techniques and goals. In order to understand how he transformed epidemic disease as a scientific problem, I locate Langmuir within the uncertain convergences of disease, warfare and government that characterized his world.

## Malaria, 1950

During the height of World War II, United States military authorities worried about the incidence of malaria among soldiers, particularly those at bases in the southern part of the country. In an experimental program called Malaria Control in War Areas (MCWA), epidemiologists joined with environmental engineers to eliminate malaria and malaria-harboring mosquitos around military bases. The project was strictly military and limited in scope to improving the health of soldiers during the war. However, MCWA director Joseph Mountain quickly began to imagine expanding the scope of the institution in both time and space. In a January 1942 memo, Mountain argued that “the defense emergency could result in an improvement in civilian health; that after the war, services having to do with the general population could be developed” (Etheridge 1992). In 1945, MCWA extended the malaria control program to include civilian populations. One year later, in the culmination of Mountain’s vision, MCWA merged with a couple of national public health laboratories and was renamed the Communicable Disease Center. The CDC soon became the most powerful arm of the federal Public Health Service.

Although the CDC was responsible for the control of a large number of communicable diseases, in the early years malaria remained central. In 1947, the malaria control program’s epidemiologists and engineers developed a five year plan for the total eradication of endemic malaria from the United States population (Andrews et al. 1950). Alexander Langmuir was the lead epidemiologist. While his engineering colleagues were digging trenches and spraying DDT, Langmuir began to develop new practices for tracking incidence and prevalence. Three years into the program Langmuir and two colleagues undertook a review of progress. The results were both surprising and provocative. Cases of malaria in the U.S. had declined from over sixty thousand in 1945 to around four thousand in 1949, apparently indicating that control and eradication methods were wildly successful. Yet Langmuir and colleagues were not convinced. Instead, they argued that “a change in the method of morbidity reporting [...] requiring the identification of patients [...] plus the elimination by states of obviously doubtful reports based on appraisal are responsible for the abrupt decline in reported malaria morbidity since 1947 (Andrews et al. 1950). In other words, the apparent abrupt decline was only a more accurate account



of actual malaria incidence. The solution was better collection of information regarding incidence and prevalence.

As Langmuir later recalled, it was in the midst of the malaria eradication program that surveillance of diseases was first implemented (Langmuir 1965). Before this program, doctors did report cases of certain diseases to state health departments. The state health departments collected these reports and periodically sent them to the federal Public Health Service. But disease reporting was by no means standardized across the country, nor were reports typically verified by states. The collected numbers were mostly used to keep long-term records of annual changes in prevalence of certain diseases (Thacker and Berkeleman 1988). During the malaria eradication program, Langmuir actively intervened in the reporting system, demanding that states eliminate obviously doubtful reports and that diagnosed cases be identified. Not trusting the original diagnostic report, Langmuir ensured that every reported case was reviewed by CDC trained nurses to ensure diagnostic accuracy. For final confirmation, the nurses collected blood samples to be sent to laboratories for analysis (Andrews et al. 1950).

By naming these new practices ‘the surveillance of disease’, Langmuir made a conceptual point. He wrote that “surveillance, when applied to a disease, means the continued watchfulness over the distribution and trends of incidence through the systematic collection of morbidity and mortality data and other relevant data” (Langmuir 1963).

Langmuir always claimed his ideas were simply elaborations of the 19<sup>th</sup> century London statistician William Farr’s work (Langmuir 1976). However, ‘surveillance of disease’ in fact differs significantly from the way Farr collected and analyzed reports of disease. The difference between Farr and Langmuir illuminates the difference between two epistemological fields of practice. Farr was a moral epidemiologist. He assembled reports of disease and death into statistical archives, analyzing the city of London into numerical populations and sub-populations with different average rates of disease. He aimed to identify the causal factors, which made certain sub-populations fall ill and die at far higher rates than others. Farr correlated sub-populations suffering higher average morbidity with patterns of social life or urban environment (including occupation, altitude above the Thames, or housing conditions) (Eyler 1979). 19<sup>th</sup> century statistical epidemiology revealed the conditions of existence that place human beings at greater or lesser risk of disease. Out of these investigations, epidemiologists developed norms of hygiene and health and disseminated them to fields as diverse as urban planning and education (Coleman 1982; Delaporte 1982; Rabinow 1995; Porter 1999a).

Langmuir's use of disease reports is quite different. Disease surveillance, in Langmuir's terms, collects reports of disease in order to provide a continuous picture of the actual extent of an epidemic. Rather than providing the material for causal determinations, the disease report enables a "continued watchfulness" over an epidemic. During the malaria eradication program, Langmuir did not attempt to determine causal factors or conditions, since he found these already established. He knew that a parasite transmitted through the bite of an *Anopheles* mosquito vector caused malaria. With this knowledge presumed, he worked to transform reporting requirements in order to more accurately detect the presence of these parasites in human hosts, including through the use of laboratory testing. By doing so, he discovered the epidemic was much smaller than anyone imagined. Through the vigilance of continued watchfulness, Langmuir suggested, the CDC could detect the appearance and disappearance of epidemics. The epidemic was transformed from a problem of population pathology into a discrete event framed by outbreak and subsidence.

## Epidemic Intelligence

In the malaria control program, Langmuir designed disease surveillance in order to track the gradual subsidence and ultimate eradication of Plasmodium parasites from the U.S. population. The use of disease surveillance to track an epidemic's subsidence to case 'o' is what makes disease eradication possible, as D.A. Henderson makes clear in his reports on global smallpox eradication (Fenner et al. 1988; Henderson 1999). However, Langmuir also made disease surveillance the basis of a second function: epidemic detection.

Langmuir elaborated disease surveillance for the detection of disease outbreaks, a function he referred to as 'epidemic intelligence'. In 1951, he persuaded reluctant state health agencies to standardize and expand reporting of certain diseases to the federal government. For the first time, all states agreed on a standard list of fifty-one diseases (classed as "of national importance") as the basis for national reporting, a system known as the National Notifiable Disease Surveillance System (NNDSS). At the same time, Langmuir personally organized a special group of federal epidemiologists called the Epidemic Intelligence Service (EIS). The construction of a mechanism for standard national disease reporting, as well as the creation of a team of epidemic "first-responders," transformed the *function* of disease reports. Langmuir's design of an epidemic intelligence infrastructure transformed disease reporting from an "archival function prior to 1950 to [a function] in which there is a timely analysis of data and appropriate response" (Thacker/Berkelman 1988).

The urgency with which Langmuir promoted this model of epidemic intelligence at first seems unlikely for the postwar period. Malaria and other infectious diseases were rapidly disappearing in the United States. Many public health researchers were calling for a shift in resources to follow the so-called epidemiological transition, the transition from infectious to chronic disease as the prime cause of population morbidity and mortality (Susser 1985, 149). And in fact, the impetus for epidemic intelligence did not come from the academies or departments of public health. The construction of the NNDSS was planned by civil defense administrators concerned about the potential enemy use of biological weapons. Langmuir was one of them. In order to understand how Langmuir turned disease surveillance from an epidemiological practice into a governmental institution, his concepts and practices must be situated within the broader history of civil defense of which they formed a part.

Civil defense was a central organizing principle of United States postwar social policy. After signing the Civil Defense Act in 1950, President Truman announced that “people, property, and production” had become concerns of national as well as social security (Federal Civil Defense Administration 1951). The legislation was deeply rooted in postwar military strategy. Military planners declared that contemporary technologies of warfare (air war, atomic, chemical, or biological weapons) erased the distinction between battlefield and homefront (Sherry 1977; Yergin 1997). With people, property and production as front lines of potential war, the protection of these resources became a national security responsibility.

In 1950, the National Security Resources Board published two volumes, which set out the role of civil government – including public health – in Cold War national security plans. In *United States Civil Defense*, the NSRB argued that the technical qualities of modern “air-atomic” warfare demanded a reconsideration of national defense strategy. Defense of military installations alone was insufficient. Rather, “productive power” (based on industrial plant, critical infrastructure, and human labor) and civilian morale were essential components of the military machine and required equivalent defensive measures:

“Since there can be no absolute military defense, an effective civil defense is vital to the future security of the United States because it might provide the means whereby this country, if suddenly attacked heavily and without warning, could get up off the floor and fight back.” (NSRB 1950a, 1)

Any city or factory was a potential target, and there was no way to know if or when an attack would come:

“The civil defense program for this country must be in constant readiness because for the first time in 136 years an enemy has the power to attack our cities in strong force, and for the first time in our history that attack may come suddenly, with little or no warning.” (NSRB 1950a, 7)

In a recent series of articles, anthropologists Stephen J. Collier and Andrew Lakoff describe the development of a “rationality of preparedness” in postwar U.S. civil defense planning (Collier/Lakoff 2007, 2008a, 2008b). Facing the threats of air war and nuclear munitions described above, administrators found the basic tools of risk assessment – probability calculations, population averages and insurance of normal conditions – ineffective. Drawing on knowledge of air warfare in Europe and the atomic bombs in Japan, planners determined that the destructive potential of warfare required new forms of preparedness planning. While the likelihood of an attack was low, the degree of potential damages was extremely high. Planners developed methods of preparing for low-probability, high-consequence events: early warning systems, vulnerability mapping, simulation exercises, and protection of critical infrastructure such as roads, electricity grids, and communication channels. Rather than absolute safety, or risk-based security, the strategy aimed to ensure the continuity of ‘critical systems’ of political authority and production if an attack did occur.

Yet air-atomic war was not the only catastrophic threat that preoccupied civil defense planners. The NSRB turned to biological weapons in a second volume published in 1950, *Health Services and Special Weapons Defense*. According to the NSRB, the characteristics of biological weapons differed significantly from air-atomic war. Whereas nuclear weapons brought entirely unprecedented dangers, the NSRB considered biological weapons a mere extension of natural forces.

“Biological warfare against people should not be looked upon as some mysterious, uncontrollable means of wholesale destruction of life. Actually, nature has directed biological warfare against man for thousands of years, but health workers have devised and applied constantly improving preventive methods.” (NSRB 1950b, 25)

According to the NSRB, an “efficient defense system” against pathogenic microbes could be found in the basic techniques of public health (Ibid., 201). Such a defense system would be equally powerful whether the enemy was “nature” or “man”. Yet much still needed to be done, and the practices of public health needed to be refocused.

“Today, with few exceptions, infectious diseases are well controlled in this country. The mechanisms, as well as the knowledge and experience to control biological warfare, whether waged by nature or by man, are present in our current health system. *The entire system, however, will need strengthening to be able to cope with enemy use of biological weapons.*” (Ibid., 25, emphasis added)

In order to strengthen the system, particularly against the threat of sabotage, the NSRB focused on improving what they called *vigilance*.

“Prevention of an overt attack with biological weapons is a military problem, but prevention of sabotage requires constant vigilance by civil agencies and civilians.” (NSRB 1950b, 25)

Ensuring constant vigilance required transforming the collection of morbidity reports into a method of epidemic detection. While the reporting system during peacetime “probably is sufficiently effective for the most dangerous diseases [...] for civil defense health services, the problem is somewhat different and the system is is probably not adequate” (Ibid., 170). Because of the unexpected and unusual epidemiology of biological sabotage, “routine detection methods would not be adequate to cope with such incidents” (Ibid., 205). The NSRB argued that the most pressing need was the “nationwide refinement and reinforcement of the present [morbidity reporting] system” (Ibid., 170).

“The reporting of cases of disease caused by biological warfare attack would be a necessary procedure to provide effective treatment and to limit the extent of damage to the population.” (Ibid., 203)

In conclusion, NSRB assigned the task of strengthening national morbidity reporting to the Public Health Service (Ibid., 205). Their suggestions did not go unheeded. In 1950, the PHS called a special meeting to address the problem of biological warfare for public health. Langmuir, who was at the meeting, later recalled that the outcome of the meeting was “common agreement that the basic need was for the development of strong epidemiological investigation of all types of epidemics occurring anywhere in the nation” (quoted in Etheridge 1992, 142). But what epistemological basis stood behind the common agreement that epidemic intelligence was the solution to the biological weapons threat?

## The potentialities of biological warfare

Langmuir began work in public health during the 1930s. At the time he strongly believed in social medicine. He later remembered his support of the Committee on the Costs of Medical Care's 1932 report that called on the federal government to enact social medicine policies. As recorded in an oral history interview, he then believed "the real future is to have the health society control the distribution of medical services" (The Reminiscences of Alexander Duncan Langmuir 1964, 28). During the war, however, he served on a high-profile epidemiological task force in the military, which was ordered to track and control outbreaks of acute respiratory illness among soldiers. The experience deeply transformed his view of public health. As he stated in the oral history interview, "the war completely turned [him] on to epidemiology, four solid years of epidemiology" (The Reminiscences of Alexander Duncan Langmuir 1964, 30).

Historian Dorothy Porter points out that the moral program of social medicine was, after the war, reduced to a technical program of epidemiological research (Porter 1999b; see also Porter/Porter 1988). While following a similar movement towards technical rather than moral problems, Langmuir differs in significant respects from the broad trend of postwar epidemiology. As he described, his epidemiological work within the military was "quite contrary to the study section research grant" epidemiology (The Reminiscences of Alexander Duncan Langmuir 1964, 30). Langmuir's team did not have the time to plan long-term statistical investigations into broad correlations of disease with socio-natural environments or lifestyle behaviors. Instead, military epidemiologists tracked ongoing disease outbreaks, identifying and isolating cases, and experimenting with control measures (different bunk arrangements, chemicals for cleaning surfaces) during the course of the epidemic.

Langmuir also saw his work taking place amidst a growing threat of biological warfare. After the war, Langmuir briefly taught at Johns Hopkins School of Public Health. There he met and befriended professor of epidemiology Kenneth Maxcy. Along with his academic duties, Maxcy served on the U.S. Committee on Biological Warfare. The highly classified committee, created in 1941, developed the program and strategy for biological warfare and defense. Langmuir often filled in for Maxcy on the committee and when Maxcy fell ill with Parkinson's disease, Langmuir took over full time. Beginning in 1947, Langmuir also served on the Army Chemical Corps. Administrative Council, the organization involved in offensive biological weapons research and production. By 1949, he had a higher security clearance than the surgeon general (Etheridge 1992, 41–42). [3]

[3] In later years (after the developments of interest in this paper), Langmuir also became assistant to the secretary of defense for research and development (1953–1959) and served on the DOD Committee on biological and chemical defense (1959–1961).

Biological weapons research in the United States skirted a fine line between defensive and offensive possibilities. The earliest investigations into the potential for weaponized disease were undertaken in 1941 in response to fears that Axis powers already possessed usable biological weapons. From the beginning, research was justified by Secretary of War Henry Stimson “because of the dangers that might confront this country from potential enemies employing what may be broadly described as biological weapons” (quoted in Moon 1999, 218). Early responsibility for the research agenda was placed under the newly formed War Bureau of Consultants (WBC), a civilian organization made up of academic experts in microbiology, many of them taken from the major research universities. The WBC concluded in its first report [19 February 1942] that “biological warfare is distinctly feasible. We are of the opinion that steps should be taken to formulate offensive and defensive measures. [...] There is but one logical course to pursue, namely to study the possibilities of such warfare from every angle, make every preparation for reducing its effectiveness, and thereby reduce the likelihood of use.” (Moon 1999, 219). And to blur distinctions between offense and defense further:

“It is obvious that preparation for defense necessitates a knowledge of offense, and if this knowledge is not available from experience, it must come from the results of careful investigation.” (Ibid.)

In 1942, President Roosevelt created the War Research Service as a department within the Federal Security Agency, the agency whose responsibilities were in social planning and public health. Roosevelt assigned pharmaceutical entrepreneur George Merck to direct the WRS in the research, development and production of biological weapons. The military took control of production once preliminary research seemed promising. In fact, as Stimson reported, “when War Research Service was first established, the primary considerations were research and secrecy so far as military participation was concerned. Therefore, this activity was placed in a civilian agency for more perfect cover” (quoted in Moon 1999, 232).

By the end of the war, the U.S. biological warfare program had investigated eighteen diseases for possible weaponization. While many diseases proved promising, only a few were proposed for mass production (anthrax and brucellosis in particular). These successes paved the way for an expanding program in the postwar period, especially before and during the war in Korea (Dando 1999, 49).

A contradiction plagued the biological weapons program, however. Throughout the 1940s, the program was justified as a method of heightening defensive capability against enemy use of biological weapons. However, while numerous offensive weapons, distribution mechanisms, and deployment strategies were developed, little success was achieved in the defense sector. In particular, although the very first report of the BWC had highlighted the potential enemy use of biological weapons against civilian populations (Moon 1999, 219), none of the biowarfare program's technical innovations (beyond the application of already existing vaccines and antibiotics) were designed for mass populations. Physical protection such as masks and clothes were unwieldy, expensive and probably ineffective outside the laboratory. Decontaminants, such as bleach and methyl bromide, were effective but obviously only in controlled or limited spaces (Ibid., 243–244).

Langmuir was well aware of this contradiction. He soon began to propose national notifiable disease surveillance as precisely this missing piece: a biological weapons defense system at the scale of the national population. In March 1951 he authored a piece entitled “The Potentialities of Biological Warfare Against Man”. The article set out to provide a “logical statement of a ‘theory of biological warfare’” that would supersede debates and controversy over the reality of the threat (Langmuir 1951, 387). He wanted to complement the broad strategies of the *Health Services and Special Weapons Defense* manual with concrete technical proposals. Many scientists at the time were skeptical about the feasibility of turning microbes into weapons. Langmuir bemoaned the lack of scientific appraisals. He wrote:

“Several hundred scientific papers have been published from Camp Detrick. These have direct application to our problem. The author is unaware, however, of any comprehensive scientific statement of the broad aspects of the problem that has been published from an official source.” (Ibid., 388)

Most importantly, Langmuir's theory defined the scope of biological warfare as a problem for public health. He wrote that “the problem may be limited to known disease agents and the potentialities of their use, whether by inhalation or ingestion” (Langmuir 1951, 389). He set aside the threats of the “super agent” and the “uncontrollable epidemic,” arguing that there was no means of rationally preparing for them. Building on the claims made in *Health Services and Special Weapons Defense*, Langmuir argued that an extension of existing public health practices could ensure preparation for biological weapons attack.



At the same time, Langmuir worried about existing capabilities for epidemic intelligence and vigilance. In a second article, Langmuir outlined the vulnerabilities of an unreformed notifiable disease system. He described a scenario of potential biological attack:

“Medical care facilities would be grossly overtaxed early in the epidemic. Emergency medical services would have to be organized as rapidly as possible. Laboratories would be swamped with specimens, but except in a few places personnel and facilities would be grossly unprepared to provide a prompt specific diagnosis. Depending on the agent used in the attack, *it might be days or weeks before an etiologic identification could be made.*” (Langmuir 1952, 236)

And in the absence of etiologic identification, therapy and prophylaxis through anti-microbial drugs or vaccines would be ineffective (Ibid.). Through the scenario, Langmuir laid out the core elements of a public health response to biological attack: (1) the early detection of the beginning of an epidemic, and (2) the identification of the causative pathogen.

In addition to conceptualizing the problem, Langmuir described two institutional reforms, which he believed would improve vigilance. Langmuir argued that “any plan of defense against biological warfare requires trained epidemiologists alert to all possibilities and available for call at a moment’s notice” (Langmuir/Andrews 1952, 237–8). This idea was realized in the Epidemic Intelligence Service. First organized by Langmuir in 1950, the EIS trains an annual class of epidemiologists and places them “on call” for epidemic alerts. Once an epidemic is reported, EIS officers are rapidly deployed to the site where they investigate and attempt to determine the etiology of the disease. After identification, they assist states in the implementation of control measures and, when the epidemic subsides, return to CDC headquarters (Langmuir 1980). Today the “disease detectives” of the EIS are involved in epidemic response activities around the world, and many countries have developed field epidemiology institutions modeled after the EIS.

Second, Langmuir also proposed building a new national morbidity reporting system. He wrote that “[...] with a strong intelligence system, based on prompt morbidity reporting, the beginning of the epidemic might be appreciated hours or even days before it was clearly apparent to any single physician.” (Ibid., 237)

Morbidity reporting, he emphasized, was necessary to guide immediate interventions, rather than for long-term archival research. He wrote, “morbidity reports are indispensable for immediate

recognition of a disease situation which requires public health action. [...] The protection of our communities depends upon immediate notification of the occurrence of these [notifiable] diseases so that, once a diagnosis is made, proper measures may be instituted.” (Langmuir/Sherman 1952, 1250)

Langmuir’s concept of a “strong intelligence system” was the blueprint for the Public Health Service’s reform of national morbidity reporting. Following the pivotal meeting regarding biological weapons, the PHS organized a Committee on Communicable Disease Reports to consider reforms. The Committee presented its proposals to the Association of State and Territorial Health Officers (ASTHO), the primary body for coordinating interstate health affairs, that fall. The plan outlined a number of arguments in favor of a standardized and intensified national notifiable disease system. Along with the archival collection of vital statistics, the committee argued that “civil defense against biological warfare requires immediate central notification of outbreaks of disease” (Public Health Service Committee 1951, 5). Rapid reporting and analysis would be essential for national defense as well as public health:

“Biological sabotage by water or food supplies or by aerial contamination of strategic buildings might produce serious consequences. Adequate defenses against such attacks are difficult to visualize but the importance of ‘epidemiological intelligence’ and the thorough investigation of all epidemics as they occur is patently necessary. The proposal for regular reporting of epidemics and outbreaks has, therefore, not only a solid justification in the logistical development of the peacetime health program but also peculiar significance in the defense of the Nation.” (Ibid., 11)

The Committee presented to ASTHO four major recommendations: 1) universal national reporting by States to the National Office of Vital Statistics, 2) a standard list of minimum notifiable diseases, divided into groups requiring immediate, weekly, or annual reports, 3) a “new mechanism” for the weekly reporting of epidemics and outbreaks, and 4) recommendations for a standard morbidity report card collected by the states from physicians (including a model card) (Ibid., 5).

Langmuir convinced the ASTHO to call a special conference in order to enact the federal recommendations. Langmuir appointed himself general chairman of the subsequently formed Conference of State and Territorial Epidemiologists [4] and his CDC colleague Dr. R. E. Serfling as executive secretary. While ostensibly the state epidemiologists held authority over morbidity reporting procedures, they were largely inexperienced and under the sway of the federal experts (Etheridge 1992, 32). Heavy lobbying by PHS included at least two presentations on civil defense and biological

[4] Later called the Council of State and Territorial Epidemiologists.

weapons (Flinn/Kiefer, 1951). The final report of the Conference in September, 1951 enacted all of the federal proposals (CDC 1951).

## Enquiring of today's oracles

Struggles over Langmuir's legacy are also struggles over the meaning and direction of contemporary public health. How do these histories reconstruct the relationship between Langmuir's life work and our contemporary world? Laudatory hymns to Langmuir the great man are built on a developmental history of linear progress. Using the metaphor of "passing the torch," they describe the persistent forward march from William Farr to John Snow to Langmuir and then to today. Langmuir's influence is truly global, the memorialists point out: in 1968, he organized the Technical Assembly of the WHO, producing "for the first time, an explicit global agenda for disease surveillance" (Thacker/Gregg 1996).

Brown and Fee use a second metaphor to provide a critical lens on both Langmuir and present day public health: the image of *déjà vu*. They argue that certain mistakes made in Langmuir's time are being repeated today. The alleged threat of bioterrorism is being exploited in order to build public health capacity, but at a serious cost. Funding to public health for bioterrorism prioritizes surveillance for "exotic" infectious disease threats at the expense of treating the existing social determinants of disease. All of this bears a "clear historical parallel" to Langmuir and his time, Brown and Fee warn.

In this paper, I have argued that Langmuir and his work do not fit into either narrative. He neither simply extended public health, nor distracted public health away from the real causes of disease. Langmuir's world brought together military defense and social planning, the dangers of enemy sabotage and contagious disease, and the tools of emergency response and epidemiology into a space of common problems. Doing so ultimately transformed the disease epidemic as a scientific object.

For the moral epidemiologists, the disease epidemic was a consequence of pathological and dangerous conditions of life. Epidemiology was the science that aimed to identify these pathological conditions and disseminate norms of health to society. But in the framework of epidemic intelligence, the epidemic was an event extrinsic to conditions of life. While the actual instigator could be either natural or human, the figure was best captured by the image of the saboteur. Although the saboteur takes advantage of conditions of life (water sources, food supply chains), in fact he is always foreign to these conditions. The goal of epidemic intelligence is to quickly identify

the appearance of an epidemic, trace the causal “saboteur,” control and eliminate the discovered vulnerability. Langmuir built an infrastructure of epidemic intelligence that enabled ‘the epidemic,’ whether natural or deliberate, to be conceptualized anew: as the appearance of a discrete event.

SARS disappeared as unexpectedly and mysteriously as it began. In the summer of 2004, new cases simply stopped appearing. Experts debated, without conclusive findings, what factors brought the outbreak to a close; whether the intensive quarantine and social distancing measures employed by some governments, or a simple rise in temperature with early summer. However, although no longer causing disease, the moral efficacy of SARS remained strong. China rebuilt its disease surveillance infrastructure, using computers and internet to connect every local health department and county hospital to the national health authorities in Beijing. In collaboration with the U.S. CDC, China is developing a field epidemiology training program modeled on the Epidemic Intelligence Service. Communication with international health institutions is also now frequent and continuous. The WHO, for its part, overhauled the International Health Regulations, strengthening the reporting requirements of member states. The new IHR (2005) shifted reporting requirements from specific pestilential diseases (cholera, plague, yellow fever) to a decision instrument defining “public health events of international importance”.

Although SARS could be called the “specific cause” of these transformations in public health practice, I argue that it is the logic of epidemic intelligence that made *surveillance* a dominant solution to emerging infectious disease problems. A reconstruction of the historical development and globalization of surveillance practices from Langmuir’s time to our own are beyond the scope of this paper. Further research must investigate the concrete historical process of this global transmission, as well as the myriad new forms of surveillance deployed to monitor animals, news reports, rumours, pharmaceutical sales, and so on. However, Langmuir’s life and words address an epistemological question fundamental to an understanding of today’s epidemic order. The development of epidemic intelligence makes clear one domain in which the problem of epidemic disease came to be distinguished from the analysis of social and environmental pathology. And across the world today, disease surveillance systems, these modern oracles, maintain constant vigilance for the next epidemic event.

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## A Predictable Unpredictability.

### The 2009 H1N1 pandemic and the concept of “strategic uncertainty” within global public health

Theresa MacPhail

#### Abstract:

This essay will examine the seemingly new paradigm shift within global public health from the use of a scientific “certainty” to a biological and situational “uncertainty” as one of the foundations of response to infectious disease outbreaks. During the recent 2009 H1N1 influenza outbreak, national and international public health officials often referred directly to the “uncertainty” surrounding both the virus itself and of the course, duration and severity of the pandemic. The vague and flexible concept of “uncertainty” – especially as it was employed by top virologists and epidemiologists in relationship to questions about the predictability of the influenza virus – provided the scientific foundation for much of the rationale behind both national and international health responses to the global pandemic. Public health officials, epidemiologists, and scientists often deployed a type of “strategic uncertainty” as an effective tool for gaining or retaining trust and scientific authority during the H1N1 pandemic.

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**Keywords:** ambiguity; global public health; influenza; medical anthropology; strategic uncertainty



*“Every outbreak is unique. Every new strain of virus is unique and until the outbreak has progressed you don’t know what it’s going to do and so it’s a matter of making decisions with incomplete information.”*

Richard Besser, *CDC Daily Press Briefing, April 2009*

Uncertainty has arguably become somewhat of a “hot topic” issue of late, debated and discussed in both policy and academic circles, as well as in popular media. One need only look to the 2009 global economic crisis, ongoing scientific research on the effects of man-made climate change, the recent elections in the United Kingdom, or the 2010 BP oil spill off the Gulf Coast of the United States to see examples of “uncertainty” playing out in the so-called real world. Especially observable throughout the latest novel influenza outbreak that began in the spring of 2009, uncertainty is as rife within global public health as it is in world economics. This ostensibly new, or reinvigorated, concept of uncertainty remains as pervasive inside the World Health Organization (WHO) as it is inside the International Monetary Fund (IMF) or the World Bank. This type of omnipresent and sustained uncertainty, it seems, is now ubiquitous to modern life. Dealing with such extended and global crisis situations has predicated a new type of response as well as need for a new type of analytic.

Borrowing from anthropologist Paul Rabinow’s elements for doing an anthropology of the contemporary, I begin my inquiry into the use of “uncertainty” during the 2009 H1N1 pandemic “midstream” and with “tentative parameters” of both the situation and what is at stake (2008, 8). Throughout the pandemic, I worked as a qualitative researcher on a large, interdisciplinary project. The Global Infectious Disease Response: Emergent Networks, Distributed Sense-making (ENDS) project is an attempt to observe public health agencies from the inside out in order to discover – in real time, so to speak – how information is shared and processed during an outbreak, as well as how the various people working within public health make sense of uncertainty. Throughout the pandemic, beginning in October 2009 and continuing until May 2010, I performed ethnographic research in two diverse field sites in different countries. During that time, I worked within a national public health agency alongside epidemiologists, observed inside a laboratory that dealt with influenza samples, and interviewed dozens of virologists, epidemiologists, analysts, modelers, and public health officials. I was an observer of people observing the influenza pandemic, making me – as Paul Rabinow suggests via Niklas Luhmann – a second-order observer of the various first-order observations of the many professionals with whom I interacted (2008, 62–66).

All through the months in which I worked closely with and spoke to various scientists and policymakers, the term “uncertainty” was rarely used in normal day-to-day conversations, during teleconferences, or in the various meetings I attended. However, the people around me often discussed information gaps, or what they didn’t yet know, and the sheer unpredictability of the virus itself. In the many informal conversations between colleagues that I observed, the talk frequently turned to questions regarding the severity of the virus and its biological makeup and origins, the problems in obtaining crucial clinical information from affected areas, or about the difficulty in ascertaining the “denominator” of cases – or the total of how many individuals had been infected with the virus. There was much that was unknown about the emergent outbreak, and efforts were constantly being made to ascertain as much information about the virus as possible in order to lessen this uncertainty.

Uncertainty is, of course, nothing new within the realm of science – epidemiology and virology included. The scientific process was crafted, at least in part, to deal with the rather slippery reality of uncertainties in the world beyond the laboratory. Scholars involved with or working inside the field of science and technology studies (STS) have often focused on the ways in which uncertainty in science is artfully turned into “socially-constructed” facts (see Callon 1999[1986]; Knorr-Cetina 1999; Latour/Woolgar 1979; Shapin/Schaeffer 1985). Science produces facts and theories about the world through the practice of examining the realm of the unknown. Examinations of the daily practice of science have highlighted just how adept scientists are at utilizing the scientific method both to garner and to retain a certain authority in relationship to their subjects and fields. Indeed, I will argue throughout this essay that scientific authority persists not despite uncertainty, but *because* of it. Uncertainty is the fertile ground for further scientific research and funding. Sustaining a partial uncertainty, grounded as it were in the ontological unpredictability of viruses, while being capable of both effectively managing that uncertainty and continuing the work of producing scientific facts – or certainty – about the virus, helped professionals working in global public health to maintain the current or reigning research paradigm. It is this *strategic utilization of uncertainty to positive effect* that is the focus of my examination of the 2009 H1N1 pandemic throughout this article.

In line with this thinking, then, one might make a valid point by arguing that an examination of uncertainty within public health is anything but new. Scholars of the themes of risk and preparedness have often pointed out how uncertainty is used within public health and policy circles to undergird planning and research paradigms to cope with possible future biological threats or devastating pandemics (see Lakoff/Collier 2008). This type of uncertainty is conceptually related to a risk that

occurs at some point in the future, but not one that is unfurling in the present moment. I will argue below that the meaning of uncertainty itself has shifted. Uncertainty as it pertains to risk and preparedness for a *possible* infectious disease event differs qualitatively from uncertainty as it pertains to risk in the present moment or immediate future *during* an infectious disease event. There is little risk of undermining scientific authority when admitting the future cannot be predicted (partially due to the fact that the specific infectious agent cannot be known in advance). Intuitively, one would surmise that there would be a much greater loss of authority as a result of admitting that the present moment was unpredictable because the disease agent itself, as well as the parameters of the developing situation, was not fully understood. This is why, in the not-so distant past, public health professionals were often loathe to openly discuss uncertainty. Throughout the early months of the 2009 H1N1 influenza pandemic, however, top public health officials regularly explained the uncertainty of the developing situation and appealed to the general public's understanding and patience. These pleas were often coupled with scientific explanations of the complex, ever-changing and ambiguous situation, with the influenza virus itself being cast as "predictably unpredictable" in its biology, behavior, and spread. Public health professionals habitually and liberally used the concept of "uncertainty" in official communications to justify immediate response measures or to preempt and clarify any future changes in recommendations and actions. In effect, then, scientific authority was at least partially maintained through *the strategic deployment of biological uncertainty* regarding the H1N1 virus itself.

In what follows, I will first examine how biological science has effectively underpinned the rhetorical casting of the virus itself as innately "unpredictable." Analyzing scientific articles, media stories, quotes from top scientists and epidemiologists, and data gathered throughout my own ethnographic fieldwork, I will attempt to highlight how the influenza virus's predictable unpredictability – a term scientists and epidemiologists frequently used to describe the virus both in conversations with me and in the press (Altman 2009; Sepkowitz 2009) – is connected to the creation of a sustained uncertainty within influenza science. I will then move on to look at how other "information gaps" are linked to uncertainty during an influenza outbreak, analyzing a random selection of media reports and interviews as well as relying on my own experience working within a public health agency during the so-called second wave of the 2009 H1N1 pandemic. Finally, I will argue that the fostering and public expression of scientific uncertainty was used strategically to either gain or retain trust during the 2009 H1N1 pandemic. The frequent deployment of what I will term

“strategic uncertainty” [1] was, and largely remains, an effective method of retaining authority and control during an outbreak of infectious disease. Management of a sustained and partial ambiguity [2] or uncertainty in relationship to the production of scientific knowledge about the influenza virus itself becomes a tool here – with “strategic uncertainty” at the forefront of a new “epidemic order” in global public health.

## The scientifically predictable unpredictability of influenza

As soon as rumors and media reports regarding an unusual, late-season outbreak of influenza in Mexico began to circulate in March 2009, international scientists and epidemiologists working on influenza in public health focused upon a set of objectives that related to gaining a better understanding the virus itself. First, public health agencies sought to obtain samples of the virus; next, virologists began to subtype those samples in order to ascertain which specific strain of influenza virus was causing the outbreaks; concurrently, evolutionary virologists began an immediate, international and collaborative effort to genetically sequence and analyze the virus in order to better understand its origins. Many public health experts believed that knowing more about the genetic makeup and origins of the influenza virus might help them to make not only better predictions about the severity and spread of the virus, but about the scope of the burgeoning pandemic. Thus, gathering information about the biology of the virus itself was crucial not only to the analysis of events as they unfolded in Mexico and in the southernmost states of the United States, but to the ability of public health experts to predict the immediate future.

By the end of April, it was evident to many of the virologists and epidemiologists who specialized in influenza that something big in scale was unfurling. An influenza pandemic was at hand. The question then became, how bad would it be? At this stage, data regarding the severity of the H1N1 virus mattered. Severity, however, is not a concept that is easily defined, especially as it related to the 2009 H1N1 pandemic. Generically speaking, understanding severity involves knowing something about a virus’s virulence and transmissibility, as well as the ability to calculate the percentage of severe cases or deaths out of the total number of persons infected. Information that pertained to severity was hard to come by, especially in the first weeks of the pandemic, and people I spoke with often complained about the absence of “good data” on the total number of infections. The “problem of the denominator” and better data regarding the biological attributes of the virus itself were often cast in the conversations I had with public health experts about the early days of the pandemic as *the* key pieces of information

[1] My use of strategic uncertainty here is distinct from the term as originally coined within economic theory by Van Huyck, Battalio, and Beil (1990). As Donald Moynihan has explained it, strategic uncertainty in economic and management theory typically refers to a specific type of uncertainty that “arises because networks contain multiple actors who retain some measure of strategic autonomy, creating uncertainty about what choices they will make” (354). Thus, “strategic” is a qualitative term used to describe their type of uncertainty being experienced by actors in a network, “as the various actors seek to maximize their position in the network but know little about the intentions of other actors” (Moynihan 356). Strategic uncertainty as I utilize it here refers instead to the strategic *deployment* of uncertainty, where strategic is a descriptive term used in relationship to an actor’s intentions when discussing uncertainty. My usage here relates, then, to how uncertainty itself becomes a rhetorical device or narrative tool for retaining scientific authority during the pandemic.

[2] I first began thinking about the role of ambiguity in public health after a correspondence with Dr. Linsey McGoey regarding a 2009 workshop she organized at the University of Oxford’s Said Business School, entitled “Strategic Unknowns: The usefulness of ambiguity and ignorance in organizational life.” The conference examined the various political, economic and social uses of ambiguity and ignorance in a variety of fields and sites. The economic concept I use throughout this essay, “strategic uncertainty,” is in many ways an outgrowth of my engagement with the idea of the “strategic unknown.” Ambiguity here is used to refer to the opacity inherent to the *production* of scientific information, whereas uncertainty is used to denote an ontological property of the *knowledge* produced about the virus itself.

that epidemiologists needed in order to recommend an appropriate set of responses and often chronically lacked. One of the biggest problems seemed to be the “unpredictability” of the virus. This rather predictable unpredictability would become central to the story that was developing about the 2009 H1N1 pandemic.

In an analysis of the characteristic stories or “narratives” constructed about infectious disease outbreaks, scholar Patricia Wald has suggested that: “As epidemiologists trace the routes of microbes, they catalog the spaces and interactions of global modernity” (2008, 2). Going further, she adds that “the outbreak narrative is itself like the epidemiological map and the electron microscope, a tool for making the invisible appear; it borrows, it attests to, and helps to construct expertise” (Wald 2008, 39). Following Wald’s lead, then, I argue that it is necessary to read closely and begin to critically examine the “narratives” about unpredictability and uncertainty at the heart of the 2009 H1N1 pandemic. By doing so, we can begin to unpack how the representation of the virus as unpredictable was strategically utilized – operating at least in part as a rhetorical tool – to maintain scientific authority throughout the pandemic.

From the start, uncertainty about the virus was rife. [3] Some of the first media articles published about the outbreak highlight how the virus itself was being cast as intrinsically unpredictable. One of the earliest stories on the pandemic in *Science* suggested that: “Much confusion surrounds the origins of the virus, why it seems to cause severe disease in Mexico and not elsewhere, and the overall threat it poses to the world. ‘Right now, there’s more unknown than there is known,’ says microbiologist Francis Plummer” (Cohen/Enserink 2009a, 572). This particular article, first published on May 1, goes on to quote the then-acting CDC Director Richard Besser as attesting to the fact that decisions were being made based on “incomplete information” (Cohen/Enserink 2009a, 573). The very next week, *Science* again reported that although information was being collected and shared internationally – and at an unprecedented speed – there continued to be many “mysteries” about the virus (Cohen 2009). A segment on the developing situation first broadcast on May 1 and then published on NPR reported that: “Experts still lack critical information about the virus” (Silberner/Greenfieldboyce 2009). An article in *The New York Times* during the first week of the outbreak emphasized the fact that even the WHO had admitted uncertainty about the virus, stating that: “The World Health Organization said over the weekend that the new swine flu virus had the potential to cause another pandemic, but that it had no way of knowing whether it actually would” (Altman 2009). Within the same article, the virus itself was being blamed for the uncertainty, while the authority of the scientists was upheld. The

[3] I do not mean to suggest that uncertainty about the influenza virus or the pandemic itself was wholly manufactured. The public health experts that I interviewed felt that there was much “uncertainty” about both the virus and the events themselves – especially during the first few months of the pandemic. What I find most interesting – and what I will focus on within this article – is how they spoke about or deployed that biological uncertainty to positive effect, and how uncertainty was partially managed by transforming it back into certainty about the unpredictability of viruses.

journalist explained that: “For all that scientists have learned about influenza since the catastrophic pandemic of 1917-19, one thing has not changed: the predictably unpredictable nature of the viruses that cause it” (Altman 2009).

The virus in these narratives is often described as a “mystery” – the implication being that unpredictability is an ontological property of the virus itself. That unpredictability, in turn, leads to an operative condition for “uncertainty” for public health. It is not inconsequential that the situation with influenza is consistently cast as inherently unpredictable; there is no end to uncertainty in this formulation. Indeed, there is also no clear beginning, as the virus was consistently put in a comparative frame with other pandemic influenzas viruses from the past. A scientific article published online in *Science* on May 11 stated that “although substantial uncertainty remains, clinical severity appears less than that seen in the 1918 influenza pandemic but comparable with that seen in the 1957 pandemic” (Fraser et al. 2009, 1557). Here, scientists have begun to analyze the “uncertainty” of the 2009 H1N1 virus in relationship to other viruses with the same or greater amounts of “unpredictability.” The scientists collectively argue that: “There are uncertainties about all aspects of this outbreak, including the virulence, transmissibility, and origin of the virus, and this in turn results in uncertainty in judging the pandemic potential of the virus and when reactive public health responses, such as recommendations to stay at home or to close schools, should be implemented in individual countries” (Fraser et al. 2009, 1557). Uncertainty is mentioned no less than five times throughout the text of the article, but still voices a confidence that “uncertainty should diminish rapidly in coming weeks as more data on severe cases in the United States and other countries becomes available” (Fraser et al. 2009, 1560).

By the end of May, two months after the beginnings of the pandemic, the statements about the unpredictability of the virus by and among scientists were already legion. *Science* reported that data on the virus remained “fuzzy” and quoted a prominent epidemiologist saying that: “There’s nothing more predictable about flu than its unpredictability” (Cohen 2009a, 997). In the same article, renowned virologist Robert Webster argued that: “You can’t lay down rules for flu viruses – they’ll break them every time. It’s almost as though the virus reads them and says, ‘I’ll do the damn opposite’” (Cohen 2009a, 996). As Ann Schuchat of the CDC stated: “We’re at early days in understanding this virus. [...] It is early days, and with influenza, we always want to be humble and know that things can change and it can be unpredictable” (Silberner/Greenfieldboyce 2009).

A little less than a year later, by late February 2010, the public consensus seemed to be that the pandemic was all-but over. Infection rates were low and a so-called second wave had never really

materialized. Hundreds of thousands of vaccines the world over were left unused. But even so, uncertainty regarding the virus and the H1N1 outbreak not only lingered in the scientific realm, it seemed to be actively promoted. Reporting on a news teleconference, a Health Day article quoted several top epidemiologists as warning against a too-easy “dismissal” of H1N1, or having a “false sense of security.” A professor of public health argued during the conference that: “The flu is very hard to predict and what you think you know is only what happened before. There can always be a surprise” (Gardner 2010). *Science* called H1N1 the “virus of the year” and suggested that it would “go down in history more for causing confusion than catastrophe” (Enserink/Cohen 2009, 1607). And Carl Zimmer, a prominent science writer, wrote in his blog for *Discover Magazine* that the flu strain was “nothing if not surprising,” both in the form of its emergence and the fact that by February 2010 – the middle of the traditional flu season in the northern hemisphere – H1N1 had “dwindled away to very low levels and stayed there” (2010). In other words, the virus was unpredictable not only for its makeup and its severity, but for the pattern of its spread and disappearance. Zimmer argued that the virus “continues to move enigmatically ahead of our understanding” (2010).

Of course, scientists and public health experts are not only accustomed to coping with the various difficulties in dealing with uncertainty, but well-versed in the more overt strategic and political uses of uncertainty as a device for the retention of authority. In an article on uncertainty published in the *American Journal of Public Health* in 2005, the co-authors working in public health stressed that: “In our current regulatory system, debate over science has become a substitute for debate over policy” (Michaels/Monforton 2005, 45). The focus of the article is the use of uncertainty by defendants in environmental health lawsuits or public hearings, but the issues discussed in relationship to the environmental arena can also shed light on similar types of arguments and debates regarding infectious disease (vaccine debates and the charge of undue influence within the WHO as pertinent examples). The authors acknowledge that while much of public health policy is grounded in uncertainty, public health practitioners must recognize that fact while still using the “best evidence available” for their decision-making.

Responding to the charge [4] that the WHO exaggerated the threat from the H1N1 virus, the WHO writes that: “[...] influenza viruses are unstable and can undergo rapid and significant mutations, making it difficult to predict whether the moderate impact would be sustained. This uncertainty, which persuaded WHO and many national health authorities to err on the side of caution, was further enforced by the behavior of past pandemics, which varied in their severity during first and second

[4] On June 3, 2010, the British Medical Journal published a feature article on alleged conflicts of interest within the WHO during the 2009 H1N1 response. In the article, BMJ features editor Deborah Cohen and investigative journalist Philip Carter suggested that the WHO’s reputation had been damaged by their lack of transparency and reluctance to publicly disclose the names of the key scientific advisors on influenza during the pandemic response. Some of these scientists have been shown to have connections with or to have taken payments from pharmaceutical companies responsible for manufacturing not only influenza vaccines, but drugs used in the mitigation of flu (such as Relenza). The quote used below was part of the WHO’s official response to the BMJ article published on June 10.



waves of international spread” (WHO 2010). In the response to its critics, the WHO discusses its evidence and data, but openly discusses the underlying biological uncertainty of the virus itself. This adept rhetorical move distances the organization from the source of the uncertainty, instead locating it within the realm of nature or biology. More research on the virus will thereby be required in order to better understand the severity of influenza outbreaks in the future. The scientific authority of the WHO is thus kept intact, even in the face of a sustained uncertainty.

In part, these “strategic” deployments of uncertainty work because the uncertainty is often displaced onto “nature” or on “society” (Shackley/Wynne 1996) – entities such as the virus itself or the general public – both perceived as inherently out of the control of the laboratory or field epidemiologist. Trevor Pinch’s seminal work on certainty in solar neutrino science (1981) showed how scientists often pointed to other disciplines or fields working on the same problem as the source of uncertainty. The scientists’ confidence, or certainty, in their own work or discipline remained unshaken under this formulation. In the case of virologists, epidemiologists and other public health experts during the 2009 H1N1 pandemic, uncertainty was primarily displaced upon the virus itself, with the virus being cast as biologically unpredictable. This unpredictability works, however, because unpredictability in the case of influenza is ultimately predictable. Thus, the creation of certainty *about* uncertainty becomes an effective method of retaining scientific authority during the pandemic. In the next section, I will explore how uncertainty concerning the virus itself expanded out into conversations regarding the overall ambiguity of the present situation, risk and the process of decision-making during an outbreak of infectious disease.

## **Expanding uncertainty: “information gaps,” risk, prediction and expert knowledge**

Much of the language used in the section above by public health professionals to describe the influenza virus during press interviews focused on terms such as “uncertainty” and “unpredictability,” but a more generic uncertainty was also revealed in relationship to other “information gaps.” Scientists and public health officials often privately grappled with what they viewed as a constantly changing and largely ambiguous situation. In the private meetings or conversations that I observed, public health experts often used phrases such as “we think” or “it seems” rather than “we know” or “it is” to reflect their own doubts about the type and quality of the information they had access to or were deriving from the various graphs, tables, charts, maps and case counts that were in circulation throughout the

2009 H1N1 pandemic. Although much of the locus of doubt remained centered on the “biology of the bug,” uncertainty quickly expanded out to include other aspects of the pandemic.

While working within a national public health agency in the fall of 2009, I attended several meetings or teleconferences that pertained to the 2009 H1N1 pandemic. By October, the public health experts that I worked with were feeling the full effects of the “damned if they do, damned if they don’t” paradox within public health (Altman 2009) – the precariousness of either sounding a false alarm or under-reacting in the wake of the discovery of a widely-circulating and novel influenza virus. The key to certainty during a pandemic is accurate information or data – data which epidemiologists everywhere lamented they were lacking, especially during the early weeks of the pandemic. Information was being circulated in a transparent manner. In fact, many public health experts felt that they were “drowning” in data, but that little of it was “actionable” or usable. By using the term “actionable,” public health experts were expressing their frustration that official case counts and other “numbers” being shared did not provide any clarity on the overall situation during the pandemic. At stake was the ability to predict the immediate future and issue recommendations for action.

In interviews with public health experts during the latter stages of the 2009 H1N1 pandemic [5], I often brought up the topic of uncertainty in relationship to information gaps and risk in order to understand – in more specificity – what public health experts meant when they utilized the term. These conversations often shed much-needed light on how uncertainty was deployed, both in a general sense and in the bounded realm of influenza research and prevention. I discovered that there was a disparity between what people working in public health meant by the usage of the term and how uncertainty was perceived in the popular media or the general public. The tension between understanding uncertainty and the ability to make predictions during an outbreak was often highlighted. During discussions about uncertainty, public health experts frequently described what they saw as essential to understanding the unpredictability of an outbreak of influenza. These conversations did not necessarily center around the unpredictability of the virus – although that never really disappeared as a concern – but around the comprehension of risk vis-à-vis the inbuilt unpredictability of an influenza pandemic. In essence, the public health experts I spoke with told me time and time again that there would never be “certainty” during an outbreak of influenza, no matter how much they knew about the virus or the current situation.

The following excerpt from one of my interviews reveals the underlying “problem” with using objective data to make predictions during a pandemic:

[5] Throughout this essay, I have changed the names and locations of all informants in order to keep their identities concealed. This set of interviews occurred both inside and outside of the United States, in what many in global public health consider to be highly-competent local and national public health agencies. Influenza science is in actuality quite a small world, so to speak, and confidentiality requirements have forced me to be rather vague about the locations in which I conducted my ethnographic research. However, it is worth noting here that it is not my intention to reify the idea of “global” public health, but rather to unpack the object “global” public health as it was understood by those with whom I worked. The narratives and practices surrounding daily influenza surveillance, prevention, and response activities at specific research sites were part of the overall production of what aggregated into my object of study here – or the narratives regarding “uncertainty” within Global Public Health.

*TM:* I'm not sure I understand uncertainty. And I don't think I understand probability and risk.

*Michael:* Well, even scientists really don't understand risk. [laughter]

*TM:* Statistics are a hard thing . . . I mean, intellectually, they are easy to understand, but they are not an easy thing to apply.

*Michael:* That's right, that's right. And uncertainty is the real big one, because, you know, whenever you see the media reporting numbers, it's just 'numbers as truth.' But actually there's always a lot of uncertainty about what numbers really mean. When they go up and down, people would like to have a lot of interpretation about *why* they go up or down. But quite often, it can be random variation."

What becomes important here is the *understanding* of the "numbers" or various epidemiological data as it relates to uncertainty, risk and the ability of public health professionals to predict the immediate future during a pandemic. Numbers here are not as "objective" as one might first conjecture, despite the fact that they are the lingua franca of epidemiological science. If these numbers ultimately form the basis for many of the decisions being made during a pandemic, then what does it mean when the public health experts themselves admit that the data is itself imbued with a certain amount of uncertainty? Uncertainty here is pre-packaged in; it adheres to the data.

An internationally recognized scientist cautioned me about the dangers of using such information to make predictions about how a pandemic might unfold. As Professor Sam Jones explained to me:

"You can look into the past, but you can't look at the future. To make a prediction about the future, you've got to get the virus, put it into a ferret or some other animal model, see if it kills them, look at how many [...] look at mortality and what virulence and what transmissibility and *then* you can make some sort of prediction."

Again, uncertainty about the course of a pandemic is rhetorically tied to the actions of the virus itself. The virus here needs to be observed directly in order to know something about how it works. The past only provides a guide for what may happen during the present, but can never predict the future. Everything here is about *comparison* – either with the past or with other locations during the same time period. Without comparison, there can be no sense-making in the present tense. A chronic lack of comparative data –just think of the debate over the number of fatalities compared with the total number of cases, or the "denominator debate" – often leads to confusion about the immediate future

and a continuation of uncertainty. As one top public health official currently working in Asia explained it to me:

“We always talk about objective evidence and objective data. In the real world, they don’t come in handy. There’s always going to be important data gaps, knowledge gaps, even interpretation gaps. So it’s never a perfect situation in which to make decisions.”

In a real sense, then, what this last quote uncovers is the construction of a type of *sustained* uncertainty within public health in relationship to infectious disease outbreaks. No matter how much data (quantity) or how “objective” the data (quality), there will always be a “subjective” (interpretation) gap that leads to uncertainty during an outbreak. When I asked if this type of uncertainty would be repeated – ad infinitum – into the future, the official responded that it *certainly* would. Thus, information not only about the influenza virus, but other epidemiological data produced during an outbreak, simply feeds back into the uncertainty loop.

In response to the criticisms from the British Medical Journal in June 2010, the WHO rejected wholesale the idea that the pandemic had been “hyped” in collusion with vaccine manufacturers. In the briefing note released on June 10, the WHO reiterated the evidence-based claim that severity of an influenza outbreak is variable – and can change in regards to time, place and population. At first glance, the briefing looks like a typical case of post-hoc fact formation, with the WHO presenting documentation to bolster its case. Looking more carefully, however, one can see evidence of strategic uncertainty being expertly deployed. Severity is difficult to pin down because it requires a case-by-case *interpretation* of the data. It is the formulation of uncertainty as part of the permanent process of public health that interests me. How has uncertainty become one of the key components of global public health’s rationale for its response to the 2009 H1N1 pandemic? More importantly, what does this collective turn toward or partial embracing of uncertainty signal? In the next section, I will begin to answer these questions by exploring how uncertainty is deployed as a strategic tool to retain scientific authority.

## Strategic uncertainty and the maintenance of scientific authority during a pandemic

By the end of 2009, little “uncertainty” was still being expressed – either publicly or privately – concerning the duration, severity or overall course of the H1N1 pandemic. The 2009 H1N1 pandemic

had, by all accounts, turned out to be similar in severity to that of a “normal” or “mild” flu season. Facts were known; a collective sense of scientific “certainty” regarding certain aspects of the pandemic – about the biological makeup of the virus, information concerning severity and its potential duration, and the immediate risk it posed to society – had all-but resumed. Many of the scientists and epidemiologists that I interviewed as late as May 2010, however, expressed a continued uncertainty relating to the H1N1 virus itself. From a virology standpoint, some public health experts worried openly that there might be an antigenic shift or a recombination event that could transform the H1N1 virus into something more ominous. In conversations throughout the latter stages of the pandemic, public health experts consistently used this uncertainty – the predicable unpredictability of the influenza virus – to support not only their past and future decisions, but their present actions as well. In what follows, I will use the U.S. CDC and the WHO’s deployment of uncertainty about the H1N1 virus during different phases of the pandemic to suggest that a new type of *strategic uncertainty* was being used within global public health as an effective rhetorical tool to retain scientific authority *during* this infectious disease event.

From the very beginning of the pandemic in April, CDC officials began to communicate uncertainty about the situation (see first section above). The then-Acting Director of the CDC, Richard Besser, stated that the agency’s overall objective during the event was to “tell everything we knew, everything we didn’t know and what we were doing to get the answers” (Maher 2009, 152). In an article on the crisis communication style of Richard Besser, the journal *Nature* praised Besser’s management of the situation, noting how Besser’s overt use of uncertainty helped to shape the tenor of the entire U.S. response. The article quotes several prominent members of the international public health community as attesting to Besser’s overall skill in “communicating uncertainty” (Maher 2009). Even noted expert on the 1976 influenza pandemic, Harvey Fineberg, argued that the CDC’s communication of uncertainty during the pandemic under Besser was exemplary (Maher 2009). Although the *Nature* article also argues that Besser had miscalculated the “political ramifications” (Maher 2009, 152) of the CDC’s more aggressive early actions (such as recommendations on school closures), the fact that Besser himself was able to parlay his communication of uncertainty into several lucrative job offers should be seen as objective evidence that his strategic use of uncertainty was effective. In his current job as the health analyst for *Good Morning America* on ABC, Besser “still projects uncertainty” (Maher 2009, 152).

My own interviews with public health experts outside of the United States support this view of the CDC’s handling of the pandemic. The CDC was rarely overtly criticized. Instead, the CDC’s strategy of

“saying what you don’t know” had been actively replicated in other locations. Public relations experts have actively coached public health experts in the art of crisis communication, advocating honesty and transparency over the projection of absolute authority. In a private conversation about the focus on uncertainty throughout the pandemic, scholar and former journalist Thomas Abraham suggested to me that the CDC – as the reigning “gold standard” of epidemiological science with a global reputation to match – had utilized the concept of uncertainty more often, and with more impunity, than other national or international health agencies had dared. It is interesting to note here, then, that the CDC has not come under the same scrutiny or criticisms as the WHO for its response to the pandemic.

In June 2010, the British Medical Journal published an investigative article that suggested the WHO’s lack of transparency in its decision-making process and its cadre of experts’ various links to pharmaceutical companies had led to various “conspiracy theories” about the WHO’s handling of the 2009 H1N1 pandemic (Cohen/Carter 2010). Also at stake was the WHO’s decision in May of 2009 to change its definition of a pandemic, striking a key phrase that had described a pandemic as an outbreak causing an “enormous” number of deaths. The authors of the article blamed, in part, the WHO’s poor communication of risk, quoting one expert in risk communication as stating that: “The problem is not so much that communicating uncertainty is difficult, but that uncertainty was not communicated” (Cohen/Carter 2010). Responding to criticisms that the WHO “overreacted” and “inflated risk” during the early weeks of the outbreak, the United Nations’ influenza expert Keiji Fukuda argued that the pandemic was not over yet, and that the risk was “real” (United Nations News Service 2010).

Uncertainty during an infectious disease outbreak is by its very nature undisciplined and anxiety-provoking. Uncertainty is not easily managed, either within the confines of a laboratory dealing with the virus or in the world-at-large coping with an outbreak. All of the various scientific and epidemiological graphs, tables, maps and lists of numbers showing lab-confirmed H1N1 cases that were produced throughout the pandemic to track the peaks and valleys of the flu season were partial attempts by public health experts to alleviate some of the uncertainty surrounding the influenza virus itself. This creation and circulation of knowledge about the immediate or distant future – or “anticipatory knowledge” – is an attempt to wield authority over uncertainty, to make the unpredictable more predictable, to “project” competence and power, to create order out of potential disorder (Nelson/Geltzer/Hilgartner 2008). As scholars of the 2009 H1N1 pandemic have pointed out elsewhere, both politicians and public health officials have opted for two rhetorical moves, often in the

same sentence, that functioned to sound an alarm and to reassure the public about epidemic events (Nichter/Briggs 2009, 191). In practice, the scientists must walk the fine line between under and overstating uncertainties in relationship to a politically-charged issue (Shackley/Wynne 1996, 278). Reports on the 2009 H1N1 pandemic constituted metapragmatic accounts (Nichter/Briggs 2009) – or accounts of the accounts – of how epidemiologists, clinicians, and others produced and circulated knowledge. Looking critically, then, at the narratives around the uncertainty of influenza, we can see that a certain type of “anticipatory uncertainty” is being deployed. Wald has argued that “the epidemiological narrative is, like the microscope, a technology” (2008, 19). The construction of sustained uncertainty – both now and in the immediate future – provides scientists with a certain flexibility, a maneuverable bracketing of the future that is used to help control the present moment, a narrative tool for both gaining and retaining scientific authority during an outbreak of infectious disease. What cannot be known *now* can be further researched, it can be known *later*. In this deft move, a certain amount of biological uncertainty does not trouble scientific authority, but helps to further generate it.

In an article looking at uncertainty in relationship to climate science and environmental policy, Shackley and Wynne suggested that uncertainty has its uses, especially for scientists; uncertainty acts as an “alibi,” a way to support further research funding, and as a hedge against the “encroachment” of policymakers into their realm of expertise (Shackley/Wynne 1996, 277). Uncertainty is negotiated in the semi-public interactions between scientists, policymakers, and politicians (Shackley/Wynne 1996, 277). Brian Campbell has argued the very existence of uncertainty is evidence of “continual interpretation and negotiation” (1985, 430), and that scientists who are asked to perform the role of expert in public hearings commonly “state that there is uncertainty, and that this type of argument can be managed and accepted as authoritative” (Campbell 1985, 431). Campbell argues that this “maneuvering in relation to uncertainty demonstrates a *strategic* importance of the issue of uncertainty to expert arguments” (1985, 445). I take his use of strategic seriously, as well as his suggestion that the strategic use of uncertainty reveals the politics inherent in policy science. For Campbell, uncertainty is not the cause of policy debates, but the result of such arguments (1985, 447). Uncertainty is a *flexible tool* that aids in negotiation of authority. The 2009 H1N1 pandemic might be seen as a “boundary-ordering device” (Shackley/Wynne 1996, 280), where uncertainty helps to redefine the authority of both scientists and epidemiologists. In essence, the strategic use of uncertainty allows the construction of a type of

“certainty *about* uncertainty” (Shackley/Wynne 1996, 281). In turn, the policymakers can use uncertainty in a strategic way to “deflect unwelcome attention and criticism of the policy process” (Shackley/Wynne 1996, 283). All of this does nothing to undermine the authority of science. Indeed, the strategic use of uncertainty strengthens that authority. Science is once again seen as the only method to close a critical “information gap”, and the authority of the current scientific paradigm further strengthens the reigning “policy order” (Shackley/Wynne 1996, 287).

Claiming that there is uncertainty is in no way an admittance that the scientist is in no position to judge – quite the contrary (Campbell 1985, 449). In fact, the strategic deployment of uncertainty guarantees that the scientific authority will be maintained, casting the scientist/epidemiologist as the only person qualified to judge an uncertain situation. They know *better*, if they do not know *all*. They have the tools to know further, to gather more information. In essence, if uncertainty somehow necessitates a return to certainty, then the strategic use of uncertainty ensures that science will be the discipline asked to shepherd us back to more solid, or certain, ground. But as Campbell points out, the “problem” of uncertainty cannot be dealt with quantitatively; it is a “social” problem (1985, 450). It was the rhetorical trick of deploying uncertainty during the 2009 H1N1 pandemic that has so deftly maintained the need for more qualitative data to interpret the pandemic.

STS scholar Susan Leigh Star has studied the ways in which “local uncertainties” are transformed into “global certainty”, or facts (1985). In Star’s epistemology, belief is a core facet of the ability of working scientists to transform uncertainty into certainty. As Star points out at the beginning of her analysis, “scientists constantly face uncertainty” (1985, 392). This is, of course, no less true thirty years later than it was when Star first began to study uncertainty as a phenomenon. However, Star’s article also reflects the sea change in scientists’ relationship to uncertainty. Star’s work centers on how various types of uncertainty were completely elided from published scientific work through six mechanisms for creating global certainty: attributing certainty to other fields; maintaining that technical failures were to blame, rather than the internal processes of science; the creation of ideal types; shifting evaluation criteria to mask uncertainty; generalizing results in an ad hoc manner; and using internal debates or arguments over *how* to perform research to “subsume” uncertainty about *whether* to perform research (Star 1985, 407–412). All of this “management of uncertainty” in the local setting had to “satisfy local constraints *and* create global certainty” (Star 1985, 413).

In effect, what Star argued in the 1980s was that local uncertainty formed the basis of a global certainty about scientific facts or the value of entire global research paradigms. This was one of the



reasons why scientific theories about the world could persist well into the future. The transformation of uncertainty into certainty was the most efficient tool for sustaining a scientific paradigm indefinitely. In 2010, however, the meaning of uncertainty itself has begun to shift. Uncertainty is no longer the “dirty secret” of science. To reflect this, I want to take Star’s old argument and flip it to argue that *sustained uncertainty* is now what ultimately holds the global influenza research paradigm together. Strategic uncertainty does not necessarily need to be transformed into certainty in order for it to form the basis of a robust research paradigm. The CDC and WHO public responses to the 2009 H1N1 pandemic are examples of how effective the deployment of strategic uncertainty can be for the retention of authority during an outbreak of infectious disease.

## **Conclusion: strategic uncertainty and the creation of knowledge in global public health**

*Uncertainty is the only certainty there is, and knowing how to live with insecurity is the only security.*

John Allen Paulos, *A Mathematician Plays the Stock Market*

As medical anthropologists and observers of global public health, we are often no strangers to the deployment of strategic uncertainty ourselves. In recent editorials on the 2009 H1N1 pandemic, anthropologists have effectively argued that what biological and epidemiological approaches to infectious diseases lack is a social or cultural component (see Atlani-Duault/Kendall 2010; Singer 2010). These prominent scholars are not so much critiquing influenza science or global health response *per se*, but rather suggesting that their own area of expertise should be more efficiently utilized in order to fill up any critical gaps in data about how different socioeconomic groups or cultures cope with pandemics and public health measures. They are arguing for inclusion in the larger scientific paradigm based on their own social scientific authority, deploying the concept of uncertainty to strengthen the case for their own discipline’s analysis of pandemics. Anthropology here is conceptualized as another effective tool for dealing with present and future uncertainty.

This essay has been, in part, an attempt to ask a new kind of question about certainty and uncertainty within global public health. Can we be “certain” about “uncertainty”? How might uncertainty be sustained and utilized in relationship to the maintenance of scientific authority? Is this a new form of uncertainty or simply a new and more robust use of it? And, perhaps most importantly,

how is the fuzzy line between biological “certainty” and “uncertainty” continuously renegotiated and/or maintained by the various scientists, epidemiologists, and other public health professionals working within public health?

Building out from the German philosopher Ludwig Wittgenstein’s last statements on certainty, the concepts of certainty and of knowledge are not all that different (1969, 3e). Under Wittgenstein’s formulation, certainty occurs the moment when someone “declares how things are” (1969, 6e). During the recent 2009 H1N1 influenza pandemic, public health experts declared vociferously and repeatedly that the situation was somehow fundamentally, naturally, biologically uncertain. In this essay, I have attempted to examine how the meanings of words like uncertainty have shifted, how other concepts have changed along with them (Wittgenstein 1969, 10e), and how they might then be used to craft a new type of epidemic order. If we take seriously Wittgenstein’s postulation that “a meaning of a word is a kind of employment of it” (1969, 10e), then we must begin to further examine how the scientists and epidemiologists working in global public health utilize the term uncertainty in daily practice: what it might signify when it is used casually in relationship to ongoing scientific work and attempts to gather epidemiological data; what it might signify when it is deployed within the public sphere; and, finally, how it is might be utilized strategically vis-à-vis scientific authority. This is not to argue, however, that present-day scientific authority rests solely upon the maintenance of uncertainty. Now, as ever, scientific expertise is firmly located in the ability to produce facts, or certainty, about the world in which we live. My goal in this short space has been to point out how a new configuration of scientific authority within global public health straddles the ever-tenuous line between certainty and uncertainty, and to examine how biological uncertainty was deployed at key moments during an infectious disease outbreak to bolster that authority. As Wittgenstein pointed out before his death, one cannot begin to doubt without being certain, without first believing a set of propositions to be true. In other words, and to pack Wittgenstein’s propositions back out into the realm of public health, one cannot have biological uncertainty about a particular virus without first having created a baseline of scientific knowledge about an entire class of influenza viruses.

In this essay, I have argued that the creation of a sustained uncertainty regarding the biological properties and characteristics of the H1N1 virus and its strategic deployment merely presupposes the need for the creation of further biological knowledge about the virus. This is how the trick works, and why the admission of uncertainty is no hindrance to the retention of authority in science or in global public health. The new epidemic order shows us that we cannot produce knowledge without uncertainty.

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## Unequal 'Partners'.

### AIDS, Academia, and the Rise of Global Health

Johanna T. Crane

#### Abstract:

The last decade has seen the proliferation of “global health” departments, centers, programs, and majors across top research universities in North America and Europe. This trend has been particularly pronounced in the United States, where it is connected to America’s new role as a major sponsor of HIV treatment in Africa. This paper describes the rise of “global health” as a research, funding, and training priority within U.S. academic medicine, and the increasing desirability of “global health partnerships” with institutions in sub-Saharan Africa. Leading spokespersons emphasize that “partnership” with poor nations is central to the mission of global health, an ethic that distinguishes it from older, more paternalistic traditions of international health and tropical medicine. However, at the same time, the field of academic global health depends on steep inequalities for its very existence, as it is the opportunity to work in impoverished, low-tech settings with high disease burdens that draws North American researchers and clinicians to global health programs and ensures their continued funding. This paradox – in which inequality is both a form of suffering to be redressed and a professional, knowledge-generating, opportunity to be exploited – makes the partnerships to which global health aspires particularly challenging.

**Keywords:** global health; Africa; partnership; AIDS; inequality

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## Introduction: Epidemics and Opportunities

Nearly 30 years ago, the U.S. Centers for Disease Control published a brief but unsettling account of a mysterious outbreak of an unusual pneumonia, *Pneumocystis carinii*, in several otherwise healthy young gay men (CDC 1981). In retrospect, the disease that would later come to be called AIDS can be seen as the first in a series of “emerging infections” that would spring into public and scientific awareness over the next several decades. The appearance of HIV and AIDS, along with the Ebola virus, BSE or “mad cow disease,” SARS, MDR- and XDR-TB, H<sub>5</sub>N<sub>1</sub> (“bird flu”), H<sub>1</sub>N<sub>1</sub> (“swine flu”), and MRSA [1] led some to declare that infectious disease – supposedly vanquished in the post-World War II era of antibiotics – had in fact “returned” with a vengeance (Garrett 1996; Fauci 2001). More often than not, these threats were framed as emerging from poor countries in the global South to endanger wealthier, whiter populations in the global North (Anderson 2000; Farmer 2001; Patton 2002).

Much has been written about this “emerging infectious diseases worldview” in the medical social sciences (King 2002). Often, this literature has focused on the sense of crisis that surrounds these new and renewed epidemics and the development of biopolitical enterprises designed to manage “risky” and “at-risk” bodies and populations in an increasingly globalized and mobile world (Bourgeois 2000; Collier/Lakoff 2008; Nguyen 2005a). The purpose of this paper is somewhat different, and seeks to bring a critical eye to a different and perhaps under-theorized consequence of the AIDS epidemic and the “return” of infectious disease. I speak here of the meteoric rise of “global health” as a field of practice and an academic discipline. In U.S. universities in particular, the number of programs, centers, institutes, and departments devoted to global health has skyrocketed in the last decade. Unlike the emerging infectious diseases worldview, which operates primarily within a register of fear (of disease) and protection (from disease), academic global health approaches AIDS and other infectious diseases as an *opportunity* for learning and for the production of scientific knowledge. In this scenario, rather than (or often in addition to) being quarantined, cordoned, or subject to other forms of biopolitical control, nations and regions suffering from a high burden of infectious disease are enlisted as “partners” in an educational endeavor. In this way, untreated epidemics are simultaneously envisioned as a socio-medical ill *and* instrumentalized as a scientific asset by American universities seeking to engage in “global health” activities.

Using ethnographic data collected at the meetings of the Consortium of Universities for Global Health (CUGH) and within a U.S.-Uganda research collaboration, this article represents an initial effort to historicize the current popularity of “global health” in the American academy and to think

[1] Human Immunodeficiency Virus (HIV), Acquired Immune Deficiency Syndrome (AIDS), Bovine Spongiform Encephalopathy (BSE), Severe Acute Respiratory Syndrome (SARS), Multi-drug Resistant and Extensively Drug Resistant Tuberculosis (MDR- and XDR-TB), and Methicillin-Resistant *Staphylococcus aureus* (MRSA).

critically about the new kinds of relationships that are being established between the global North and South in the name of global health. In particular, I am interested in the ways in which global health is forging new kinds of ties between the United States and sub-Saharan Africa.

## Global Health and the New Scramble for Africa

As of 2008, nearly half of U.S. medical schools and their affiliated institutions included “initiatives, institutes, centers, or offices” dedicated to “global health” (Crump/Sugarman 2008). This is a new phenomenon. Global health courses, majors, and minors have become increasingly “hot” within undergraduate programs (Brown 2008), and in response to student and faculty demand for global health opportunities, universities are both founding new departments and changing the names of existing programs to ally themselves with this emerging field. **[2]** A 2009 survey showed that total of forty-one universities in North America (mostly in the U.S.) have created “pan-university institutes, centers, and the like” devoted to global health, and that an additional eleven schools have established global health programs within existing departments or divisions (Merson/Page 2009, 3). Furthermore, many schools house international learning or research endeavors which, while clearly of a “global health” nature, are not officially administered by the university’s office or department of global health. Notably, the growth of interest in global health is significant enough that non-academic entities are seeking to capitalize on it: for example, Seattle’s Chamber of Commerce recently launched an organization called the Washington Global Health Alliance in an attempt to harness the city’s sizable global health activity – some have called it an ‘industry’ – for local economic development (Paulson 2008; Heim 2010). Similarly, a recent conference in Boston touted “New England’s Strategic Advantage” in the field of global health, pointing to the region’s high concentration of research institutions and bioscience companies. **[3]** Thus, in the U.S., “global health” is emerging as a powerful force for mobilizing resources and action both within and outside the academy.

Academic global health programs encompass a wide variety of activities and include a broad range of participants, from first-year undergraduates to internationally-known research scientists. For example, activities housed under the umbrella of “global health” may include overseas service learning programs for college students, international clinical rotations for medical students and medical residents, and/or medical and public health research conducted in the global South, often in collaboration with foreign colleagues. What ties these varied activities together is their operation in what the medical literature refers to as “resource-poor” or “resource-limited” settings. Specifically, the

**[2]** For example, the University of Washington established a new Department of Global Health in 2007 ([http://globalhealth.washington.edu/about\\_us/chairmsg.php](http://globalhealth.washington.edu/about_us/chairmsg.php)), and Harvard changed the name of its Department of Social Medicine to the Department of Global Health and Social Medicine in 2008 (<http://ghsm.hms.harvard.edu/about/history/>).

**[3]** See <http://harvardscience.harvard.edu/culture-society/articles/panel-examines-new-england-s-contributions-role-global-health>.



existence and success of such programs depends upon the ability of U.S. universities to establish ties with clinics, teaching hospitals, and universities in the global South willing to serve as hosts for American students, medical residents, and research faculty wishing to “do” global health. Countries in eastern and southern Africa (particularly Uganda, Tanzania, Kenya, and Botswana) have become some of the most popular locations for U.S. academic global health programs in search of host institutions, as they offer relative political stability as well as an English-speaking elite due to their status as former British colonies (American Universities’ Engagement in Global Health 2009). [4]

In the course of my research, more than one American HIV researcher has described the rapidity of this expansion to me with some concern. As U.S. research universities rush to establish partnerships that can give their students and faculty opportunities to work in “resource-poor” African settings, some have expressed worry that the juggernaut of global health is engendering a 21<sup>st</sup>-century academic “scramble for Africa” (see also Nguyen 2009, 206). Perhaps in response to these postcolonial anxieties, the term “partnership” has emerged as a key word within this new arena or “social world” of global health [5] (Clark/Star 2003). Host institutions in Africa and elsewhere in the global South are described as “partners,” and Northern global health leaders cite “real” or “true” partnership with poor countries as a key factor distinguishing global health from its predecessor fields of international health and tropical medicine, which are seen as having operated in a more top-down, paternalistic mode (Koplan 2009). In contrast, this paper starts from the premise that “global health” – at least in the academic context described here – is primarily a North American concept, and aims to explore some of the discourses and practices that make it so (see also MacFarlane et al. 2008; Holm/Malete 2010).

## ‘Global Health’ as an ethnographic object

In my analysis, I draw primarily upon my experiences as a participant-observer at meetings of a nascent group called the Consortium of Universities for Global Health (CUGH). In addition, I supplement these observations with data collected during my fieldwork within a university-based, U.S.-Uganda HIV research partnership. The CUGH was founded in 2008 for the purposes of giving U.S. and Canadian universities active in global health a place to share ideas and experiences, and to shape the future of education and research in the field. In order to become a full member of the organization, a university must house a multidisciplinary global health program, pay \$3500 in annual dues, and “have at least one substantive, current, long-term relationship with an international partner university in a low- or middle-income country” (Consortium of Universities for Global Health 2010). (Universities in low-income

[4] Examples include the University of California San Francisco’s program at Muhimbili University in Tanzania, the University of Pennsylvania-Botswana Partnership in Gabarone, Harvard’s research program at Uganda’s Mbarara University of Science and Technology, Cornell University’s relationship with Kilimanjaro Christian Medical College in Moshi, Tanzania, and Weill Cornell Medical School’s partnership with Bugando University College of Health Sciences in Mwanza, Tanzania – recently renamed Weill-Bugando University of Health Sciences. In addition, there are a few notable examples of long-standing partnerships that predate the current popularity of “global health.” These include the Johns Hopkins/Makerere University (Uganda) partnership and the Indiana University/Moi University (Kenya) partnership, both established in the late 1980s.

[5] See recent dissertations by Rene Gerrets and Elise Carpenter for ethnographic accounts of a malaria partnership in Tanzania (Gerrets 2010a) and an HIV/AIDS partnership in Botswana (Carpenter 2008).

countries that have existing partnerships with CUGH universities are able to join for free.) The organization's inaugural meeting was held in 2008 in San Francisco, and 50 representatives from 20 universities were invited to attend. The group's first annual meeting, held one year later in 2009, took place on the campus of the U.S. National Institutes of Health and was much larger, with over 250 attendees from more than 50 universities participating. Recently, over 850 people attended the 2010 meeting at the University of Washington in Seattle.

Why study an organization like the CUGH? In a now-classic essay on the importance of "studying up," Laura Nader urged anthropologists to turn their ethnographic attention not just to the poor and underprivileged, but to institutions of wealth and power. "Anthropologists," she argued, "have a great deal to contribute to the processes whereby power and responsibility are exercised in the United States" (Nader 1972, 284). The state of anthropology is different now than when Nader first published this piece in 1969, and the subject of power and its exercise is now a major focus of ethnographic studies both of the U.S. and elsewhere. Nonetheless, her intervention remains relevant, and provides a useful perspective from which to approach "global health" as an ethnographic object.

Although anthropologists have made important contributions to the analysis of postcolonial power relations within global health projects, especially in the field of HIV/AIDS (see, for example, Pigg 2001; Pfeiffer 2004; Nguyen 2005a,b; Farmer 1999), the ethnographic lens has not usually focused on the field's power brokers. Yet, it is these networks of experts or "epistemic communities" that have the power to "set agendas, frame issues, identify problems, and propose solutions" within global health (Janes/Corbett 2009, 174). The CUGH brings together some of the most influential individuals and institutions in academic medicine today. Its meetings are populated by prominent and powerful researchers from the most prestigious universities in North America, as well as by a very select group of elite researchers from low-income countries. This high level of symbolic capital gives the CUGH considerable power over the shape and priorities of global health as a field, and makes it a particularly valuable venue in which to "study up." In doing so I follow not only Laura Nader's directive, but the urgings of James Pfeiffer, Mark Nichter, and the Critical Anthropology of Global Health special interest group, who recently argued that medical anthropologists can make a valuable contribution to redressing inequality by "illuminating the social processes, power relations, development culture, and discourses that drive the global health enterprise" (Pfeiffer/Nichter 2008, 413).

Where analysis of the CUGH meetings provides an opportunity to interrogate the discursive field of North American global health leaders, it does not afford the chance to observe how "partnership"

between American and African actors and institutions is enacted in practice. For this reason, I include select accounts from my fieldwork within a university-based U.S.-Ugandan HIV research collaboration as a supplement to my discussion of CUGH. The formal aspects of this fieldwork were conducted over 12 months in the U.S. (2004-2005) and two months in Uganda (2005, 2009), though as a former employee of the project, my ties to both the American and Ugandan sides of the research group extend beyond these periods.

## PEPFAR and the Rise of Global Health

“Global health” is often described as having emerged out of the older fields of tropical medicine and international health, though the question of whether it is truly distinctive is debated, even among those who describe themselves as within the field (Bunyavanich/Walkup 2001; Brown et al. 2006; Macfarlane et al. 2008). The phrase became increasingly visible in the 1990s, spurred in part by the WHO’s efforts to “refashion itself as a coordinator, strategic planner, and leader of ‘global health’ initiatives” in an attempt reclaim some of the power and visibility it had lost to the World Bank’s growing international health programs during the 1980s and 1990s (Brown et al. 2006, 69). Notably, as a term, “global health” appears most commonly in North America. For example, a 2008 search of the PubMed medical literature database found that 87% of articles by authors with affiliations with university global health programs were North American (Macfarlane et al. 2008, 389).

Recent anthropological and historical literature has focused on the interpenetration of global health with national security concerns in the late 20<sup>th</sup> and early 21<sup>st</sup> centuries, particularly within the U.S. (Lakoff/Collier 2008). For example, Nicholas King describes the rise of the “emerging infectious diseases worldview” in the 1990s, which emphasized the need for rigorous surveillance of global health threats and the integration of postcolonial economies into the global marketplace (King 2002). King and others describe a rising concern that epidemics (particularly AIDS) might further weaken fragile states and create a “nontraditional” national security risk to the U.S. – what Alan Ingram has described as “the securitization of disease” (King 2002; Ingram 2005). However, in a recent address to the Society for Medical Anthropology, Didier Fassin provided a reminder that the formulation of global health as a security issue co-exists with a second “semantic network” in which global health connotes compassion for the poor and a concern with global inequalities. These two frameworks are not necessarily contradictory, he argues, though

they are distinct (Fassin 2009). Furthermore, both frameworks are worthy of problematization; good intentions and compassionate action are not immune to the power imbalances and inequalities they seek to redress.

North American universities pursuing global health activities do so primarily within this second register of compassion, pairing it with a scientific mission in which international research and medical education are valorized as humanitarian endeavors (“saving lives”). Interrogating this invocation of “global health” allows us to see things that are not visible – or at least appear very different – from the security perspective. This is particularly the case with AIDS. From the security perspective, AIDS is emblematic of the emerging infectious diseases worldview, demonstrating the globalization of disease, the porousness of borders, and the threat posed to the American public by new forms of infection. By contrast, from the perspective of academic global health, AIDS seems distinctly rooted in place. Its primary symbolic register in this arena is not global, but rather African. Furthermore, the severity of the AIDS epidemic on parts of the African continent is envisioned not so much as a security threat, but as a scientific and humanitarian opportunity to “do” global health.

The current juggernaut of activity within academic global health has its roots in the African AIDS epidemic and recent U.S. government responses to it. Throughout the 1990s, U.S. doctors and researchers became increasingly aware of and concerned about the scale of the AIDS epidemic in parts of Africa (Fauci 2007), and activist pressure on donor nations to make antiretroviral therapies available in poor countries mounted. In the early 2000s, a major shift occurred with the establishment of the multilateral Global Fund to Fight AIDS, TB, and Malaria (to which the U.S. was the largest donor), and the President’s Emergency Plan for AIDS Relief (or “PEPFAR”), which was initiated and funded entirely by the U.S. government under George W. Bush. Both programs offered substantial funding for the support of free antiretroviral treatment programs in low-income countries. With an initial promise of \$15 billion over 5 years, PEPFAR represented both the “largest ever international public health program,” (Rottenburg 2009, 424) and the largest expenditure the U.S. government had ever made towards a single disease (Jones 2010). It also ushered in an era of unprecedented involvement in African health by the American state and its collaborating institutions.

Significantly, PEPFAR funds travel not only through the U.S. State Department and government agencies such as the U.S. Agency for International Development (USAID) and the Centers for Disease Control (CDC), but also through both public and private U.S. universities. In 2007, three of the top ten PEPFAR grant recipients were American universities engaged in HIV treatment, prevention services,

and vaccine research in 13 different countries, 12 of which were in sub-Saharan Africa. [6] In addition, many other universities work with PEPFAR as “sub-partners” to primary grant recipients (PEPFAR 2010). In this way, the advent of PEPFAR has facilitated the expansion of American academic involvement in public health in Africa by laying some of the institutional groundwork for the establishment of global health partnerships between U.S. and African institutions.

## Defining Global Health

The Consortium of Universities for Global Health (CUGH) is one organization that has emerged in recent years in response to the explosion of enthusiasm for global health on North American campuses. Initially funded by grants from the Gates and Rockefeller Foundations, the consortium currently includes 44 dues-paying member universities in North America and fourteen Southern “partner members” (all but one affiliated via either Johns Hopkins or the University of Washington) (CUGH 2010). Both CUGH membership and meeting participation have expanded rapidly since the group’s initial formation three years ago.

One of the priorities of the CUGH’s inaugural meeting in 2008 was to produce “a common definition of global health.” As numerous science studies scholars have noted, defining the boundaries of what does and does not count as “science” is a powerful act, as it accords legitimacy to certain kinds of knowledge and practice while excluding others (Geiryn 1999; Epstein 1996). Likewise, as “global health” rises in scientific prominence and as a funding priority, the ability to define the field – and thus what lies outside it – becomes a powerful exercise in inclusion and exclusion. The CUGH’s definition was published in 2009 on behalf of the consortium in a widely-cited article in the medical journal the *Lancet* (Koplan et al. 2009). In the published article, the CUGH authors are diligent about distancing global health from the older fields of international health and tropical medicine, which are seen as embodying outdated and paternalistic modes of relating between wealthy and poor nations. As such, they ally their preference for the term “global health” over “international health” to “a shift in philosophy and attitude that emphasizes the mutuality of real partnership, a pooling of expertise and knowledge, and a two-way flow between developed and developing countries” (Koplan et al. 2009, 1994). At the 2008 meeting, the lead author of the *Lancet* article made this point somewhat more bluntly, stating, “global health recognizes that the developed world does not have a monopoly on good ideas.” In this way, North American global health leaders employ the idea of “partnership” to position the field morally by allying it with an ethic of equity that earlier incarnations of transnational health research and practice are seen as lacking.

[6] In 2007, Harvard University received PEPFAR funds for programs in Botswana, Nigeria, Tanzania and Vietnam; Columbia University ran PEPFAR-funded projects in Cameroon, Cote d’Ivoire, Ethiopia, Kenya, Mozambique, Nigeria, Rwanda, South Africa, Tanzania, Uganda, and Zambia; and the University of Maryland received PEPFAR funds for a vaccine research program in Nigeria (Avert 2008).

However, despite this aspiration to partnership, global health is in many ways a creation of the “resource-rich” world. This becomes evident upon comparing the *Lancet* article’s definition of global health to the CUGH conference discussions that surrounded it. The second morning of the 2008 conference included a panel titled “Perspectives from Our Global Health Partners,” which featured the four conference participants who had been invited to represent “partner” institutions in the global South. Of the 50 conference attendees, these were the only scientists not from U.S. or Canadian institutions, a fact that did not go unnoticed by some of the participants. (As one researcher from the Rockefeller Foundation noted in an aside, “If having an international partner is what got us invited to this conference, why weren’t we required to bring our partners?”) The four international panelists were senior academic researchers from Haiti, Mexico, Bangladesh and Uganda. The list of their Northern partner institutions read like a check-list of elite American schools – Harvard, Cornell, Johns Hopkins, Columbia, University of Michigan, and UCSF among others – plus government agencies such as the National Institutes of Health and the U.S. Agency for International Development. But unlike their American colleagues, who had spent most of the previous day in discussions about how to improve global health education opportunities for their undergraduate and medical students, the international panelists expressed uncertainty and sometimes skepticism regarding the term “global health” and what it meant to “do” global health.

For example, Mushtaque Chowdhury, Dean of the School of Public Health at BRAC University in Bangladesh, assured the audience that “what we do in Bangladesh is global health, though we don’t call it global health.” Mario Rodriguez-Lopez from the National Institute of Public Health in Cuernavaca, Mexico – by his own account, the least well-known of the four panelists – recounted a conversation he had had the day before with Jeffrey Koplan, Vice President for Global Health at Emory University and leader of the CUGH’s effort to forge a common definition of global health. Koplan had told him, “what you are doing in Mesoamerica is global health,” to which Rodriguez-Lopez responded, “ah yes, I only just realized it!” Nelson Sewankambo, Principal of Makerere University College of Health Sciences in Kampala, Uganda and one of the first scientists to publish data on AIDS in Africa, was more confrontational. He told the room, “when you see it the way I see it, people are not discussing global health. [...] How do *our* students learn global health? By coming North? By staying home? You need to examine what global health actually means from other countries’ perspectives.” Jean William Pape, an internationally known AIDS researcher from Haiti, echoed these sentiments by arguing in favor of a consortium that was global, rather than North American, in membership, telling the audience, “How

can you talk about collaboration when you are thinking one way and you don't even know how the other side is thinking? Yesterday we heard lots of issues relevant to Northern institutions. A *global* consortium is a great idea. You need to include partners early on."

Overall, the partners' comments seemed to reflect that what North American institutions were calling "global health" was simply public health, or "business as usual," in their countries (MacFarlane et al. 2008, 384). If this is so, Sewankambo's question is a provocative one: how *do* students from "host" countries in the South learn global health? One possible answer is that they travel North, requiring Northern universities to reciprocate their global health training programs by hosting students from Africa, Asia, and Latin America. The dean from BRAC University expressed a desire for such opportunities, but noted that whenever his students tried to travel to the U.S. they had trouble getting their visas approved (at which point, a Canadian researcher yelled out "come to Canada!", eliciting a laugh from the audience). Another possibility is that "global health" actually refers strictly to health care delivery and research in poor countries, which puts residents of these countries in the paradoxical position of needing to remain anchored in place in order to participate in "global" health. This question also arose during the 2009 meeting, when a Latin American member of the CUGH's Education Committee wondered aloud how Southern institutions might initiate global health partnerships, asking "what do you do, look for an even poorer country to work in?"

This tension over the meaning of "global health," and who gets to define it, was acknowledged by CUGH organizers both during the conference itself and in the report of the meeting's proceedings that was later published on the consortium's website. In the report – whose author is unnamed – the assertions that "global health is a Northern concept" and that "for the academic institution in the South, everyday public health, medical and nursing education and practices constitute 'global health'" are made on the first page (CUGH 2008). But, significantly, these important points were not included in the much more widely-read *Lancet* article that followed the conference, titled "Towards a Common Definition of Global Health," even though this article was co-authored by both Northern and Southern consortium members who attended the meeting, including some of the same researchers who had both made and acknowledged the objections described above. Instead, the *Lancet* article avoids any references to the postcolonial power dynamics of global health and speaks mainly in positive terms of its promise, offering up the following as a suggested definition: "global health is an area for study, research, and practice that places a priority on improving health and achieving equity in health for all people worldwide" (Koplan et al. 2009, 1995).

## Bodies of Knowledge

In addition to clinical care and training programs, global health also encompasses a burgeoning transnational research apparatus aimed at studying and addressing health problems in the so-called “developing” world. One beneficial outcome of the fact that global health is currently “fashionable” in academia (Koplan et al. 2009) has been a growing attention to what researchers have dubbed the “10/90 gap”: the critique that only ten percent of global expenditure on health research goes towards studying and fighting the diseases that most afflict 90% of the world’s population (Global Forum for Health Research 2009). This awareness, along with increases in funding, have contributed to the prestige and desirability of international work in U.S. scientific circles, making global health research an increasingly rewarding career move for American clinical scientists. This shift has been led by the field of HIV/AIDS research, which has witnessed a dramatic increase in the amount of work being conducted outside the U.S., particularly in Africa, in the last decade (Crane 2007).

American HIV researchers were drawn to sub-Saharan Africa both out of a humanitarian desire to assist the countries hardest hit by the AIDS epidemic and by the unparalleled research opportunities provided by the availability of large populations of HIV-infected individuals. Take, for example, a research meeting I observed at Mariposa University [7] in California in February of 2005. The meeting was attended by eight researchers from a variety of fields – epidemiology, biostatistics, public health, and medicine – and the agenda was to design a common research protocol that could be used across the university’s growing number of HIV studies being conducted in Africa. The goal was to develop a standardized way of collecting social, behavioral, and biological information from African HIV patients participating in research, so that the data could then be “pooled” across studies conducted in different countries, creating larger and more powerful data sets for researchers to work with.

At this time, data regarding the advent of HIV treatment in Africa was of particular interest to these researchers. Free antiretroviral medications were just beginning to become available on the continent through PEPFAR and the Global Fund to Fight AIDS, TB, and Malaria, and the Mariposa scientists were eager to capture patients’ biological information at “baseline,” before they started therapy, in order to better document their response to treatment. This was particularly important because it provided a second chance to study the impact of HIV drugs on a large population of previously untreated people – a research opportunity that had been, in the words of the meeting’s organizer, “lost” in the U.S. As the group discussed how large a blood sample would be necessary in order to obtain the desired biological data, Greg Robin, the physician-researcher leading the meeting, suggested that the African study

[7] The name of the university, as well as the names of researchers attending the meeting, are pseudonyms.



participants have their blood drawn twice, arguing, “I can’t emphasize this enough – a biological specimen in the pre-treatment era is just golden to us. And 7mls of blood just isn’t enough.”

Afterwards I asked Dr. Jason Beale, another researcher in attendance, what Robin had meant when he said a research opportunity had been “lost” in the U.S. What Africa offered, Dr. Beale told me, was the possibility of studying the virus as it evolved in relation to exposure to drugs. The Mariposa researchers believed that knowledge about this evolution could provide useful information about both the pathophysiology and treatment of HIV. The opportunity to conduct such a study was lost in the U.S. because effective drugs became available here much earlier in the epidemic, before researchers realized what Beale called the “scientific value” of such a project. This recognition of scientific value would come later, after the development of viral load and drug resistance tests that allowed researchers to study the impact of antiretroviral drugs at the molecular level, rather than simply at the level of the patient’s body (the clinical level). As a result, researchers did not begin to study the impact of treatment in this way until after drugs had been available for several years, and most U.S. patients had already been exposed to HIV medications. Thus, the opportunity to study the impact of HIV drugs on a large number of previously untreated patients in the U.S. was seen as “lost.” This was precisely the opportunity that Africa now offered.

However, it is important to note that Beale and many of his colleagues were not driven by scientific ambition alone, but also by a moral and humanitarian commitment to promoting access to HIV medications in Africa, where the vast majority of the world’s AIDS patients were dying (in part) due to lack of antiretrovirals. Beale’s own African research on adherence to HIV drugs was undertaken in response to claims by experts and policymakers that the drugs would be wasted on impoverished patients, who (the argument went) would likely miss doses and facilitate the development of drug-resistant viral strains (Donnelly 2001; Popp/Fisher, 2002; Stevens 2004). Beale’s work on antiretroviral therapy in Africa did take advantage of a scientific opportunity, but it was also a moral and political project aimed at using science to “prove” that Africans could indeed take the drugs properly, and should be given the opportunity to do so (McNeill 2003). In this way, global health can envision African patients both as suffering persons in need of treatment and also as “bodies of knowledge” capable of yielding valuable scientific information. This, in and of itself, does not make this research different from that conducted on patients in the United States or elsewhere. Research subjects and research scientists everywhere must balance between the clinical imperative to heal and the scientific priority of data production. What makes global health research different is the radical inequality and geographic distance

that underpin it, leaving the field haunted by a postcolonial power differential that it continually struggles against. In this context, the discourse of “partnership” between Northern and Southern institutions has emerged as a key strategy for confronting, at least rhetorically, the problem of inequality.

## Postcolonial Partnership

Aspiring academic global health researchers in the North, such as those at Mariposa University, are not unaware of the dubious ethical conditions under which earlier international research was carried out. In the colonial and post-independence eras, American and European scientists often simply collected the data they wanted and left with little accountability to local communities or researchers. In global health circles, this style of science is referred to disparagingly as “parachute,” “helicopter,” or “safari” research. Instead, “partnership” with scientists and institutions in poor countries is advocated as an alternative, more equitable approach to conducting international research. Most often, this call to collaborate is aimed at African universities, which make up the bulk of global health partnership agreements with North American institutions (American Universities’ Engagement in Global Health, 2009).

Partnership between American and African institutions provides U.S. researchers with access to desirable patient populations, as well as African colleagues qualified to shepherd proposals through local IRB approval. At the same time, partnership offers genuine benefits to African host institutions, including investment in infrastructure (such as laboratories, information technology, and buildings), job creation, and funded research opportunities for African investigators who might otherwise have little access to scientific grants. Many global health partnerships espouse an explicit commitment to “capacity building,” and offer training in research skills to African physicians with the goal of fostering local expertise and leadership in global health science. Thus, “partnership” is not an empty promise, and there are many ways in which these alliances are mutually beneficial. At the same time, however, significant inequalities persist, and the promotion by Northern stakeholders of global health as a “win-win” example of “real partnership” risks mystifying this. As Rene Gerrets has noted in his work on public-private partnerships in global health, “the notion of ‘partnership’ and its emphasis on equality and consensus, stands at odds with the diverse social realities and dynamics among the sites and actors that global health partnerships typically engage” (Gerrets 2010b).

Within the context of the CUGH meetings, the term “partnership” played a prominent role, serving as a defining characteristic of the field of global health, a descriptor of the role played by Southern institutions and experts, and a qualifying condition for membership in the consortium. However, what

defined a “real partnership” – legally, administratively, scientifically, or otherwise – was not discussed, leaving “partnership” as a vague idea that could be employed to many ends. For example, when meeting participants described challenges or inequalities they had encountered in their global health work, “partnership” was often proposed as the remedy. In one instance, a university president speaking at the 2009 meeting noted the need for “humility” in the face of global health interventions that had been unsuccessful. Citing a scenario in which donors had failed to realize that Sudanese recipients of insecticide-treated bed nets would want to wash the nets in order to remove the cooking smoke they collected (thus also removing the insecticide), he asked, “How will we do better in the future? By partnering with the people it impacts.” In a different mode, at the same meeting, an NIH researcher described the reluctance of some U.S. institutions to participate in global health research out of fear of losing grant money to foreign collaborators. This anxiety could be assuaged, she said, by funding “partnerships” between domestic and foreign universities. In juxtaposing these two examples, we can see that the concept of partnership is being used to describe very different things: in the first case, a call for community-based public health intervention, and in the second, the creation of a transnational institutional structure for the purposes of administering research funds.

However, despite the frequent invocation of the idea of “partnership” in global health, the field has given little consideration to what partnership actually entails in practice or to the wide variety of relationships that currently exist between Northern and Southern entities. This lack of attention to the meanings and activities taking place in the name of partnership risks obscuring the diversity of arrangements and complex power dynamics at stake. In particular, the equity aspired to in the term “partnership” does not account for the fact that collaborations between American and African institutions often resemble donor/aid receipt relationships. Because these partnerships often bring material benefits in the form of new or renovated university facilities, updated laboratory infrastructure, additional faculty, and/or employment opportunities, they provide an important source of revenue and resources for underfunded African institutions. However, this assistance often comes with embedded assumptions about research and curriculum priorities that American funders (often unknowingly) impose on their African collaborators (Holm/Malete 2010). Furthermore, the resources and prestige that come with international partnership may exacerbate existing inequalities within host nations by disenfranchising universities unable to attract partners. For example, while partnership with a Northern university may bring much-needed resources to an African host university such as Nelson Sewankambo’s Makerere University, the flocking of American partners to prominent, urban

institutions such as this one may at the same time further marginalize lesser-known Ugandan universities in more rural locations. **[8]**

The donor/recipient dynamic within such partnerships is further complicated by rules set by funding bodies, most notably the U.S. National Institutes of Health (NIH), where much of the funding for American-sponsored global health research originates. NIH regulations cap reimbursements for “indirect costs” – i.e., administrative and infrastructural overhead – at 8% for foreign institutions. By contrast, American institutions, which negotiate this rate with the NIH individually, are reimbursed for indirect costs at much higher rates: for example, the reimbursement rate for federally funded research at Johns Hopkins School of Medicine is 64% (Johns Hopkins Medicine 2010). For example, if Hopkins receives an NIH grant for \$100,000, another \$64,000 will be added on to this to cover “indirect” overhead costs, but a foreign university receiving a grant of the same size would only be given \$8000 to cover administrative expenses. The result is that universities in low-income countries in Africa and elsewhere are being asked to manage large scientific grants on behalf of global health partnerships, but are offered insufficient reimbursement for the administrative costs of doing so. I witnessed this in my own research, where the Ugandan organization established to serve as Mariposa University’s local fiscal agent suffered a financial meltdown as U.S. interest in conducting research at the site grew and the number of projects it was expected to administer ballooned.

This problem did not go unrecognized at the CUGH meeting, where one American scientist noted that the low reimbursement rate was simply not enough for foreign universities to build the infrastructure needed to support international partnership. An eight percent reimbursement rate, he said, is simply, “not very partner-like.” Or is it? While on some level this disparity in overhead reimbursement seems blatantly unfair, it is also arguable that in a “true” – i.e. truly equitable – partnership, one partner would not be responsible for subsidizing the operational costs of the other. **[9]** However, because of the steep economic inequalities that underlie these arrangements, the reality of “partnership” is that an African institution may asked to administer an NIH grant that is larger than its entire university budget, as one American CUGH meeting participant recounted. The result is a partnership that sets up the African participant for failure.

**[8]** Thanks to Ghefari Elsayed for highlighting the importance of this dynamic for me.

**[9]** Thanks to Richard Rottenburg for alerting me to this point.

## Conclusion: The Value of Inequality

Given the ongoing context of global socioeconomic inequality, how might these emerging disparities within global health partnerships and practices be ameliorated? Clearly, efforts to define the meaning,

scope, and mission of “global health” need to be more inclusive of perspectives from low-income nations – otherwise, claims of partnership are likely to remain strictly aspirational rather than actual. To do this, scholars from poor and middle-income countries need to be included in larger numbers and at higher levels in organizations like the CUGH, lest such groups become de facto clubs of North American academic power brokers. At the CUGH meetings, there was some awareness of this problem among North American participants. For example, in a discussion concerning the membership status of Southern “partner” institutions at the end of the 2009 meeting, one CUGH board member noted that “there is lots of discussion about how low and middle income partners should participate, but there are no representatives *from* a partner institution in the room.”

However, the challenges to equity within global health go beyond issues of definition and representation. In addition to making global health more inclusive, U.S. universities must come to terms with the fact that the very poverty and inequality that they aspire to remedy is also what makes their global health programs both possible and popular. In other words, in the world of academic global health, inequality is a valuable opportunity. In making this assertion, it is by no means my intention to reduce academic global health to opportunism. Although both American and African scholars and institutions may benefit from global health partnerships, their participation is motivated not only or even primarily by scientific or career ambition, but also by a genuine compassion for patients and a humanitarian desire to ease suffering. However, as the work of Fassin and others has shown us, humanitarian efforts do not stand outside politics, and humanitarian actions should not be above examination and critique (Fassin 2010; McFalls 2010).

The legacy of colonial-era power relations is an uncomfortable topic in global health, and one which the field seeks to avoid reproducing through the invocation of an ethic of “partnership.” However, as I hope this paper has shown, the espousal of partnership – while a noble aspiration – runs the risk of obfuscating both the enduring and novel forms of inequality that shape the transnational relations of global health. This includes the dependence of Northern global health programs on easy access to the bodies of under-treated patients in the global South, and the difficulty in envisioning how Southern clinicians and researchers might participate in global health. This complicated and paradoxical relationship to inequality is not usually addressed by Northern actors and institutions within the field, which tend to position their activities as straightforwardly beneficial for both the wealthy sponsor nation and the lower-income host country. To be fair, these programs do bring benefits to institutions in poor countries, and their presence is most often quite welcome. However, if global health wishes to

truly make strides towards its ethic of equitable partnership, the field must make a more genuine effort to grapple with the unequal terrain on which it operates and which, ultimately, serves as its condition of possibility.

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## Rezensionen

### Reviews

#### **Bruno Latour: *The Making of Law*. Cambridge (UK)/ Malden: Polity Press 2010.**

„Since Napoleon’s foundation of the Council”, zitiert Bruno Latour aus dem Brief eines Counselliers des Conseil d’État, der auf das Manuskript des Buches reagiert, „never has the Voice of Law been downgraded to the level of a mere interlocution among individual judges”. Der Mann beklagt sich über das, was Latour zumindest auf den ersten Blick mit seiner ethnologischen Studie „The Making of Law” tut: die alltäglichen Praktiken der Rechtsprechung in einem der höchsten französischen Gerichte, die auch zur Rechtsetzung beitragen, detailliert zu beschreiben. Und doch passiert tatsächlich das Gegenteil: Durch das Nachzeichnen der einzelnen Operationen, der Vielfältigkeit der Verbindungen, die für sich genommen banal und wenig belastbar erscheinen, zeigt Latour, wie und unter welchen Bedingungen „Recht sprechen“ vor sich geht und stabile Netzwerke aufbaut, welche das formale „Recht“ mit der übrigen Welt verbinden. Dementsprechend antwortet Latour – knapp 200 Seiten später – auf die Bedenken mit einer Gegenfrage: „What is the origin of this kind of defeatism that compels us to believe that if a human speaks he inevitably and quite pitifully lapses into error and illusion, and a thundering voice must always emerge from nowhere – the voice of nature or the voice of Law – to dictate his behaviour and his convictions? [...] The way in which unquestionable truths are gradually constructed through human interactions has always seemed to me to be more interesting, more enduring and more dignified.” (197)

Für die Studie hat das Enfant Terrible der Sozialwissenschaften, dessen Schriften gerade in den letzten Jahren im deutschsprachigen Raum eine gewisse Popularität erreicht haben, über vier Jahre hinweg sporadisch immer wieder als stiller Beobachter die Arbeit des Conseil d’État begleitet. Die daraus entstandene Ethnographie erschien bereits 2002 auf Französisch; wohl wegen der Spezifität ihres Gegenstandes brauchte es jedoch acht Jahre, bis 2010 die englische Übersetzung auf den Markt kam. Der Conseil d’État ist eine Art Oberster Verwaltungsgerichtshof, der jedoch gleichzeitig die Aufgabe hat, für Regierung und Parlament Expertisen für Gesetzesvorlagen zu erstellen. Mit ihrer Rechtsprechung schafft die Einrichtung, die als Bollwerk gegen einen übermächtigen Staat paradoxerweise von Napoleon (!) ins Leben gerufen wurde, Präzedenzfälle – *Case Law*, das ansonsten in Frankreich

eine untergeordnete Rolle spielt. Damit ist dieser institutionelle Zwitter (Judikative und Legislative) eine zwar auch in Frankreich den Meisten kaum verständliche, aber eine zentrale staatliche Institution. Dies und die Tatsache, dass Latour mit „The Making of Law“ eine Studie vorgelegt hat, welche die Perspektive und die Werkzeuge der von ihm maßgeblich geprägten *Actor Network Theory* an einem Gegenstand außerhalb der Naturwissenschaften – und insbesondere von naturwissenschaftlichen Laboren – anwendet, macht das Buch trotz des spezifischen Falls auch und gerade für alle lesenswert, die sich für die alltägliche Produktion und Reproduktion von Staatlichkeit in deren ganzer Materialität interessieren.

Das Buch umfasst fünf Kapitel, die auch für sich je eine abgeschlossene Einheit bilden und verschiedene Fragen oder Aspekte des „Recht Sprechens“ im Conseil d’État bearbeiten.

Das erste Kapitel („In the Shadow of Bonaparte“) führt in die eigenwillige Welt des Conseil ein, erklärt dessen Aufgaben und die groben Abläufe und stellt auch den (etwas eitlen, wie er von sich selbst sagt) Ethnographen vor, dessen Beobachtungen im Weiteren die Seiten des Bandes füllen. Gerade die Struktur der Körperschaft, um die sich das Buch dreht, sind für nicht juristisch ausgebildete Leser/innen ohne Vorkenntnisse des französischen Rechts- und politischen Systems nicht immer leicht zu verstehen, und die Vielzahl an französischen Titeln und Rechtsausdrücken, die Latour so gut als möglich in eine adäquate englische Übersetzung bringt, ohne dass es dort tatsächlich eine Entsprechung gäbe, erleichtern dies nicht. Hinzu kommt die für Latour typische Geschwätzigkeit, die aus seinen Texten häufig gute Prosa macht, es aber nicht immer erleichtert zu erkennen, wo genau der wichtige Punkt liegt. Man braucht also etwas Durchhaltevermögen auf den ersten 70 Seiten, wird dafür aber im Folgenden belohnt. Bereits im ersten Kapitel legt Latour nämlich die Besonderheiten des „Recht Sprechens“ („dire le loi“) dar, die später nach und nach wieder aufgegriffen werden: Die Übersetzung von relativ unartikulierten oder – aus juristischer Sicht – unspezifischen Beschwerden in eine rechtlich verständliche Sprache mit Positionen, die eine Verbindung mit dem vorhandenen Korpus der Rechtsprechung (Case Law) und allgemeiner Gesetze möglich machen. So werden aus hungrigen Tauben, welche die Sonnenblumen eines unbescholtenen Bürgers plündern, „böartige Tiere“. Einen ersten Eindruck bekommt man auch über die Feinheiten der Deliberationen, der Logiken der Entscheidungsfindung (es werden inhaltliche ebenso wie prozedurale Fragen verhandelt) und der Vielschichtigkeit der Schritte, die zu einer Entscheidung (im Fall der Rechtsprechung) oder zu einer Empfehlung (im Fall der Beratung eines Gesetzentwurfs) führen.

Im zweiten Kapitel („How to make a file ripe for use“) verfolgt Latour den Prozess, den eine eingereichte Beschwerde durchläuft, bis sie mit einer Entscheidung endet, und zwar anhand des verbindenden

den Elements des gesamten Durchgangs. „[T]he files, like the King, never die“ – Akten, sagt Bruno Latour, sind das zentrale Vehikel der Rechtsprechung, und Rechtsprechung, insbesondere die in den Gerichten der höchsten Instanz, ist ein Bereich, in dem „Staat gemacht“ wird. Und wie der Staat (oder der König) sterben sie nie. Sie sind es, um die die gesamte Arbeit an den verschiedenen Stationen organisiert ist. Erreicht eine Klage den Conseil, erhält sie eine Aktennummer, die auf ewig bleibt, und eine eigene Akte, in der nach und nach die unterschiedlichsten Schriftstücke mit irgendeiner Relevanz für den Fall gesammelt werden. Die Akte „reift“ heran, bis sie zur Bearbeitung durch einen „Reporter“ bereit ist. Der Reporter (Berichterstatter) beginnt dann, allein mit den Materialien der Akten und seiner Kenntnis (bzw. der Recherche) vorhergehender Entscheidungen des Conseil sowie einschlägiger Gesetze, den Fall in eine juristische Systematik zu übersetzen – er ordnet die Papiere, verbindet diese Texte mit Gesetzestexten und Entscheidungen von Präzedenzfällen. Im Verlauf dieser Prozedur verändert der Text der Klage den Charakter und wird mehr und mehr rechtsförmig. Dies setzt sich in den folgenden Beratungen fort, der Anteil der juristischen Termini nimmt zu, während der ursprüngliche Inhalt der Klage mehr und mehr in den Hintergrund rückt, bis am Ende nur noch ein grüner Umschlag mit einem Blatt Papier bleibt, in dem das Urteil verkündet wird. Die restlichen Unterlagen wandern in die Archive.

Hier fühlt man sich sofort an frühere Arbeiten Latours zum Labor oder zur Transformation des Amazonasbodens in einen wissenschaftlichen Text erinnert. Zusammen mit dem vierten bildet das zweite Kapitel den Kern der ethnographischen Analyse, und es ist dasjenige, das am stärksten auf die Materialität der Rechtsprechung eingeht.

Im dritten Kapitel zeichnet Latour die Hierarchien und die Zusammensetzung der Mitglieder des Conseil d'État nach und inszeniert diese – auch graphisch – ihrerseits als Knotenpunkte, in denen akkumuliertes Wissen und Verknüpfungen zur Außenwelt zusammenlaufen, welche in die Produktion des Rechts einfließen. Die Mitglieder sind keine Berufsrichter/innen, sondern zumeist Absolvent/innen der Staats-Elitenschmiede École Nationale d'Administration (ENA). Einige Wenige kommen als Quereinsteiger/innen nach Karrieren in anderen Bereichen (die durchaus auch in der Quantenphysik liegen können). Die meisten, auch nicht die Direkteinsteiger, verbringen keinesfalls ihr gesamtes Berufsleben im Conseil d'État, sondern nehmen immer wieder Auszeiten, um sich auf anderen Posten in Politik, Wirtschaft oder Wissenschaft zu versuchen. Es sind also Generalist/innen, die hier Recht sprechen, und sie tun es mit ihrem Common Sense und ihrer Lebenserfahrung. Dies und die zunächst etwas in der Luft hängende Aufzählung von Lebensläufen machen deutlich, dass Recht keinesfalls ein isolierter Raum der reinen

Lehre ist, sondern durch zahlreiche Bahnen mit gesellschaftlichem Leben verbunden. Gleichzeitig gelten, und dem widmet sich ein Großteil des Bandes, in der Sphäre der Rechtsprechung eigene Bedingungen und Mechanismen der Produktion von Wahrheiten ebenso wie spezifische Enunziationsregime.

Darauf geht das vierte Kapitel („The passage of Law“) genauer ein, das beobachtet, wie im Verlauf der Beratungen verschiedene „Wertobjekte“ oder auch (Akteurs-)Positionen verhandelt und neu bestimmt werden. Latour findet hier zehn Punkte, angefangen von der Autorität der beteiligten Akteure, über den Fortschritt der Klage und das Gewicht, das Texten (bzw. verschiedenen Textsorten) zugestanden wird, bis hin zur Frage der Grenzen des Zuständigkeitsbereichs von Recht. Hier werden die verschiedenen Elemente und Instrumente deutlich, die für die Arbeit des Conseils von so großer Bedeutung sind. Jeder Fall wird nur anhand von Texten und anderen Materialien, die sich in der Akte befinden, bearbeitet; darüber und über Schreibmaterial (inkl. Computern und deren Programmen) und Gesetzestexte hinaus gehend werden keine Hilfsmittel zurate gezogen.

Daran anschließend kontrastiert Latour im fünften Kapitel die Produktion von Fakten im Labor, wie er sie in früheren Arbeiten untersucht hat, mit der Feststellung von Fakten im Verlauf von Rechtsprechung – um gleich festzustellen, dass es sich bei den jeweiligen „Fakten“ um Gebilde unterschiedlicher Natur handelt. Er zeigt, dass die Perspektive der Actor Network Theory nicht nur für die Untersuchung der Entstehung und Stabilisierung von Wahrheiten in den Naturwissenschaften nutzbar ist, sondern auch in anderen Feldern wie der Wahrheitsproduktion im Feld des Rechts. Das heißt nicht, dass die Mechanismen in beiden Feldern die gleichen wären, sondern erst einmal nur, dass beide aufeinander anwendbar sind. Deutlich wird zum Beispiel, dass im Recht, anders als in den Naturwissenschaften, die Referenzpunkte ebenso wie die eingebundenen Akteure homogener (im Allgemeinen menschlich) sind, dass die Referenzketten anders funktionieren (nicht in beide Richtungen verfolgbar, sondern immer nur vom Fall zum Urteil führend) und dass andere Arten von Wissen verlangt werden.

Das Buch endet mit generellen Betrachtungen (a) zur Natur von „Anthropologie“ (Latour rechnet mit den Ethnolog/innen ab, welche die Welt als Symbolsystem verstehen und alles, so auch Rechtsprechung, als symbolische Handlungen interpretieren), und (b) zur Bedeutung des Rechts im Gesamtgefüge gesellschaftlicher Beziehungen, und nach gut 270 Seiten ist man überzeugt, dass die Behauptung, das Recht bestehe zwar zu 99,9% aus Löchern, die restlichen 0,1% machten aber das Ganze menschlicher Existenz aus, nur richtig sein kann – und ist dem großen Rhetoriker Bruno Latour wieder einmal für eine Sekunde auf den Leim gegangen.

**Anne Dölemeyer**

## **James C. Scott: *The Art of Not Being Governed. An Anarchist History of Upland Southeast Asia*. New Haven/London: Yale University Press 2009.**

Anarchists have a long history of criticising the state, especially in its 19<sup>th</sup> century form, and for good reason:

“To be governed is to be watched, inspected, spied upon, directed, law-driven, numbered, regulated, enrolled, indoctrinated, preached at, controlled, checked, estimated, valued, censured, commanded. [...] To be governed is to be at every operation, at every transaction noted, registered, counted, taxed, stamped, measured, numbered, assessed, licensed, authorised, admonished, prevented, forbidden, reformed, corrected, punished. It is, under pretext of public utility, and in the name of the public interest, to be placed under contribution, drilled, fleeced, exploited, monopolised, extorted from, squeezed, hoaxed, robbed; then, at the slightest resistance, the first word of complaint, to be repressed, fined, deported, sacrificed, sold, betrayed; and to crown all, mocked, ridiculed, derided, outraged, dishonoured. That is government, that is its justice; that is its morality” (Proudhon 1851). [1]

Anarchism has, however, ceased to be of great appeal, partly because the state has changed. Of course, the state has not changed as much as imagined in Max Barry’s dystopian novel *Jennifer Government*. [2] There, big corporations have taken over many of the functions of the state, including the functions Proudhon lamented 160 years ago. For corporations do keep track – at least, of their employees. Those not employed are not tracked, free, if you will – including being free not to have the means to live. Governance, hence, does continue in the logic of that novel even after the demise of the state, and order prevails, even if the order is more orderly for those solvent enough to afford it. For once a crime has occurred, one has to be able to shoulder the police fees in order to extract restoration, retribution, or justice (scaled in accordance with the service requested). Government is here a corporation among others, providing the service of crime prevention without having a monopoly on even this, for it cannot compel all who reside within its territory to pay their taxes. And yet, the numbering, assessing, regulating, indoctrinating, controlling and commanding lamented by Proudhon does continue to take place, only now by big corporations, only one of which is Government. These corporations convey the company name to their staff in order to organise their flock and publicise their allegiance (hence, “John Nike”, “Hayley McDonald”, “Jennifer Government”, and so on).

[1] Proudhon, J.-P. (1851) *What Is Government? General Ideal of the Revolution in the Nineteenth Century*. <http://fair-use.org/p-j-proudhon/general-idea-of-the-revolution/> (13/11/2010)

[2] Barry, M. (2003) *Jennifer Government*. New York: Vintage.

James Scott's great (not just large) Book *The Art of not being Governed* does take the anarchist perspective seriously, and just for the reasons that Proudhon and Barry find creepy: for governing, by corporations or states, depends on turning an assemblage of people into a *legible* aggregation. One European way of doing this was to insist on the patrilinearity of names – allowing, first, the identification of individuals as such, then, their registration, then their immobilisation, their compulsory schooling and concomitant comparisons and finally their ethnogenesis. From the requirement that each have a unique name, has been added the stipulation that each individual is to have a national and sometimes additionally an ethnic identity. From the state perspective, this allows individuals to be taxed, conscripted, compared, counted and checked. From the individual's perspective, the reach of the state has come to seem insurmountable – and yet, as James Scott argues here, this is a misapprehension.

In *The Art of Not being Governed*, James Scott presents a novel reading of several decades worth of ethnographical scholarship on the peoples living in a mountain range in Southeast Asia that has been called “Zomia”. It is a mountain range spanning from Vietnam to Tibet to Afghanistan, and in which about 100 million people are said to live. These millions *technically* live *within* the territories of states, but, according to Scott, this is but an oft-repeated fiction. For in practice, he argues, they have arranged themselves in terms of *distance* to the states – vertical distance, as it were: The higher the groups of people live on the mountains, the less likely it is that they are named, registered, immobilised, schooled and ethnicised.

Instead of reading these groups as *uncooked*, *barbaric* or *uncivilised*, as a statist or “civilised” perspective would suggest, Scott argues that they life styles should be seen as the result of wilful attempts to escape being made legible by the states that surround them. Even their ethnicity changes with respect to the circumstances in which they are asked about them. Nor can (state-sponsored) ethnographers easily tie them into ethnic groups with the help of “objective” markers: The people at issue tend to be versatile in several languages, change their claims as to their ancestry in view of the allegiances that seem relevant now, alter their names in the course of their lives or the circumstances in which it makes sense. They also tend to be very flexible when it comes to religious allegiances, and are happy to be millenarian if that seems enjoyable. They participate in economic ventures that allow for great freedom of movement – hunting and gathering, trade, raids on sedentary valley peoples, and so on. And finally, they are, according to Scott, *post-literate*. Scott argues that oral history has great advantages over written history in situations in which people need to be able to change allegiances quickly and with determination: their alliances can only have always already existed when there is no written document to the contrary. All in

all, these people, through large and small adaptations in their mode of living, have repelled the incursions of state ordering, by over centuries developing strategies in evading the very first steps that would allow them to have become legible: They cannot be individuated or grouped. Thus, they cannot be taxed, nationalised, ethnicised, schooled or controlled. Hence the subtitle: An Anarchist History.

In contradistinction to Proudhon's invective inflection, Scott's tone is measured, his pace moderate, his enthusiasm contained. Nevertheless, he clearly admires the rationales of the escapees from the state. Other anthropologists have shown since that statehood can be unhealthy, hinder development and stand in the way of good order. Since the demise of the Somali state, for instance, by most measurable (and measured) indications of a good (or at least, acceptable) life, life in Somalia has – despite all laments as to the *failure* of the state – improved: life expectancy has increased, as have standards of living. And things are expected to become better, as long as the country continues to avoid the establishment of “another predatory state”. [3]

Between the Scylla of submitting to a Leviathan and the Charybdis of submitting to large corporations, there does, then, seem to be possibility of a Behemoth, relatively orderly and peaceful, but requiring a very different type of human than the state does. Scott's book is a compelling read, and if the core of the argument becomes clear in any set of a dozen pages of so one may pick out, the argument is nevertheless startling enough to keep the reader interested to the end. Of course, such an ambitious argument about the nature of the state and the nature of state evasion will be ambitious and hence controversial. Scott argues that his five state repellent techniques [4] (as one reviewer puts it) are used by all manner of “barbarian” peoples, including Berbers, Roma, Tuareg and Cossacks, and ethnographers of these people may very well quibble with the details. For social scientists interested in a refreshing look at the principles of order, this book is highly recommended.

**Rebecca Pates**

## **Matthias Bohlender: *Metamorphosen des liberalen Regierungsdenkens. Politische Ökonomie, Polizei und Pauperismus*. Weilerswist: Velbrück Wissenschaft 2007.**

Wie ist das liberale Denken entstanden und wie hat diese Denkweise des Liberalismus Ordnung in die Gesellschaft gebracht? Wie und in welchen Kontexten hat es Veränderungen erfahren? Und wie sind in diesem Zusammenhang neue Vorstellungen des Handelns der Subjekte, des Verhältnisses von Ökono-

[3] Powell, B./Nair, M. (2010) On the governance of 'not being governed'. In: The Review of Austrian Economics. Published online 28 April 2010, <http://www.springerlink.com/content/m646t5675g4h78vr/> (04/12/2010)

[4] 1. Mobility 2. Ethnogenesis 3. Post-Literacy 4. Religious Flexibility 5. Economic Mobility.



mie, Staat und (Zivil-)Gesellschaft sowie der Regierung dieser Bereiche entstanden? Das sind zentrale Fragen, die das Buch von Matthias Bohlender zu beantworten versucht.

Zunächst ist auffällig, dass Bohlender keine Gesamtgeschichte des Liberalismus schreibt – wie der Titel suggerieren könnte –, sondern sich die Periode zwischen 1750 und 1850 in Großbritannien als Forschungsschwerpunkt aussucht. Diese zeitliche Eingrenzung ist sinnvoll gewählt, denn sie umfasst zentrale Denker und Lenker des liberalen Regierens wie Smith, Ricardo, Malthus, Townsend, Chadwick, Hume, Mill, Bentham etc.

Theoretisch und methodologisch orientiert sich Bohlender an einer Gouvernamentalitätsanalyse, die nach Veränderungen in den Denkweisen zu „Staat“, „Ökonomie“ und „Gesellschaft“ fragt und die diesbezüglichen Problematisierungen, Wissensbestände und politischen Rationalitäten in den Blick nimmt. [5] Der Autor hat sich für diesen Fokus auch deshalb entschieden, um zu zeigen, dass für den Liberalismus das Problem der Regierung zentral ist. Auf der Grundlage eines Regierungsbegriffs verstanden als Führung von Individuen, Kollektiven und Dingen wird laut Bohlender für den Liberalismus die Erfindung von Technologien des Regierens von Gesellschaft, Staat und Ökonomie zur wichtigsten Aufgabe.

Um zu zeigen, dass diese Lesart im Gegensatz zu vorherrschenden Liberalismusinterpretationen steht, arbeitet sich Bohlender in der Einleitung an zwei dieser Interpretationen der politischen Ideen- und Ökonomiegeschichte des Liberalismus ab. Er konstatiert für die Geschichte des Liberalismus „zwei Meistererzählungen“ (10). Eine von ihnen wurde von A. V. Dicey schon Anfang des 20. Jahrhunderts inspiriert, die andere hat durch das Werk „The Great Transformation“ von Karl Polanyi die Prägung einer „ideengeschichtlichen Gründungsphilosophie“ (12) erfahren. „Unter dem Titel *Lectures on the Relation between Law and Public Opinion in England during the Nineteenth Century* zieht A. V. Dicey eine rechts- und ideengeschichtliche Bilanz des Liberalismus, die deutlich politisch motiviert und konfrontativ strukturiert ist. Interessant ist diese politisch-historische und ideengeschichtliche Bilanzierung deshalb, weil sie gleichsam das paradigmatische Raster einer Geschichtsschreibung des Liberalismus begründet, die bis heute an Suggestivkraft nur wenig eingebüßt hat“ (10). Dicey entwickelt die Idee des Liberalismus und dessen Ende als hegemoniales Gesellschaftsprojekt entlang sehr schematisch wirkender Gegensätze wie „Liberalismus vs. Sozialismus, Individualismus vs. Kollektivismus und *Laissez-faire* vs. Staatsintervention/Protektionismus“ (11). „Diese ausschließlich binäre Logik der Interpretation wird jedoch in einigen Fällen mit eigentümlichen historischen Ereignissen, Gesetzgebungs- und Regierungspraktiken konfrontiert, die Diceys Interpretationslinie (Periodisierung) zumindest soweit durchlöchert, dass er gezwungen ist, ein kurzes Kapitel über *The Dept of Collectivism to*

[5] Vgl. zu diesem Ansatz die aktuellen Sammelbände *Governmentality. Current Issues and Future Challenges*, hrsg. von Ulrich Bröckling, Susanne Krasmann und Thomas Lemke, 2010 und *Michel Foucaults ‚Geschichte der Gouvernamentalität‘ in den Sozialwissenschaften. Internationale Beiträge*, hrsg. von Susanne Krasmann und Michael Volkmer, 2007.

Benthamism' einzuschieben. Es sind seltsame Fälle wie ,die Bildung einer effektiven Polizei für London (1829), die rigorose und wissenschaftliche Verwaltung des Armenrechts (1834) unter der Kontrolle der Zentralregierung, die Einrichtung von Behörden für die Durchsetzung von Gesetzen zur Förderung der öffentlichen Gesundheit und die zunehmende Anwendung eines neuen Systems der Zentralisierung' (Dicey 1926, 306). Seltsam sind diese juristischen und politischen Arrangements nicht zuletzt deshalb, weil sie hybride Gebilde darstellen, die durch Diceys interpretatives Raster fallen. Sie enthalten freiheitliche und autoritäre, individualistische und kollektivistische Elemente; sie sind staatliche, legislative Interventionen zur Erzeugung eines regulären freien Marktes für Arbeit, Kapital und Boden.“ (11)

An Polanyis bekanntem Werk irritiert Bohlender die eindimensionale Sichtweise einer „sozialistisch-demokratischen Interpretationslinie des Liberalismus“ (13). Diese Sichtweise erfasst nur einen idealtypischen Liberalismus des totalen Marktes und der freien Individuen, dem ebenfalls politische und andere Grenzziehungen feindlich entgegen stehen. Hier findet Bohlender erstaunliche Ähnlichkeiten zwischen beiden Meistererzählungen, obwohl die eine sehr stark pro-liberal argumentiert, während die andere die Zerstörungskraft liberaler Politiken betont: „Für Dicey wie für Polanyi ist der Liberalismus eine relativ geschlossene, homogene und von einem zentralen Prinzip oder einer bestimmten Idee organisierte Weise der Weltanschauung“ (15). Dieser auf den Erklärungsprinzipien „Freiheit“ und „Markt“ basierenden hermetischen Interpretationen wirft Bohlender sogar A-Historizität vor.

Dem entgegen stellt der Autor seinen methodologischen Zugriff einer „Regierungserklärung“ des Liberalismus: Flankiert von historischen Arbeiten zur Entwicklung der englischen Staats- und Administrationsaktivitäten, die innerhalb der konstatierten Blütezeit des Liberalismus gedeihen konnten, versucht Bohlender den Zusammenhang von liberaler Gesellschaftsvorstellung und der gleichzeitigen Notwendigkeit seiner Regierbarkeit plausibel zu machen. „Der Liberalismus wäre demnach der theoretisch-reflexive und politisch-praktische Ort, an dem das Regierungsdenken, die Gouvernamentalität, überhaupt erst in Erscheinung treten kann; er wäre gleichsam das Vehikel, den gouvernementalen Zusammenhang von Staat, Gesellschaft, Bevölkerung und Ökonomie auf jenes Realitätsniveau zu heben, auf dem wir uns bis heute wie selbstverständlich zu bewegen glauben.“ (23)

Davon ausgehend arbeitet sich die Studie an Veränderungen ab (den titelgebenden „Metamorphosen“), die als Problemkonstellationen, als Regierungswissen und -techniken sowie als Interventionsfelder und -ziele eben kein konsistentes Bild des Liberalismus abgeben, sondern für genau diese Wandlungen konstitutiv sind. Die Studie setzt sich, von der Einleitung und dem Schluss („Vom Arbeitshaus zum Arbeitsamt“) flankiert, aus drei großen Teilen zusammen: von den „Metamorphosen I: Von der

Entdeckung der Gesellschaft zur Geburt der Politischen Ökonomie“ über die „Metamorphosen II: Wie man die Armen regiert“ hin zu den „Metamorphosen III: Die liberale Gouvernentalisierung des Staates“.

Im ersten Teil „Metamorphosen I: Von der Entdeckung der Gesellschaft zur Geburt der Politischen Ökonomie“ zeichnet der Autor anhand zentraler zeitgenössischer Schriften die Krise der Regierungskunst des 18. Jahrhunderts nach, welche bemüht war, auf die sozioökonomischen Veränderungen zu reagieren und neue Interventionssphären („Gesellschaft“) sowie neue Modi des Regierens („Politische Ökonomie“) zu konstituieren. „Eine Krise und Kritik der politischen Rationalität ist hier in Gange; Regieren und Regierung, die Bedeutung dieser Worte und die Praktiken, die mit ihnen verbunden sind, werden zu einem Problem und daher erneut problematisiert“ (29). Rousseau wird von Bohlender beispielsweise nicht als Vordenker von Gemeinwohl und Republik präsentiert, sondern zurecht als Übergangsdenkler zwischen den alten Vertragstheorien von Hobbes und Locke und der noch nicht entwickelten Rationalität der Politischen Ökonomie verortet.

Um diese Krise zu überwinden, half die sog. „Entdeckung der Gesellschaft“, also die Erfindung eines neuen Realitätsniveaus, das sich wiederum aus dem Spannungsverhältnis zwischen Kontinuität (Stabilität) und Wandel (Dynamik) sowie den (ökonomischen) Transaktion innerhalb dieser Gesellschaft zusammensetzte. Das Verhältnis von Gesellschaft und Ordnung ändert sich dabei grundlegend, denn „die Gesellschaft erfindet und reguliert ihre Ordnung selbst“ (49). Daraus ergibt sich, dass „Staat, Regierende und die Regeln der Gerechtigkeit (...) aus der Ökonomie der gesellschaftlichen Transaktion abgeleitet“ sind. (50) Hieran knüpft eine Bohlendersche These an, die einer dialektischen Denkweise entspricht: „Die Gesellschaft existiert, weil sie erklärt werden muss und die Gesellschaft muss erklärt werden, weil sie existiert“ (54). Was eigentlich für Humes Konzept eines neuen Regierungsdenkens reserviert ist, passt auch auf alle weiteren Schritte der Entwicklung und Transformation einer liberalen politischen Rationalität, die an eine „Dialektik der Ordnung“ (Zygmunt Baumann) erinnert. Bohlender zeigt sehr genau: Der Gesellschaftsbegriff hilft, um die (Un-)Ordnung ganzer neuer sozialer Dynamiken und menschlicher Handlungsweisen zu kanalisieren und zu klären (die den Erfindern der Gesellschaft bekannt waren), und umgekehrt ist dieser Begriff notwendig, um überhaupt Gesellschaft gewissermaßen als Gesamtheit zu produzieren.

Blicken wir wiederum auf die in der Gesellschaft sich entwickelnden Probleme, die sich aus dem Fortschreiten des Kapitalismus ergaben, skizziert Bohlender folgende Schwerpunkte: Zum einen muss die Arbeit aus ihrem vorkapitalistischen Arbeits- und Armenregime befreit werden, das aus der engen

Regulation der Arbeits- und Beschäftigungsverhältnisse, der Politik der Armenfürsorge, sowie der Arbeitsmobilitätsbeschränkung bestand (76-77). Diese alte gesellschaftliche Ordnung wurde problematisiert und es galt, eine den neuen Verhältnissen entsprechende neue politische Rationalität zu entwickeln. In deren Zentrum standen dem Autor zufolge die Freiheit der Menschen über ihr Leben und vor allem über ihre Arbeit, wie es Adam Smith in seinem Buch „Wealth of Nations“ formuliert hat (101). Die gesellschaftliche Ordnung wurde hierbei mit einem Plädoyer für die Selbstregierung von Arbeit, Handel und Ökonomie sowie mit dem Rückzug des Staates neu konstituiert. Es könne jedoch nicht von einem Ende des Staates die Rede sein, sondern von verschiedenen Bewegungen der ökonomischen Regierung des Staates (Adam Smith); des weiteren wurden Forderungen nach einer „Vervielfältigung, Intensivierung und Methodisierung“ (104) staatlichen Regierungshandelns proklamiert (James Stuart).

Wie der zweite Teil der Studie „Metamorphosen II: Wie man die Armen regiert“ zeigt, konnte das Ende des staatlichen Regierens auch deshalb nicht folgen, weil mit dem Problem der Armut und vor allem des „Pauperismus“ neue soziale Dynamiken auftraten, die wiederum politische Eingriffe erforderten bzw. legitimierten. Allerdings zeigt Bohlender, dass innerhalb des Zeitraums von 1790 bis 1820 auch aus heutiger Sicht radikale Ansichten innerhalb des Liberalismus diskutiert wurden, die das Problem der Armut einfach durch eine Politik des „Sterben-lassen“ lösen wollten (Joseph Townsend). Auch wenn sich diese Sichtweise des „*Laissez faire, Laissez mourir*“ nicht durchsetzen konnte (150), blieb die Bevölkerungsproblematik ein zentraler Punkt liberaler Denkweisen, die sich u.a. in Thomas R. Malthus „Essay on the Principle of Population“, der als einer der ersten auf die ökonomischen und sozialen Knappheitsprobleme von Bevölkerungsentwicklung und Nahrungsmittelproduktion hingewiesen hat, sowie in Frederic Morton Edens empirisch-historischem Meisterwerk „The State of the Poor“ widerspiegelte. Um der Armutsproblematik Herr zu werden, wurden neben dem „Pauper Management“ neue „Politische Technologien des Pauperismus“ entwickelt (175): Zunächst eine neue Polizei, die auf dem Prinzip der Prävention aufbauend die sozialen Verhältnisse regieren sollte. Die ab 1798 sich entwickelnden neuen Polizeibehörden hatten ausdrücklich für eine bestimmte „Ordnung, Klassifizierung und Überwachung einer bestimmten Personengruppe innerhalb eines bestimmten Raumes“ zu sorgen (175), wobei es darauf ankam, diese Regulierung der Gesellschaft präventiv auszuüben. Die Polizei wurde hier als Teil der Politischen Ökonomie verstanden, welche die guten Teile der Gesellschaft vor ihren schlechten Einflüssen schützte und bewahrte. Eine zweite Management-Technik war das Arbeitshaus, dem vor allem Bentham in seiner Utopie des „Pauperland“ eine zentrale Rolle zuge-

stand. Benthams Arbeitshäuser waren seine berühmten Panopticons, in denen die Pauper unter ständiger Beobachtung und Kontrolle produktive Arbeit verrichten sollten. Das „Pauper Management“ war somit auf eine Steigerung der Produktivität ausgerichtet.

Im dritten Teil seiner Forschungsarbeit „Metamorphosen III: Die liberale Gouvernentalisierung des Staates“ beschreibt Bohlender die entscheidende Konsolidierungsphase des Liberalismus, in der folgende Interpretation Plausibilität beanspruchen konnte: „Entgegen der weitverbreiteten Meinung, dass der Liberalismus eine institutionalisierte Trennung von Staat und Gesellschaft, von Politik und Ökonomie anstrebe, wird hier deutlich, dass er vielmehr die Herstellung eines spezifischen Verhältnisses beider Bereiche verfolgt“ (257). Diese These belegt der Autor zunächst anhand des Lohnverhältnisses, das nach 1830 durch das liberale Regierungsdenken politisiert wurde, indem z.B. das Verhältnis zwischen Arbeiter\_innen und Unternehmern plötzlich als Regierungsverhältnis definiert wurde, aus dem sich der Staat, aber nicht die Politik, herauszuhalten habe. Diese neue Logik der Trennung von Staat und Politik „eröffnet dem liberalen Regierungsdenken erstmals die Möglichkeit, den Staat im Namen der Politik zu kritisieren und zugleich zu reformieren“ (263). Gesetze und andere staatliche Regulationen, die den jeweiligen Regierungsrationalitäten nicht entsprechen, können so erkannt, verändert oder abgeschafft werden. Dies ist umso mehr nötig, wenn z.B. die innere Regierung des Arbeitsverhältnisses an dem Verhalten der Arbeiter und Armen scheitert, weil sich diese den geregelten und geordneten Verhältnissen durch Streik, Lohnforderungen und politische Selbstorganisation (Gewerkschaften, Parteien) entziehen.

Als nächste Schritte wurden deshalb verschiedene Reformvorhaben entwickelt, die im Spannungsverhältnis zu der Frage standen, wie zivil, ökonomisch oder politisch das Regierungshandeln (des Staates) war. Die Neuerungen betrafen wiederum die Polizei, die Armengesetze, die Fabrik, das Strafrecht und das Gefängnis sowie die Neuordnung des öffentlichen Gesundheitswesens (284-285). Um diese Reformen herum entstanden neue Institutionen, Behörden und Wissenskomplexe, die ein immer engmaschigeres Netz gouvernementaler Rationalitäten und Praxen ausbildeten. Dieses Netz spannte sich zwischen „der Lebensführung der Bevölkerung und der Lebensführung des Staates“ (329), dem laut Bohlender kein Gegensatz zur liberalen Denkweise eingeschrieben war: „All die Arbeitshäuser, Gefängnisse, Polizeitruppen, Kontrollbehörden und die Inspektoren waren als *Regierungstechnologien der Freiheit* gedacht“ (329, Her. i. O.).

Dieses Zeitalter der Gouvernentalität hat bis heute Bestand, auch wenn neue Problematisierungen und politische Rationalitäten wie die „Versicherungsgesellschaft“ (Francois Ewald) oder die

Lohnarbeitsgesellschaft mit ihrer Kehrseite – der Arbeitslosigkeit – entstanden sind und gesellschaftlich verarbeitet wurden. Hierbei übersieht Bohlender, der sonst souverän mit den Quellen und der historischen sowie methodisch anschlussfähigen Literatur umzugehen weiß, die historische Arbeit „Unemployment and Government“ von William Walters, welche die Erfindung und Regierung von Arbeitslosigkeit zum Thema hat. Dieses Buch ist wie Castels’ „Metamorphosen der sozialen Frage“ ein weiteres wichtiges analytisches Bindeglied zum Verständnis der Gegenwärtigkeit von Geschichte. Dennoch gelingt es Bohlender – und dieses Ziel betont er im Schlusskapitel –, das Verstehen des liberalen Denkens und Handelns unserer Zeit durch einen detailreichen Blick in seine Genese zu erleichtern. Bedauerlich aus Sicht des Rezensenten ist lediglich Bohlenders zu bescheiden formulierte Kritik am Liberalismus, indem er zwar konstatiert, dass *„die selbstverständliche und unhinterfragbare Ausrichtung der menschlichen Lebensführung auf jenes historisch-gesellschaftliche Tausch- und Regierungsverhältnis, das allein geleistete Arbeit in Zeit und Geldlohn misst“* (382, Her. i. O.) bis heute existiert, aber keine Alternativen zu dieser Problematik diskutiert. Andererseits ist dem Autor zugute zu halten, dass die durch das Schreiben einer alternativen Geschichte des Liberalismus erzeugten Irritationen möglicherweise für mehr Bewegung in der herrschenden Ordnung sorgen könnten als herkömmliche und altbekannte Plattitüden und Polemiken.

**Stefan Kausch**