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Life at the Influenza Epicentre:  
Transactions of Global Health and Animal Disease  
in Contemporary China

By

Lyle Arthur Fearnley

A dissertation submitted in partial satisfaction of the requirements for

the degree of

Joint Doctor of Philosophy  
with University of California, San Francisco

in

Medical Anthropology

in the

Graduate Division

of the

University of California, Berkeley

Committee in Charge:

Professor Paul Rabinow, Chair  
Professor Xin Liu  
Professor Dorothy Porter  
Professor Massimo Mazzotti

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## Abstract

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This thesis is an anthropological study of the making of global health on China's grounds, based on inquiry into the health programs assembled around China's poultry sector amidst outbreaks of avian influenza. Influenza pandemics are global in scale, but since the 1960s flu experts have hypothetically located the origin, or "epicenter," of flu pandemics in southern China. Drawing on extensive fieldwork among flu experts, Chinese veterinarians, and farmers, the thesis describes how the farmed ecology of Poyang Lake in southern China is being studied as a possible epicenter of influenza emergence. As global dangers are attributed to specific Chinese environments and agricultural practices, I follow flu experts as they move from the laboratory to the field and as they shift their scientific object from the sequence to the ecosystem. This movement outside the laboratory is also a movement onto China's grounds, a motion that I argue raises three anthropological problems.

First, emerging transnational scientific collaborations around avian influenza go well beyond the idea that China is a political obstacle to global health. These scientific transactions taking place on China's grounds reframe the historical problematization of China and modern science, as a rising China plays a fundamental role in shaping the contemporary global science of influenza.

Second, unlike other well-studied Asian biotech sites, bird flu research is grounded in transactions with farmers, livestock animals, and rural ways of life. The thesis analyzes how influenza experts adjust their scientific concepts and categories to account for historically and culturally specific practices of breeding and raising livestock, and shows how these livestock breeding practices continue to exceed and complicate scientific categories.

Lastly, the thesis concludes by showing how China's livestock veterinarians become controversial mediating figures between the lab and the farm, transforming the social break between the scientist and the livestock breeder into an ethical problem of vocation and "quality" (*suzhi*). As a result, the thesis argues that the movement from the lab to the field calls for an

anthropology of science that attends to vocation and social relation as ethical problems, going beyond the sociology of scientific fact construction.

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## Introduction

In a 1982 paper, Kennedy Shortridge, the director of the microbiology lab at the University of Hong Kong, and C.H. Stuart-Harris, an English virologist, identified southern China as a possible "epicentre" of influenza pandemics. Due to the "interchanges of virus" between animals and humans in the heavily populated region, they argued that southern China was a hypothetical "point of origin" for new influenza viruses that could spread throughout the world's human population. "The closeness between man and animals could provide an ecosystem for the interaction of their viruses," they wrote. This "closeness" between human and animal was a consequence of long-term agricultural transformation of the natural environment, and in particular the wet-rice paddy system, which produced "a close bird/water/rice/man association." A dangerous pathology lurked in southern China's unique configuration of nature and culture, they suggested, or more specifically, in the "age-old" Chinese techniques of cultivating nature.<sup>1</sup>

Although locating pathology in cultures and environments, Shortridge investigated the influenza epicentre at a molecular scale. Much like Bruno Latour's rendition of Pasteur<sup>2</sup>, Shortridge made the agricultures and ecologies of southern China pass through his laboratory, identifying and distinguishing pathological forms based on the quality and quantity of viruses brought back inside the walls of the Pathology Compound, Queen Mary's Hospital, University of Hong Kong. As Frédéric Keck has argued, Shortridge constructed his laboratory--and ultimately, Hong Kong itself--as a "sentinel" for pandemic influenza viruses, a device able to isolate new viruses as they emerged in southern China but before they spread to the world.<sup>3</sup> When, in 1997, members of his laboratory attributed the cause of a three year old boy's death to a highly pathogenic avian influenza virus (typed as "H5N1") that had recently been isolated during outbreaks among Hong Kong's poultry farms, scientists widely saw a confirmation of Shortridge's hypothesis and raised alarm about an imminent pandemic.<sup>4</sup>

As the highly pathogenic avian influenza (HPAI) H5N1 virus continued to spread outward from southern China into southeast Asia and beyond, however, Shortridge's formulation of the object and the problem began to show its limitations. New agencies and actors, with different forms of expertise and objectives, began to join the now increasingly publicized and well-funded battle against the next flu pandemic. In particular, a "framework" known as "One world, one health" became a powerful, if often vague, motto for organizing new forms of interdisciplinary research on influenza, joining virologists, veterinarians, wildlife specialists, and

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<sup>1</sup> Kennedy Shortridge and C.H. Stuart-Harris, "An influenza epicentre?" *The Lancet* (October 9, 1982): 812-813.

<sup>2</sup> Bruno Latour, "Give me a laboratory and I will raise the world!" in Mario Biaogoli, ed., *The science studies reader* (New York: Routledge, 1999); Latour, *The pasteurization of France* (Cambridge, Ma.: Harvard University Press, 1988).

<sup>3</sup> Frédéric Keck, "Une sentinelle sanitaire aux frontières du vivant. Les experts de la grippe aviaire à Hong Kong" *Terrain* No. 54 (2010): 27-41. Cf. Keck, *Un monde grippé* (Paris: Flamsarrion, 2010).

<sup>4</sup> JC de Jong, EC Claas, AS Osterhaus, RG Webster, WL Lim. "A pandemic warning?" *Nature* Vol. 389, No. 6651 (Oct 9 1997): 554-554.

social scientists, among others.<sup>5</sup> Most importantly, China's governmental and scientific institutions began to participate actively and collaboratively in the making of influenza science, inviting international organizations and influenza researchers to work in China, as well as developing their own large-scale research and control programs.

This dissertation is about how the science of influenza changed when scientists moved into the influenza epicenter and onto China's grounds. Without necessarily abandoning the tools and insights of virology or molecular biology, these scientists made the cultivated nature of the influenza epicentre, rather than the viruses it produced, into a scientific object in its own right. This was something Shortridge had never done: remarkably, his claims about China's agriculture and ecology are attributed either to anecdotes or in one case to Joseph Needham's classic historical epic, *Science and Civilization in China*. Now, new research was initiated into the flight patterns of wild birds, poultry market-chains, or the geography of wet-rice paddy cultivation in southern China. Scientists began to localize the influenza epicentre within southern China, honing in on the Poyang Lake in Jiangxi Province and developing an "integrated pilot study" at the lake. Put simply and practically, and in a form that will have to be subsequently defended, I argue that when scientists moved inside the influenza epicentre, they also left the laboratory. This dissertation analyzes the stakes of this simultaneous entry and exit for science, for China, and for the ethics of truth and life.

### *Life outside the laboratory*

Life, it is often said, was redefined at a molecular scale by the 20th century genetic synthesis.<sup>6</sup> The DNA molecules that make up the genome of the individual or species not only stand in a metonymic symbolic relation to a form of life, but also a teleological one, as genotype to phenotype. Nor are they limited to the determination of physical form, for the "selfish gene" is also proposed as a powerful explanation of animal behavior and even human culture. In a mundane sense, this "molecularization of life" has been made possible by the particular configuration of space and scientific practice provided by the laboratory. In this respect, the science of infectious disease is paradigmatic.<sup>7</sup> The late 19th century work of the great bacteriologists Pasteur and Koch, as well as their less well known but far more numerous followers, accomplished an ontological shift in the understanding of health and disease by defining infectious disease according to the presence or absence of "germs" or "microbes."<sup>8</sup> As Bruno Latour has argued, Pasteur succeeded by translating problems such as anthrax into the space of the laboratory where they could be worked over on his own terms, then subsequently extending the laboratory to encompass and transform French 'society.' Latour's work on Pasteur

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<sup>5</sup> Yu-ju Chien, "How did international agencies perceive the avian influenza problem? The adoption and manufacture of the 'One World, One Health' framework" *Sociology of Health and Illness* Vol. 35, No. 2 (2013): 213-226. See also Chien, "Constructing knowledge and policies on avian influenza: How do international agencies craft global models?" Ph.D. Dissertation, University of Minnesota (May 2013). Benjamin Hickler, "Epidemic oversight: Emerging infections and rural livelihoods in the Mekong." Ph.D. Dissertation, University of California, San Francisco (2010).

<sup>6</sup> See Canguilhem, *A vital rationalist: selected writings from George Canguilhem* (New York: Zone Books, 1994); Lily Kay, *Who wrote the book of life?: a history of the genetic code* (Stanford: Stanford University Press, 2000); for a critical review and questioning of the viability of this conception for understanding contemporary bioscience, see Carlo Caduff and Paul Rabinow, "Life--after Canguilhem" *Theory, Culture and Society* Vol. 23, No. 2-3 (2006): 329-331.

<sup>7</sup> But see Warwick Anderson, "Natural Histories of Infectious Disease" *Osiris* Vol. 19, No. 1 (2004): 39-61, for a minor tradition of "ecology" minded infectious disease research during the twentieth century.

<sup>8</sup> Canguilhem, *op cit*.

is, he suggests, a demonstration that the anthropology of laboratory practice need not be confined to the laboratory--for in fact, he claims, the laboratory is a mechanism for destabilizing distinctions between inside and outside, micro- and macro- scale. But the text far more convincingly shows, perhaps against his own intentions, the historical appearance and therefore *particularity* of the laboratory as a mode of veridiction or truth-telling.<sup>9</sup>

In this vein, historians have shown that the rise of the laboratory as the dominant mode of truth also produced its outside: the field. As Jeremy Vetter writes,

"In its broadest meaning, the field can be anywhere outside the laboratory where scientists have worked. While laboratories have aimed to produce universal, placeless knowledge, field sites have produced knowledge that is based in place. Historically, as rhetorically placeless laboratories ascended to their high epistemic status in modern science by the late nineteenth century, "the field" was simultaneously reconstructed as the residuum of messy, complex, and uncontrollable nature."<sup>10</sup>

Indeed, Latour is clear that there are both winners and losers in the Pasteurian laboratory. The increasing "strength" Pasteur gains from his laboratory is used to defeat a different kind of knowledge practitioner: the veterinarian.

"The change of scale makes possible a reversal of the actors' strengths; 'outside' animals, farmers and veterinarians were weaker than the invisible anthrax bacillus; inside Pasteur's lab, man becomes stronger than the bacillus, and as a corollary, the scientist in his lab gets the edge over the local, devoted, experienced veterinarian."<sup>11</sup>

This dissertation is an examination of scientific practices that attempted to understand influenza *at the epicentre* rather than inside the laboratory. My fieldwork, conducted between September 2010 and January 2012 in China, followed two transnational venues of scientific activity as they organized research and control activities on avian influenza in China. The first of these venues is an "emergency center" established by the Food and Agriculture Organization of the United Nations in response to the global health project to control avian influenza H5N1. The second is a "workshop" funded by the National Institutes of Health that facilitated the organization and communication of research on avian influenza H5N1 in China and southeast Asia. In both venues, the veterinarians, ecologists, wildlife researchers and others involved were aware of the surplus of prestige accorded to laboratory sciences, and often resented it. As a wild bird migration specialist complained,

"[E]ven when you have expensive, detailed studies with it [wild bird field studies], there's lack of control in the experimentation, and there's a lot more in lab work that you could control

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<sup>9</sup> On modes of veridiction, see Foucault, *The courage of truth (The government of self and others II): lectures at the College de France, 1983-1984* (New York: Palgrave Macmillan, 2011); Paul Rabinow, "Dewey and Foucault: What's the problem?" *Foucault studies*, No. 11 (2011): 11-19. For an historical approach to the changing configurations of truth and social order, including the birth of the "experiment" and the "laboratory," see Shapin and Schaffer, *Leviathan and the air-pump*.

<sup>10</sup> Jeremy Vetter, "Introduction" in Vetter, ed., *Knowing Global Environments: New historical perspectives on the field sciences* (New Brunswick, NJ: Rutgers University Press, 2010). On the history of the field sciences, see also the essays collected in Henricka Kuklick and Robert Kohler, eds., *Science in the Field, Osiris*, 2nd Series, Vol. 11 (1996); Daniel M. Schneider, "Local knowledge, environmental politics, and the founding of ecology in the United States: Stephen Forbes and 'The Lake as Microcosm' (1887)" *Isis* Vol. 91, No. 4 (Dec 2000): 681-705.

<sup>11</sup> Latour, "Give me a laboratory," *op cit*.

and have more refined analyses, which may or may not be still better science in my view. So it ends up that in the higher journals, you might see publications in *Science* or *Nature* that, for the field there in, its not necessarily rocket-shaking big things, but because its the right field, it'll go in, and the highest, the best ecology paper, you put it in there and it won't get accepted because its "oh, its another ecology paper".

Turning this resentment into activity (and therefore not falling into *ressentiment*), these scientists took steps to transform the modes of truth-telling. For Latour as for Pasteur, claims to truth and efficacy have to pass through the laboratory; for the scientists I describe in this dissertation, claims to truth about influenza have to pass through the pandemic epicentre in southern China. In the process, they figured influenza as a *living being* that could only be known in terms of its active *existence* within a mileu.<sup>12</sup> Rather than the virus, their research investigated what Paul Rabinow has called "zones of virulence."<sup>13</sup>

By opening up the question of the mileu, however, they found themselves confronting anthropological questions about the nature of China's cultivated nature. Influenza viruses existed within--even, as Shortridge had suggested, were created by--an environment that was fundamentally shaped by human practices. I argue that in order to epistemologically account for the mileu of the influenza viruses, the scientists also had to develop new and normatively uncertain relations and transactions with Chinese subjects, from government administrators to state-employed scientists, and from local veterinarians to poultry breeders, transporters and marketers. In refiguring the knowledge of influenza to account for its modes of living in a mileu, the scientists also posed new questions about the practice of science and the making of scientific knowledge.

### *Science and China: Global encounters on China's grounds*

China has frequently played the mirror to our understandings of modernity. Given the scale, wealth, power and vitality of its civilization, why did China fail to develop modern forms of science? "Why not" China?<sup>14</sup> This question was also adopted by Chinese reformers, who objectified and interrogated their own tradition for the cause of China's lack of wealth and power in the modern world. Scholarship on the Chinese modernizers, rather than wondering why science did not develop autonomously in China, instead asks how and in what forms modern science *was* adopted in China. Many accounts narrate this process of modernization as troubled or troubling. Joseph Levenson, for example, argues that the violent arrival of modern forms of social life in the late 19th century forced Chinese intellectuals to reckon with a breakdown between the "true" and the "mine," as the powerful modern forms of scientific knowledge were felt to be wholly foreign to the Chinese Confucian tradition, and thus caused a crisis of identity. Kwok argues that modernizers adopted science largely in order to discredit the Chinese philosophical tradition, and therefore embraced "scientism," an assertion of the value of science that often lacked any interest in or even awareness of contemporary scientific research.

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<sup>12</sup> Georges Canguilhem, "The living and its mileu"

<sup>13</sup> See for example Paul Rabinow and Carlo Caduff, "Security, territory, population." Unpublished manuscript. Rabinow draws on Foucault's notion of "zones of danger" but asks how Foucault's security apparatus is being re-problematized by contemporary emerging diseases.

<sup>14</sup> On science, see Joseph Needham, *Science and civilization in ancient China*. Multiple volumes. (Cambridge: Cambridge University Press, 1954--). For a similar question on capitalism, see Max Weber, *The religion of China: Confucianism and Taoism* (Glencoe, Ill.: Free Press, 1951)..

Following the institution of Soviet-style communism under the People's Republic of China, European and North American scholars began to raise questions about whether Chinese forms of modern science appropriately or effectively adopted the norms or practices associated with modern science.

Recently, scholars have critiqued what they describe as this "impact-response" formulation of the growth of modern science in China. Benjamin Elman, most notably, has argued that the narrative of China's "failure" to modernize is an outcome of how modernizers sought to interpret the Chinese loss in the Sino-Japan war of 1894-5. Long before this, Chinese had been active participants in the adoption of the "Western learning" which was seen to extend, but not require a radical break from, Chinese practices of investigating things (*gewu*).<sup>15</sup> Fa-ti Fan, in a monograph and a review essay, has similarly called for studies that investigate "transnational networks of science" to replace what he finds to be inadequate distinctions of modernity/tradition, West/non-West, and global/local.<sup>16</sup>

In this dissertation, I analyze how the project to scientifically understand and control pandemic influenza on a global scale was made to pass through China--the hypothetical influenza epicentre. Paul N. Edwards has argued, writing about the formation of climate science and the theory of global warming, that making knowledge on a global scale requires a groundwork of shared standards and infrastructures of communication. Only upon such a foundation could scientific claims about the entire globe become possible.<sup>17</sup> The same is true of the science of pandemic influenza, which as I demonstrate in Chapter One, is intrinsically tied to a global scale. But how is China figured in this globalism? I draw on Lydia Liu's work in translation studies, particularly her argument that the challenge is to "account for the philosophical connections between the universalizing logic of modernity and the invention of *hypothetical equivalences* among the world's languages" in the context of politics and power.

Rather than a comparison of science "in China" and science "in the West," this dissertation focuses on the *transnational* and *translational relationships* formed in the practice of making influenza science *on China's grounds*. Adopting the emphasis on the transnational introduced by scholars such as Fa-ti Fan, I diverge from their reliance on concepts such as "network" to describe the *form* of transnational relations. The concept of network, as scholars have shown, is closely tied to particular historical developments in economy and organization that can be dated to the 1970s and mostly to Europe and North America.<sup>18</sup> I suggest that the *uncertain* qualitative form of contemporary transnational relations is precisely what is at stake and remains *problematic*. Again drawing on Lydia Liu, I suggest that we cannot describe China as the "particular" in relation to universal modernity (as local to global), nor can we simply bracket the problem of difference through a description of networks without end. The narrative arc of my dissertation describes not only the effort of global science to *include* China within its purview, but also how the global is adopted by Chinese actors, or *included* within the world of China. A science adequate to the global health endeavor to contain or prevent pandemic influenza would have to be built on China's grounds--in terms of the material landscapes and

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<sup>15</sup> Benjamin Elman, *On their own terms: science in China, 1550-1900* (Cambridge, Ma.: Harvard University Press, 2005). Elman adds that *wu*, or 'things', could include not only material objects, but also events, mental and physical phenomena, the unknown, the anomalous (xxix).

<sup>16</sup> Fan, "Redrawing the map: Science in twentieth-century China," *Isis* Vol. 98, No. 3 (September 2007): 524-538.

<sup>17</sup> Paul N. Edwards, *A vast machine: computer models, climate data and the politics of global warming* (Cambridge, Ma.: MIT Press, 2010).

<sup>18</sup> Paul N. Edwards, "Weak discipline: network power/knowledge and new knowledge infrastructures."

ecologies that the virus inhabited; and in terms of the transnational political and ethical relationships within which knowledge about influenza could be made.

In the case of avian influenza, like SARS before it, much has been made about China's failure to report outbreaks or reticence to share samples of virus. The impression often exists of a barrier separating China from the world of global health--as one American scientist told me, he felt he faced a "Chinese epizootic wall". But during my fieldwork I was impressed by the vast number of transnational research projects on avian influenza being conducted in China, as well as the extent of transactions and exchanges across transnational boundaries. This dissertation provides an analysis of the *qualitative* form of these emerging relationships and the ethical and political problem-spaces growing up around them.

### *The ethics of truth: vocation and relation*

The laboratory became an important part of the sociology and anthropology of science because it metonymically captured an important shift in object of study from theory or social structure to the *practice* of making scientific knowledge.<sup>19</sup> Outside the laboratory, however, the object of science is also always the object of other modes of practice with distinct, non-scientific objectives. For example, a scientific knowledge about poultry and their diseases must inevitably contend with the breeders of poultry, whose practices ultimately create what will become the objects of scientific knowledge.<sup>20</sup> It would be tempting, therefore, to suggest that the claim to make scientific knowledge from field sites is quixotic, inevitably crippled by what Pierre Bourdieu diagnosed as the gap separating "objectivist" scientific knowledge from the "logic of practice."<sup>21</sup> Instead, I argue that the gap is not a sociological inevitability, but rather poses an *ethical problem*: how to appropriately govern the relationships and transactions between the parties of these distinct practices?

I suggest that as the movement outside the laboratory onto China's grounds produces a distinctive form of science with its own problems, an *anthropology* of this scientific practice must draw from roots other than laboratory studies and the 'practice turn'. Outside the laboratory is not simply a physical place; rather, it also refers to how science is taken up within emergent assemblages of ethics and politics, as Paul Rabinow has articulated in his work on the anthropology of contemporary science. Rather than a theory of science (sociological, philosophical, or what have you), Rabinow has devoted analytic and conceptual inquiry to how developments in the life sciences pose new "problematizations" to existing classifications, practices and things, as well as the reconfigurations of modes of living that result.<sup>22</sup>

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<sup>19</sup> See, especially, Bruno Latour and Steve Woolgar, *Laboratory life*; Karen Knorr-Cettina, *The manufacture of knowledge*; Knorr-Cettina, *Epistemic Cultures: How the sciences make knowledge*; Ian Hacking, *Representing and intervening*; Shapin and Schaffer, *Leviathan and the air-pump*. For overview, see Knorr-Cettina, "Laboratory studies: the cultural approach to the study of science" in Jasanoff, et al, eds., *The handbook of science and technology studies* and Andrew Pickering, ed., *Science as practice and culture* (Chicago: University of Chicago Press, 1992).

<sup>20</sup> The case I develop in Chapter Four further elucidates this point.

<sup>21</sup> Pierre Bourdieu, *Outline of a theory of practice* (New York: Cambridge University Press, 1977); Bourdieu, *The logic of practice* (Stanford, Ca.: Stanford University Press, 1990).

<sup>22</sup> Paul Rabinow, *French DNA: Trouble in Purgatory* (Chicago: University of Chicago Press, 1999); *Anthropos Today: reflections on modern equipment* (Princeton: Princeton University Press, 2003); *Marking Time: on the anthropology of the contemporary* (Princeton: Princeton University Press, 2008); *The Accompaniment: assembling the contemporary* (Chicago, Il.: University of Chicago Press, 2011); Rabinow and Anthony Stavrianakis, *Designs on the contemporary: anthropological tests* (Chicago: University of Chicago Press, forthcoming 2014).

Both Max Weber and Michel Foucault situate truth, or scientific knowledge, within the ethical project of forming and living a life.<sup>23</sup> In this dissertation, I place these ethical practices of self-formation always in relation to ethical practices of relating to others. As James Faubion has argued, although Foucault did not reject the importance of other-oriented ethics--one of his major lecture courses on ethics was, after all, entitled 'The government of self *and others*'--he did not put these practices at the center of his historical inquiry.<sup>24</sup> But when scientists from the Food and Agriculture Organization organized a training program for Chinese veterinarians, or when American wild bird researchers went to Poyang Lake to mark wild birds, how to act within relationships that crossed national, disciplinary, and occupational differences came to the forefront of practice. But also, to make one last turn backward, I argue the ethical government of transactions with others must also be understood in terms of practices of self-cultivation.

The dissertation is divided into two parts. In the first part, I narrate a series of movements into the influenza epicentre, and how these movements raised ethical questions and discordances: first, about the transnational relations linking China and "global health" (Chapters One and Two); second, about the divisions separating the knowledge of life, accomplished by scientists, from the practices of cultivating life, accomplished by poultry breeders (Chapters Three and Four). In the second part, I show how these ethical problems became embodied in the ethical task of cultivating "high quality" state-employed, or "office" veterinarians (Chapter Five). The office veterinarian, trained in international standards, became figured as a mediating point for transnational collaborations and transactions. But the office veterinarian was also forced to confront the possible break between science and poultry breeding as two distinct and often opposed forms of life. The Chinese term for 'quality' (*suzhi*) indicates that this work of *training* and *exercise* involved cultivating both technical ability and moral value, but also, in contemporary China, sets the office veterinarian in an hierarchical relation to the 'backwards' rural areas. Finally, in Chapter Six, I contrast this office veterinarian with a figure I call the duck doctor, a kind of 'grassroots' (*caogen*) veterinarian who works out of small livestock pharmaceutical shops and develops ties of reciprocal obligation with poultry breeders. By examining how the duck doctor's diagnosis is situated within a series of reciprocal transactions, I develop an alternative *spectrum* of possible configurations of truth and ethics from the viewpoint of the duck doctor. Thus, if the first part of the dissertation tells how a "global" form of knowledge about influenza is brought onto China's grounds, the dissertation concludes by asking what happens if global forms of scientific knowledge--including my anthropological knowledge--are included within the duck doctor's moral and material diagnostic spectrum.

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<sup>23</sup> Max Weber, "Science as a vocation" in Gerth and Mills, ed., *From Max Weber: Essays in sociology*; Michel Foucault, *The hermeneutics of the subject: lectures at the College de France, 1981-1982* (New York: Palgrave-Macmillan, 2005).

<sup>24</sup> James D. Faubion, *An anthropology of ethics* (New York: Cambridge University Press, 2011).

**PART ONE**

**ON CHINA'S GROUNDS**



# Chapter 1

## Into the Epicentre

*The Poyang Lake is a perfect storm.* These words, spoken to me by an avian influenza expert a few weeks earlier, rang in my ears as the twenty-seat bus lurched down the embankment and bounced over the last low-lying fields to Wucheng, a small town on the Poyang Lake in southern China's Jiangxi Province. In Song dynasty times, the island town of Wucheng was a major rice trading post, a stopping point for barges running along the Poyang Lake's waterways between Nanchang, the provincial capital of Jiangxi, and the cities of the Yangtze river. Once dotted with clan temples established by the traders to memorialize their distant ancestral homes, only one temple remains standing today, decrepit and dusty, the faces of the statues broken and disfigured. These days it is the Poyang Lake Environmental Protection Zone that brings people to Wucheng from afar. The hotel there provides a temporary home for many birdwatchers and ornithologists--and most recently of all, for researchers seeking the origins of highly pathogenic forms of the avian influenza virus.

When I first visited Wucheng in April of 2011, I would never have imagined that I had arrived on an island in the middle of a lake. We had crossed no bridges, and had driven over a road that seemed remarkable for its dust. Speculation about the cause of the Poyang Lake's dramatic annual metamorphoses dates from antiquity. After the spring and early summer rains, the water level begins to rise, sometimes slowly and gradually, sometimes in terrible floods. By late fall, the water level drops again, in total fluctuating in depth up to fifteen meters. More significantly, the land covered by water during the summer flood season is nearly double that of the winter. Even the road to Wucheng is underwater during at least part of the summer, and the ability to make the journey at all meant that the dry season was still here.

Based on the studies of hydrologists, scientists now understand that Poyang Lake's water level depends on a complex relationship with the ebbs and floods of the Yangtze river. This expansion and retraction of the lake's surface has important consequences for the living environment of the lake. When the water retreats from the flooded area in the late fall, an enormous meadow appears, thick and tangled with the exposed roots of water plants. These roots and other small grasses attract the eye of migratory birds flying south from Siberia, and millions of birds make their home at Poyang during the cold winter months. Beginning in 1978, just as the status of expert scientific research was reemerging from the Cultural Revolution, two scientists from the Chinese Academy of Science Animal Research Center conducted preliminary surveys of the lake in search of the rare White Crane's over-wintering site. In the winter of 1980 they discovered a flock of 91 White Cranes wintering on the lake. Within a few years, Jiangxi Province had established a protected zone for migratory birds at the lake, a zone elevated to become one of the first national wildlife refuges in 1985.

But after a microscopic virus prompted global alarm about a possible influenza pandemic, scientists declared that danger lurked in the peaceful waters and dancing birds of Poyang Lake. For some, the "Kingdom of Precious Birds" (*xiniaowangguo*) could produce a perfect storm: a pandemic influenza virus.

## *Pandemic Globe*

There are broadly speaking two anthropologies of globalization, each claiming its own notion of the "global." The first, drawing largely from a Marxist tradition and world-systems theory, figures the global as a space unified under Western domination, including both colonial and capitalist forms. In this reading, "Western" science arrived as one part of a colonial or postcolonial intervention, and "transfers" of science or technology are implicated in the extension of these relations of power.<sup>25</sup> The second, drawing from Weber's identification of the "universal significance" of "formal" rationalities, argues that global forms are those which "have a distinctive capacity for decontextualization and recontextualization, abstractability and movement, across diverse social and cultural situations."<sup>26</sup> Science as a global form is thus not in conflict with the local, as "system" to "life-world,"<sup>27</sup> but rather always actualized in concrete "assemblages".<sup>28</sup>

In studying how China has become the grounds for a global project to understand and prevent influenza pandemics, I situate these two anthropological concepts within a seemingly much more straightforward notion of the global: a discourse or technique that makes the globe an object of knowledge or practice. Paul N. Edwards has argued, writing about the formation of climate science and the theory of global warming, that making knowledge on a global scale requires a groundwork of shared standards and infrastructures of communication. Only upon such a foundation could scientific claims about the entire globe become possible--including such apparently simple claims as 'the globe is getting warmer'.<sup>29</sup> Such a globalism is not as simple as it may seem, and indeed, can draw together the two anthropologies of the "global" into a common problem-space around the historical conditions of translatability. Adopting Lydia Liu's language--but extending its domain to include scientific practices as well as literary works or languages--the challenge is "to account for the philosophical connection between the universalizing logic of modernity and the invention of *hypothetical equivalences* among the world's languages" in the context of politics and power.<sup>30</sup> One way of approaching this challenge is to examine the relationships of encounter and exchange that form the conditions of possibility for the "globe" as an object of knowledge.

The influenza pandemic, much like global warming, is a concept with a global scale of reference. However, the concept is contested and sometimes confused, even among scientists

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<sup>25</sup> The classic examples are Wolf, *Europe and the people without history* (Berkeley, CA: University of California Press, 1982); Mintz, Sidney Wilfred. *Sweetness and Power: The Place of Sugar in Modern History*. New York, N.Y.: Viking, 1985.

<sup>26</sup> Stephen Collier, "Global assemblages" *Theory, Culture and Society* Vol. 23 (2006): 399-401. Cf. Stephen Collier and Aihwa Ong, *Global Assemblages: Technology, Politics and Ethics*; Giddens, "Living in a post-traditional society" in Beck, Lash, and Giddens, *Reflexive modernization: Politics, tradition and aesthetics in the modern social order* (Stanford, CA: Stanford University Press, 1994).

<sup>27</sup> George E. Marcus, "Ethnography In/of the World System: The Emergence of Multi Sited Ethnography." *Annual Review of Anthropology* 24 (1995): 95-117.

<sup>28</sup> Ong and Collier, *op. cit.*

<sup>29</sup> Paul N. Edwards, *A vast machine, op cit.*

<sup>30</sup> Lydia Liu, "The question of meaning-value in the political economy of the sign," in L. Liu, ed., *Tokens of exchange: The problem of translation in global circulation* (Durham, N.C.: Duke University Press, 1999): 37. Cf. Liu, *Translingual practice*

and health authorities.<sup>31</sup> As a recent review article authored by influenza experts at the National Institute for Allergies and Infectious Diseases points out, the concept of "pandemic" has been used to describe a wide range of disease events and is not clearly distinguishable from "epidemic." Among the range of historical usage "there seems to be only 1 [sic] invariable common denominator: widespread geographic extension."<sup>32</sup> The linguistic vagaries and multiple usages of the term suggests that understanding the concept of the influenza pandemic requires studying the techniques used to know and identify influenza pandemics.

Although the use of the term "influenza" dates back to the eighteenth century or earlier, the first event contemporaneously named as an "influenza pandemic" was an outbreak in 1889-90. Finkler's account, published eight years after the event, states that

" . . . this influenza epidemic broke forth from the East, and overwhelmed the world in a pandemic such as had never before been seen. The high flood of the pandemic flowed over the whole globe in a matter of months."

The texts of Finkler and others make clear how emerging conditions of communication--largely shaped by the network of colonial trade and troop movements<sup>33</sup>--allowed the disease to be followed as it moved from "Burkhara in Turkestan" across Siberia, to Greenland and then England and America. It was this ability to track a disease in its movement around the world that enabled the conceptual distinction of a pandemic from seasonal epidemic influenza, based on its global scale. The more famous 1918 influenza pandemic--alleged to have caused the deaths of over twenty million people around the world--broke out in the waning months of the Great War and has since become an exemplar of the flu pandemic. Its spread was monitored so effectively in part because of the "global infrastructure" of troop deployments and wartime concern about the vitality and health of soldiers.<sup>34</sup>

Despite occurring at the height of the bacteriological era, the monitoring of these epidemics relied on clinical diagnoses and their identification as pandemic was based on broad, descriptive inferences. The causal pathogen had yet to be identified.<sup>35</sup> During the 1889-90 outbreak, Pfeiffer claimed to have found a bacteria that he named *Haemophilia influnzae* in influenza patients. However, experimentation in animals persistently failed to achieve the typical signs of influenza, failing one of Koch's four postulates of specific causality.

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<sup>31</sup> The concept of the influenza pandemic most notoriously came into question during an epidemic of novel, swine-origin h1n1 influenza in 2011. Although WHO preparedness plans included a six stage guide for when to declare a pandemic, the WHO leaders refused to declare that phase 6--pandemic--had appeared. They refused because pandemic plans contained strict regulations for how to respond to a pandemic, including quarantine and travel restrictions, but the h1n1 virus caused very mild illness. Still, in the midst of this controversy, the fundamental "global" aspect of the pandemic was reaffirmed. WHO Assistant Director-General for Health Security and Environment Keiji Fukuda stated:

An easy way to think about pandemic – and actually a way I have some times described in the past – is to say: a pandemic is a global outbreak. Then you might ask yourself: “What is a global outbreak”? Global outbreak means that we see both spread of the agent – and in this case we see this new A(H1N1) virus to most parts of the world – and then we see disease activities in addition to the spread of the virus (26 May 2009, WHO Transcript).

<sup>32</sup> Morens DM, Folkers GK, Fauci AS. “What Is a Pandemic?” *The Journal of Infectious Diseases* 200, no. 7 (2009): 1018–21. Etymology dictionaries suggest that the noun form "pandemic" first appeared in 1853 in the context of the spread of cholera across the world.

<sup>33</sup> Marta Hansen, presentation at Berkeley, CA.

<sup>34</sup> John M. Barry, *The great influenza: The story of the deadliest pandemic in history* (New York: Penguin, 2005).

<sup>35</sup> Cf. Richard Krause, "The swine flu episode, 1976" in Ping-chung Leung, Paul A. Tambyah, eds., *Bird flu: a rising pandemic in Asia and beyond?* (Singapore: World Scientific, 2006).

As Carlo Caduff has shown, the identification and analysis of the etiologic agent of influenza--a virus--brought forth an entirely new ontological distinction between epidemic and pandemic.<sup>36</sup> The development of the hemagglutinin assay during the 1940s not only improved methods for detecting the virus, but also, unexpectedly, enabled the marking of distinctions *among* influenza viruses. The hemagglutinin assay works by exploiting a biological effect: a protein present in influenza viruses will cause blood from a fertilized chicken egg to clot. But as Hirst discovered during experiments in 1941-2, the clotting effect can be inhibited by the presence of antiserum, that is, blood containing antibodies extracted from an organism infected with influenza virus. More importantly, Hirst found that the inhibition is specific: antiserum will not inhibit clotting if it contains antibodies from infection from an influenza virus sufficiently distinct from the virus being studied. The researchers realized that they had experimentally discovered an immunological, or *antigenic*, relationship: the difference between viruses is reflected in the variable ability of antibodies to detect and destroy the virus.

Caduff argues that following this discovery, the antigenic difference between viruses was transposed onto the epidemiological difference between epidemic and pandemic. A pandemic was redefined as any influenza epidemic caused by a *novel* strain of influenza virus, novelty being defined by antigenic properties. Influenza, then, added its own specific supplements to the concept of pandemic. As William Ian Beveridge put it in 1977, the “word pandemic means simply a widespread epidemic, but in the case of influenza, virologists now restrict the term to mean a world-wide epidemic *caused by a new [...] influenza virus.*”<sup>37</sup>

In the aftermath of the Second World War, the Interim Commission of the World Health Organization attempted to apply these new laboratory techniques to monitor the epidemiology of influenza across the world.<sup>38</sup> In 1947, the Interim Commission established the World Influenza Center at the National Institute for Medical Research in London. In a contemporaneous article describing the formation of the WIC, Payne lists three facts known about influenza that influenced the Interim Commission’s influenza planning:

- (1) that successful vaccination against influenza depends on knowledge of the virus causing the epidemic;
- (2) that continuous vigilance is necessary to detect new and potentially dangerous strains of virus at the earliest possible moment; and
- (3) that epidemiological reports can be correctly interpreted only in terms of laboratory studies of the viruses responsible.<sup>39</sup>

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<sup>36</sup> Carlo Caduff, "Microbial Matters: An anthropology of pandemic influenza in the United States" Ph.D. Dissertation, UC Berkeley, 2009.

<sup>37</sup> William Ian Beveridge, *Influenza: The Last Great Plague: an unfinished story of discovery* (New York: Prodist, 1977).

<sup>38</sup> During the Second World War, the U.S. Army made the first strategic countermove in the dance between population immunity and the influenza virus, developing the first influenza vaccine and testing its efficacy during an epidemic in 1943. But in the first large-scale epidemic after the war, in 1947, the vaccine failed. In a pattern that would continue to recur down to the present day, researchers discovered that the existing distinctions among influenza types (A or B) were not refined enough, and named the new virus "A Prime". But they also determined that the new virus was first detected in Australia in 1946, appearing only much later in the United States. Scientists concluded that new viruses appeared in one place and may spread from there around the world, potentially providing a window of time to develop vaccines to protect at least some portions of the globe. See Goerge Dehner, *Influenza: A century of science and public health response* (Pittsburgh, Pa.: Pittsburgh University Press, 2012).

<sup>39</sup> A.M. Payne, "The influenza programme of WHO" *Bulletin of World Health Organization* Vol. 8 (1953): 755-774.

The Committee planned for the World Influenza Center to be the center of an international network of laboratories able to isolate and submit viral samples to London for analysis. As Kitler has described,

The Expert Committee envisaged that, by cooperating with an international network of laboratories, WHO could advise member states regarding control of influenza. WHO would also coordinate surveillance on the appearance and spread of influenza in order to accurately forecast the time and place of influenza epidemics. Furthermore, WHO felt that antigenic studies of prevalent viruses would permit the committee to recommend the strains of virus that should be included in the influenza vaccine and to recommend which vaccines in stock could be used.<sup>40</sup>

In an official report, the Expert Committee declared the World Influenza Center to be “an essential part of the world-wide public health defence against influenza.”<sup>41</sup> The WHO's surveillance network, by supplementing research goals with the attempts to “forecast” influenza epidemics and match emerging strains with vaccine prophylaxis, turned influenza into an exemplary project of the growing field of “international” health.<sup>42</sup> During the early years of the WHO, however, tensions emerged between the goals of international cooperation and national health programs. Indeed, these sometimes approached the challenges that had crippled earlier efforts to build international health governance through agreements on quarantine and the formation of the League of Nations Health Organization. In particular, the United States was developing its own partly autonomous international network of influenza surveillance through its military research bases around the world. As Robert Dehner's account of these early years demonstrates, the formation of the World Influenza Center and its surveillance network was one site where U.S. scientific aspirations struggled for and ultimately achieved dominance (through funding of research and the rise of the CDC) while the earlier networks of colonial tropical medicine weakened. It also shows that the ‘global view’ of worldwide surveillance networks is a view from somewhere: and in these formative years of world influenza surveillance, the contest was over whether that view was from London or from Bethesda, MD.<sup>43</sup>

By building this international network of laboratories, the WHO set two forms of globality against each other. On the one hand, the global scale of the influenza pandemic was now defined by an ontological difference in the virus, a difference that could only be identified in the laboratory. On the other hand, a worldwide network of laboratories (originally centered on London) tracked the distribution and appearance of these viral differences across a global space. These were not simple mirrors of each other: in other words, the globality of the pandemic concept did not in a simple sense *depend* on the global scope of the surveillance network. Rather, the two concepts of globality played against each other to produce a new narrative of

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40 M.E. Kitler, et al, “Influenza and the work of the World Health Organization” *Vaccine* Vol 20(Suppl. 2) 15 May 2002: S5-S15.

41 World Health Organization, “Expert Committee on Respiratory Diseases: First Report” *World Health Organization Technical Report Series No. 170*. Geneva: 1952.

42 I have explored elsewhere the role of Alexander Langmuir and his institute, the U.S. Centers for Disease Control, in marking a shift between two modes of “epidemiology”: one oriented towards research and management of health, which Langmuir described as “study section research grant” epidemiology; and a second oriented towards rapid detection of outbreaks, tracking of ongoing epidemics, and immediate control of infectious diseases, which Langmuir called “epidemic intelligence.”

43 See Carlo Caduff's book review, “Whose world? Which health? What security? The facts and fictions of global health security” *Biosocieties* Vol. 8, No. 1 (2013): 93-103.

how pandemics appeared and spread. The 1957 pandemic, one of the first to be monitored by this system, shows how a new understanding of China as the epicentre of pandemic influenza viruses was one important consequence of this doubled globality.

### *Pandemic Epicentre*

In 1957, a new variant of influenza spread around the world. Scientists quickly claimed the outbreak confirmed the association between antigenically novel viruses and pandemic events. During a previous pandemic in 1947, American scientists had realized that antigenically novel viruses could hypothetically be detected in their place of first appearance, weeks or even months before they arrived at American shores. The 1957 pandemic was the first time in which such tracking of the influenza virus across the globe was attempted in real-time. American observers first reported unusually severe outbreaks of influenza in Hong Kong and southern China. In May of 1957, following laboratory research conducted by the U.S. Army 406th Medical General Laboratory in Japan and Walter Reed Army Institute of Research, Dr. Maurice R. Hilleman reported that influenza viruses from the Hong Kong epidemic were "distinctly different from type A strains previously isolated from this country [the United States] and Europe."<sup>44</sup> In the U.S., Alexander Langmuir's "epidemic intelligence" system closely tracked the influenza virus from the moment it was introduced into the United States to its spread around the country.<sup>45</sup>

Along with claiming confirmation of the hypothesis that the antigenic novelty of viruses caused pandemics, the scientific accounts of the 1957 pandemic developed an understanding of influenza as an entity existing within a heterogeneous globe, in which the temporal development of a pandemic could be mapped onto geographic space. Most importantly, in a fateful identification, China came to stand for the source and origin of pandemic viruses. In the process of linking the globality of the pandemic to a global infrastructure of laboratory research and disease surveillance, the globe became heterogeneously marked: China became a source of viruses that caused worldwide harm. In this sense, the constitution of a global knowledge of the influenza pandemic was joined to a world view that narrated the appearance and temporal progression of influenza pandemics.

During the 1970s, Kennedy Shortridge, an Australian director of a Hong Kong microbiology laboratory, proposed the hypothesis that southern China was what he called an "epicentre" of influenza pandemics. In an article under the title "An influenza epicentre?" published following a 1982 conference that joined researchers from Hong Kong with their counterparts from mainland China (People's Republic of China), Shortridge (along with British virologist C.H. Stuart-Harris) wrote that existing evidence pointed to southern China as "the point of origin of pandemic viruses."<sup>46</sup> Shortridge and Stuart-Harris argued that the periodic appearance of influenza pandemics could be traced to the creation of new strains of influenza virus in southern China, mapping the qualitative distinction of new viruses and the temporality of pandemic appearance onto the geography of the globe.

Shortridge and Stuart-Harris draw on three kinds of evidence to support their claim. The first was the growing field of virology and early recombinant microbiological research. Robert Webster and Edwin Kilbourne, American virologists, had both shown that influenza viruses are

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<sup>44</sup> William H. Stewart, "Administrative history of the Asian influenza program" *Public Health Reports* Vol. 73, No. 2 (1958): 101-113.

<sup>45</sup> Langmuir, et al, "Asian influenza surveillance".

<sup>46</sup> K. F. Shortridge and C.H. Stuart-Harris, "An influenza epicentre?" *The Lancet* (9 October 1982): 812-813.

capable of being "genetically recombined" to produce new strains.<sup>47</sup> Initially accomplished in the petri dish, Webster reported in a 1971 paper the successful production of "new" (the quotation marks are his) influenza viruses *in vivo*, that is to say, inside the bodies of laboratory animal hosts. Webster infected animals (Young Tamworth pigs, Beltsville White turkeys, and White leghorn chickens) with multiple strains of influenza viruses and was able to experimentally demonstrate the isolation of antigenically distinct (or "new") influenza viruses following mixed infection. Webster goes on to suggest that animal hosts may serve as "mixing vessels" for avian, mammalian, and human influenza viruses *outside the laboratory*, and that these extra-lab "recombinants" could be the "new" influenza viruses that cause pandemics. In Webster's framing, an event first accomplished artificially in the lab is seen as a possible analogy for how influenza evolves outside of the lab.

Meanwhile, Shortridge and his Hong Kong laboratory isolated a vast diversity of influenza strains from poultry in southern China. During the late 1970s, Shortridge's lab conducted extensive surveillance on birds sourced from Hong Kong and from southern China (PRC). The lab found poultry in the region to be hosts for a wide variety of influenza viruses. In a 1982 paper, Shortridge remarks that he was able to isolate forty-six of the one hundred-eight antigenic combinations possible under existing scientific nomenclature. All but one of them were isolated from ducks.<sup>48</sup>

Descriptive and anecdotal accounts of the culture, agriculture and ecology of southern China form a third kind of evidence, less experimentally rigorous perhaps, but narratively important. Citing personal communications, ad-hoc interviews with travelers from southern China to Hong Kong, and the descriptions provided by conference participants from the P.R.C., Shortridge and Stuart-Harris paint a picture of a southern Chinese form of life:

"In the villages, it is common to see ducks, geese, and chickens running loose in proximity to pigs and water buffaloes and to see small children playing in the environment. The closeness between man and animals could provide an ecosystem for the interaction of their viruses."<sup>49</sup>

The three sources of evidence lead Shortridge and Stuart-Harris to draw two conclusions. First, pandemic viruses are produced by the "interchange of viruses between host species." Second, they argue that the form of life characteristic of southern China--and in particular, the "closeness between man and animals" that is common there--promotes this interchange, and therefore, promotes the production of pandemic viruses. Shortridge and Stuart-Harris conclude that "if there is an influenza epicentre in China, the likely area appears to be south of the Yangtze river."<sup>50</sup>

Conceiving of southern China as the epicenter of influenza pandemics enhanced the importance of Hong Kong--and Shortridge's lab there--within programs for the investigation and control of influenza pandemics. As Frédéric Keck has shown, the influenza epicenter hypothesis allowed Shortridge to transpose the temporal progression of pandemic appearance onto the

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<sup>47</sup> R.G. Webster, C.H. Campbell, and A. Granoff, "The *in vivo* production of "new" influenza A viruses, 1. Genetic recombination between avian and mammalian influenza viruses" *Virology* Vol. 44 (1971): 317-328; Kilbourne?

<sup>48</sup> K.F. Shortridge, "Avian influenza A viruses of southern China and Hong Kong: ecological aspects and implications for man" *Bulletin of the World Health Organization* Vol. 60, No. 1 (1982): 129-135.

<sup>49</sup> Shortridge and Stuart-Harris, *op cit*.

<sup>50</sup> *Ibid.*, 813.

"frontiers of the living."<sup>51</sup> Territorial borders, along with distinctions between species, could stand in for the qualitative shifts in the virus that marked the difference between epidemic and pandemic viruses. In this way, Shortridge turned his access to the viruses of southern China into a kind of living capital, a resource that could be worked over into the laboratory into scientific values. Much like Hong Kong's powerful currency traders, Shortridge worked with the tools of translation and exchange, turning southern China's poultry first into virus samples and then into taxonomic facts that circulated in global scientific networks such as WHO's Global Influenza Surveillance Network. But a different metaphor could also be used to describe their activity, a metaphor drawing from the discursive resources of the Cold War more than the age of global finance. The Hong Kong scientists themselves began to speak of their work as the construction of "sentinels," referring to both chickens as sentinels for human influenza and Hong Kong as a sentinel for global pandemics. The notion of sentinel, as Keck has written,

does not only designate the "alarm-raisers" capable of revealing the signs of danger in order to elaborate a general vigilance. It also describes a "collective at the frontiers of the living", tracking in one species of animal the infectious agents or toxins transmissible to other species.<sup>52</sup>

As Shortridge himself claimed, "By building a profile of influenza ecology over the years particularly as it applied to domestic animals in southern China, Hong Kong essentially functioned as a sentinel post for it and the wider region."<sup>53</sup>

### *Event or episode?*

Lesley Sims was chief veterinary officer for Hong Kong from 1993 until 2003. A stout, jovial Australian officer in the colonial government, Sims was kept on for a few years after the city returned to Chinese sovereignty. In early May of 1997, Sims investigated an outbreak of disease on one of the relatively few (around two hundred) chicken farms in the "New Territories" of northern Hong Kong. Mortality was rapid and extensive, killing almost every single one of the farm's two thousand birds. The epizootic spread to two neighboring farms with similar high mortality. Back in his laboratory, Sims and his colleagues isolated the antigenic strain of influenza, which they classified taxonomically according to two proteins on the virus: hemagglutinin protein 5 and neuraminidase protein 1, or H5N1. Employing an OIE standard pathogenicity test, Sims determined that the new virus was "highly pathogenic", "with most birds dying within 24 hours of inoculation."<sup>54</sup>

Influenza experts distinguish avian influenza viruses into two broad classes based on pathogenicity, dividing viruses, in their colloquial language, into low and high "path". High pathogenicity is defined quite precisely by its effects on chickens. " Any influenza virus that is lethal for six, seven or eight of eight 4- to 8-week- old susceptible chickens within 10 days

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<sup>51</sup> Frédéric Keck, "Une sentinelle sanitaire aux frontières du vivant. Les experts de la grippe aviaire à Hong Kong" *Terrain* No. 54 (2010): 27-41. Cf. Keck, *Un Monde Grippe*.

<sup>52</sup> Keck, *op cit*.

<sup>53</sup> K.F. Shortridge, "Avian influenza viruses in Hong Kong: zoonotic considerations" Wageningen UR Frontis Series 8: 9.

<sup>54</sup> L. D. Sims, T. M. Ellis, K. K. Liu, K. Dyrting, H. Wong, M. Peiris, Y. Guan, and K. F. Shortridge, " Avian Influenza in Hong Kong 1997–2002" *Avian Diseases* Vol. 47, No. s3 (September 2003): 832-838.



following intravenous inoculation with 0.2 ml of a 1/10 dilution of a bacteria-free, infective allantoic fluid" is classified as highly pathogenic.<sup>55</sup>

I met Sims in Beijing in 2011 where he was attending an influenza conference. Retired from Hong Kong and moved back to Australia, he still circulated as a self-described "consultant" and worked on a number of specific contract projects for the Food and Agriculture Organization. He told me that back in the 1990s, "there were always new diseases popping up." When he identified the virus as influenza, he was not especially worried or even surprised. He already knew there was avian influenza circulating in the region. Besides, avian influenza viruses were considered to be a possible reservoir for pandemic flu, but not a direct threat to human health. In August 1997, everything changed when Shortridge's lab isolated H5N1 influenza virus from a three year-old boy who had died in May. As Sims, Shortridge, and co-authors later reported, "the transmission of this virus to man has fundamentally changed our concepts and views of HPAI."<sup>56</sup> The direct transmission obviated the need for an intermediary between birds and humans, Webster's so-called "mixing vessel." It was this transmission event that led J.C. de Jong, Robert Webster and others to connect the transmission event and the antigenic novelty of the virus with the oft-told history of pandemics, suggesting that the virus could be the source of the "next pandemic."<sup>57</sup> From late fall into winter of 1997, as media attention increased and the unrelated Asian financial crisis deepened, additional human cases appeared in humans and virus was detected in poultry markets. In late December, Sims received an order: "Kill all the poultry in the city in twenty-four hours." The slaughter, or in the veterinary parlance, 'culling' of all poultry in every market and on every farm in Hong Kong would remove the virus from poultry and human populations and allow Sims to begin directing the reconstruction of "biosecure" poultry farms. The 1997 outbreak had, it seemed, confirmed the Shortridge hypothesis and his conception of Hong Kong as a sentinel post for pandemic viruses.<sup>58</sup>

Over the next few years, Hong Kong laboratories continued to isolate new viruses --from fecal swabs of waterfowl cages in 1999 and 2000; at retail poultry markets in 2001; in farms in 2002. In 2003, the isolation of the SARS coronavirus and the identification of the civet cat as its zoonotic reservoir by members of Shortridge's lab additionally confirmed the "sentinel" capacities of Hong Kong's microbiology infrastructure. Indeed, SARS patients were first

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<sup>55</sup> According to the OIE Manual of Standards for Diagnostic Tests and Vaccines 2000. The OIE manual adds two qualifications:

b) The following additional test is required if the isolate kills from one to five chickens but is not of the H5 or H7 subtype: growth of the virus in cell culture with cytopathogenic effect or plaque formation in the absence of trypsin. If no growth is observed, the isolate is considered not to be a HPAI isolate.

c) For all H5 and H7 viruses of low pathogenicity and for other viruses, if growth is observed in cell culture without trypsin, the amino acid sequence of the connecting peptide of the haemagglutinin must be determined. If the sequence is similar to that observed for other HPAI isolates, the isolate being tested will be considered to be highly pathogenic.

From Alexander, D.J., 2000. Highly pathogenic avian influenza. *In: OIE manual of standards for diagnostic tests and vaccines*. 4th ed. World Organization for Animal Health OIE, Paris, 212-220. See also the slightly different formulation in D.E. Swayne and D.L. Suarez, "Highly pathogenic avian influenza" *Rev. sci. tech. Off. int. Epiz.*, 2000,19 (2), 463-482.

<sup>56</sup> L.D. Sims, et al, *op cit*.

<sup>57</sup> JC de Jong, EC Claas, AS Osterhaus, RG Webster, WL Lim. "A pandemic warning?" *Nature* Vol. 389, No. 6651 (Oct 9 1997): 554-554.

<sup>58</sup> Indeed, Shortridge had published an article entitled "The next pandemic?" in 1995, two years before the virus appeared in Hong Kong, which reiterates his hypothesis that the virus will emerge from the "epicentre" of southern China. de Jong and Webster's 1997 article "The next pandemic" seems literally to be an affirmation of Shortridge's speculations.

suspected of suffering from influenza, and as Keck argues, the coronavirus was investigated in the mode of the influenza sentinel and later considered as a trial exercise for a possible flu pandemic.<sup>59</sup>

Otherwise, however, not much was made of highly pathogenic avian influenza H5N1 during those years. No outbreaks were reported anywhere outside of Hong Kong, including China, although some Hong Kong scientists continue to believe that the virus was actively circulating throughout that period. For some, it seemed that the storm had passed. The Food and Agriculture Organization, for example, had still yet to develop any specific flu preparedness or response programs, and considered rinderpest to be the primary "transboundary animal disease" of concern. From a certain perspective today, these years appear as the eye of a hurricane, a few moments of peace while the clouds hurtle closer. Then again, as time has passed without any pandemic appearing, one could also narrate the moment of pause as a much needed period of calm sanity amidst hyperbolic fears. The "Swine Flu Affair" of 1976, in which the U.S. government initiated widespread vaccination for a novel strain of influenza that simply never caused illness in the United States, as well as the more recent mild pandemic of 2009, demonstrate the deep uncertainty about the significance and danger of flu, even among influenza experts. Richard Krause, the former director of the National Institutes for Allergies and Infectious Diseases who oversaw the 1976 vaccination preparations has described "the Fog of epidemics" caused by "the uncertainty that surrounds any response to a microbial outbreak."<sup>60</sup> Yet the challenge of distinguishing the significant event from the over-hyped episode is ultimately as much a question of scale, value and narrative as it is a quality of microbial dynamics.<sup>61</sup>

In late 2003, the highly pathogenic influenza virus H5N1 reappeared--although molecularly in a slightly different form.<sup>62</sup> For the narrative of this dissertation, the reappearance in 2003 is a significant event--precisely because it began the process of moving things beyond Shortridge's hypothetical framework of influenza emergence. Hong Kong once again first discovered human infections from H5N1, when a grandfather and grandson who had recently traveled to Fujian province fell ill upon return. And in January 2004, large outbreaks broke out across the poultry operations in Vietnam and Thailand, ravaging the large-scale operations of smallholders and industrial poultry giants alike. In Spring of 2005, the largest outbreak of avian influenza took place on the remote Qinghai Lake in northwestern China. Finally, in winter of 2005 dead migratory birds were found across Europe, killed by H5N1 virus. Robert Wallace, a "phylogeographer" involved in the avian flu research that forms the topic of this dissertation, once pointed out to me that the relationship between a virus and its geography is not determinative but dynamic. As a virus spreads through populations and across space, he argued, its ontology changes in the process, including transformed capacities of transmissibility and pathogenicity and different possibilities of control. "Once the level of coordination changes,

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<sup>59</sup> See Keck, "Karl Taro Greenfeld, China Syndrome. The True Story of the 21st Century's First Great Epidemic; Thomas Abraham, Twenty-First Plague. The Story of SARS." Book review, *China Perspectives* No. 4 (2007); see Christine Loh and Civic Exchange, eds, *At the Epicentre: Hong Kong and the SARS Outbreak* for one example of how SARS is refigured as an exercise in pandemic preparedness.

<sup>60</sup> Krause, *op cit*.

<sup>61</sup> Paul Rabinow, "Episodes or Incidents: Seeking Significance" in Collier and Lakoff, eds., *Biosecurity Interventions: Global Health and Security in Crisis*.

<sup>62</sup> Peiris JS, Yu WC, Leung CW, Cheung CY, Ng WF, Nicholls JM, Ng TK, Chan KH, Lai ST, Lim WL, Yuen KY, Guan Y., " Re-emergence of fatal human influenza A subtype H5N1 disease" *Lancet* Vol. 346, No. 9409 (2004 Feb 21): 617-619.

spatially," he said, "The nature of the problem changes." Paul Rabinow has used the term "zones of virulence" to describe these dynamic relationships between a virus and its environments. As Rabinow points out, these zones cannot be distinguished from the *practices*, including knowledge practices, that actively reconstruct dynamics of virus, host and environment. As the virus reconstitutes its relation to the environment, the knowledge practices devoted towards understanding and controlling the viral epidemic are also put in motion.<sup>63</sup>

When highly pathogenic avian influenza H5N1 spread out of southern China, many more organizations and actors became involved in the loosely coordinated project to prevent an influenza pandemic. This dissertation argues that as the epidemic transformed itself through its spatial extension, and when the number of actors and approaches involved in trying to understand and control the epidemic grew, a series of distinctions--epistemological and social, political and practical--appeared in the global science of pandemic influenza. The WHO's global surveillance of influenza, I have argued, was built upon a network of virology laboratories, making the pandemic in scientific terms a product of the laboratory. In Hong Kong, Kennedy Shortridge had set himself and his laboratory as a sentinel monitoring the "frontier" of the epicentre. Now, some scientists began to call into question the centrality of the laboratory in the identification and analysis of influenza pandemics, and even to displace the dominance of the "virus" as the object upon which influenza knowledge is made. These scientists began to orient research towards understanding the epicentre itself and developed research projects that would bring them into the epicentre. In doing so, the science of influenza was moved onto China's grounds. This dissertation is about this movement of science and scientists outside of the laboratory, onto China's grounds and into the influenza epicentre. It is also about how the relations between truth and ethics, between knowledge and social order, and between China and the world were changed in the process.

### *Beyond the laboratory*

The anthropology and sociology of science has relied heavily on fieldwork inside laboratories. Indeed, the "practice turn" in science studies could largely be captured by the metonym of the laboratory. For according to these works it is in the laboratory, broadly or narrowly construed, that science is done and scientific knowledge is made.<sup>64</sup> Although anthropologists and sociologists of science have long acknowledge the existence of scientific activities outside of the laboratory, they have often described their scientific validity as depending on the supports of the lab, or studied them in analytic terms adopted from the study of laboratories. Bruno Latour, most famously, claimed that there is no outside to the lab. Scientists traveling to the field must make sure not to leave the laboratory behind:

"The only way for a scientist to retain the strength gained inside his laboratory by the process I have described is not to go outside where he would lose it at once. It is again very simple. The solution is in never going out. Does that mean that they are stuck in the few places where they

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<sup>63</sup> Paul Rabinow, "The biological modern" *ARC Working paper*.

<sup>64</sup> Bruno Latour and Steve Woolgar's *Laboratory Life* is the classic ethnography of the genre, followed closely by Karen Knorr-Cetina's *The Manufacture of Knowledge* and Sharon Traweek's more anthropological *Beamtimes and Lifetimes*. The history of science tends to locate its material more broadly, often because in the periods under study there were no laboratories in the current sense. Shapin and Schaffer's *Leviathan and the Air-Pump* provides a history of the emergence of the experimental laboratory. But the lack of what we would understand to be laboratories is no protection against the analytic tendency to extend the model of the laboratory to all kinds of scientific activity.

work? No. It means that they will do everything they can to extend to every setting some of the conditions that make possible the reproduction of favorable laboratory practices."<sup>65</sup>

Karen Knorr-Cetina delinks the concept of laboratory from its everyday association with the house where experiments take place. Instead, she argues that laboratory practice includes any practice that "entails the detachment of objects from their natural environment and their installation in a new phenomenal field defined by social agents."<sup>66</sup> Ian Hacking, on the other hand, defines the laboratory far more narrowly: a laboratory, for him, involves the "study of phenomena that seldom or never occur in a pure state before people have brought them under surveillance."<sup>67</sup> Hacking is basically referring to the importance of manipulation and intervention--even the creation of new realities--as the defining feature of scientific knowledge.<sup>68</sup>

But these analytic framings of scientific activity do not help to understand the activity of scientists who explicitly reject the laboratory model of knowledge production. Historians have recently begun to examine the particular conditions of the field, rather than laboratory, sciences. Jeremy Vetter writes that:

"In its broadest meaning, the field can be anywhere outside the laboratory where scientists have worked. While laboratories have aimed to produce universal, placeless knowledge, field sites have produced knowledge that is based in place. Historically, as rhetorically placeless laboratories ascended to their high epistemic status in modern science by the late nineteenth century, "the field" was simultaneously reconstructed as the residuum of messy, complex, and uncontrollable nature."<sup>69</sup>

As Vetter's historical synopsis makes clear, these studies include within their purview the *relationship* of the field to the laboratory. Robert Kohler, one of the leaders of this sub-field, emphasizes the dynamic relationship between field and lab.<sup>70</sup> But the relationship is not a neutral or balanced one. As the laboratory has risen to epistemological authority, the field has become a site of "messy nature." And the scientists who promote field-based knowledge practices sometimes do so with an explicit resentment towards the lab, a visceral affect nothing like the distanced impersonalism that, rightly or wrongly, is often associated with the pursuit of scientific truth.

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<sup>65</sup> Bruno Latour, "Give me a laboratory and I will raise the world!" In: Mario Biagioli, ed., *The Science Studies Reader* (New York: Routledge, 1999).

<sup>66</sup> Karen Knorr-Cetina, *Epistemic Cultures: How the sciences make knowledge* (Cambridge, Ma.: Harvard University Press, 1999).

<sup>67</sup> Ian Hacking, "The self-vindication of the laboratory sciences" in Andrew Pickering, ed., *Science and Practice and Culture* (Chicago: University of Chicago Press, 1992).

<sup>68</sup> Hacking, *Representing and Intervening: Introductory Topics in the Philosophy of Natural Science* (Cambridge: Cambridge University Press, 1983).

<sup>69</sup> Jeremy Vetter, "Introduction" in Vetter, ed., *Knowing Global Environments: New historical perspectives on the field sciences* (New Brunswick, NJ: Rutgers University Press, 2010). On the history of the field sciences, see also the essays collected in Henricka Kuklick and Robert Kohler, eds., *Science in the Field, Osiris*, 2nd Series, Vol. 11 (1996); Daniel M. Schneider, "Local knowledge, environmental politics, and the founding of ecology in the United States: Stephen Forbes and 'The Lake as Microcosm' (1887)" *Isis* Vol. 91, No. 4 (Dec 2000): 681-705.

<sup>70</sup> Robert Kohler, *Landscapes and Labscapes: Exploring the lab-field border in biology* (Chicago: University of Chicago Press, 2002).

In *Leviathan and the air-pump*, Steven Shapin and Simon Schaffer explore the origin of the modern laboratory and the 'experiment' as mode of truth-making.<sup>71</sup> They argue that Boyle's experimental laboratory simultaneously reconfigures the forms of social order and scientific knowledge: they argue that "solutions to the problem of knowledge are embedded within practical solutions to the problem of social order, and that different practical solutions to the problem of social order encapsulate contrasting practical solutions to the problem of knowledge."<sup>72</sup> In the following, I aim to elucidate how scientists working on influenza in China reconstituted the relationships of social order and scientific knowledge as they moved from the lab to the field, and traveled onto China's grounds. What are the venues in which truth about avian influenza is being made, if not the laboratory? What form does this truth-making and truth-speaking take, if not the experiment? And why does it matter that China is the "grounds" of this truth practice?

The influenza scientists who moved into the epicentre themselves raised questions and concerns about the social organization of knowledge-making. They often argued that the nature of the avian influenza epidemic--the transmission of viruses from wild birds to domestic birds, or from poultry directly to humans, deeply challenged the existing modes of classifying scientific knowledge and the related forms of organizing scientific activity. In particular, modern governmental distinctions of wild from domestic animal life, or human from animal health, had become enshrined in the organizational structure of administrative bureaucracies in ways that hindered the investigation of the bird flu. In making these critiques, these scientists drew on the language of a recently popularized discourse known as "One Health." Yu-ju Chien and Ben Hickler have documented how the avian influenza epidemic problematized the organizational frameworks of international health agencies such as the World Health Organization, the Food and Agriculture Organization, and the Organization International de Epizootiques (OIE).<sup>73</sup> They also show how this problematization was worked over into new forms of inter-agency collaboration around the model of "One Health," arguably forming the basis for a new regime of global health governance.<sup>74</sup>

The term "One World, One Health" was first used as the theme of a September 2004 meeting of the Wildlife Conservation Society (WCS) in New York City. The meeting was held in response to the perception of increasing emerging disease threats, and meeting organizers used

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<sup>71</sup> Shapin and Schaffer, *Leviathan and the air-pump: Hobbes, Boyle, and the experimental life* (Princeton: Princeton University Press, 1989).

<sup>72</sup> *Ibid.*: 15.

<sup>73</sup> Yu-ju Chien, "How did international agencies perceive the avian influenza problem? The adoption and manufacture of the 'One World, One Health' framework" *Sociology of Health and Illness* Vol. 35, No. 2 (2013): 213-226. See also Chien, "Constructing knowledge and policies on avian influenza: How do international agencies craft global models?" Ph.D. Dissertation, University of Minnesota (May 2013). Benjamin Hickler, "Epidemic oversight: Emerging infections and rural livelihoods in the Mekong." Ph.D. Dissertation, University of California, San Francisco (2010).

<sup>74</sup> On international health, see: George Foster, "Applied Anthropology and International Health: Retrospect and Prospect" *Human Organization* Vol. 41, No. 3 (1982): 189-197; Foster, "Bureaucratic Aspects of International Health Agencies" *Soc.Sci & Med.* Vol. 25, No. 9 (1987):1039-1048; Foster, "World Health Organization and Behavioral Science Research: Problems and Prospects" *SS&M* Vol. 24, No. 9 (1987): 709-717. On global health, see: Craig R. Janes and Kitty Corbett, "Anthropology and global health" in Byron J. Good, Michael M.J. Fischer, Sarah S. Willen, and Mary-Jo Delvecchio Good, eds., *A reader in medical anthropology: Theoretical trajectories, emergent realities* (Malden, Ma: Wiley Blackwell, 2010); Andrew Lakoff, "Two regimes of global health" *Humanity: An international journal of human rights* Vol. 1, No. 1 (2010): 59-79; Joao Biehl and Adriana Petryna, eds., *When people come first: Critical studies in global health* (Princeton: Princeton University Press, 2013).

avian influenza, Ebola, and chronic wasting disease as case studies framing a contemporary problem. However, unlike previous framings of disease emergence, the WCS symposium drew attention to the fact that these diseases threatened not only human health, but more broadly the "health of life on Earth." The call for "One Health" grew out of a recognition that disease-causing pathogens are shared among human, domestic animal, and wildlife populations. The essential corollary is the demand for new forms of scientific and governmental collaboration, capable of sharing and coordinating expert knowledge about each of these populations into a single "One Health" framework.<sup>75</sup>

The WHO, the FAO, and OIE initiated early attempts at collaboration around avian influenza in 2005, when they established the GLEWS, OFFLU, and CMC-AH mechanisms for disease surveillance and exchange of scientific information or biological materials. The term One World, One Health was adopted in the "road map" for a ministerial conference in 2007. One year later, WHO, FAO, OIE, along with UNICEF, UNSIC and the World Bank issued a joint policy document entitled "Contributing to 'One World, One Health' strategic framework."<sup>76</sup> However, as Chien argues, despite the consensus on the need for collaboration among the agencies, diverse interpretations have been given for what exactly One Health is or should be. As one FAO official noted at the 2011 One Health Congress in Australia,

"One Health means different things to different people. If you ask 10 people here, you may get 10 different ideas. We may not eventually obtain an agreement on One Health in this room. However, all of us believe that it is important . . . . During the next three days, we will discuss and conceptualize One Health in order to put our words to practice."<sup>77</sup>

Chien argues that this vagueness was dynamic and productive, enabling new forms of inter-agency organization to supersede conflicts over administrative territory and epistemological or governmental norms. At the same time, it threatened to become merely "ceremonial."<sup>78</sup> In my own fieldwork, FAO experts often made arguments very similar to Chien's. On the one hand, they lamented the emptiness of "rhetorical" usage of the term One Health, while on the other hand they promoted many of their favorite projects in the language and terminology of One Health. In one of my first meetings with Vincent Martin, director of the FAO ECTAD office, he described a presentation he would give to a United Nations working group about "One World, One Health." "This is sort of a cliché," he said, "and you get sick of people saying it all the time, but there is something to it after all, and so I want to say what I think it means."

Anthropological studies of international or global health often rely on a conceptual opposition: global health institutions bring standards and homogeneity, which are contested or troubled or exceeded by the local lives or bodies or problems. One Health could appear to be yet another one of these global protocols, developed in Manhattan and subsequently implemented or applied in China. However, the vague Panglossian hopes of One Health frameworks did not

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<sup>75</sup> The conclusions of the 2005 WCS meeting were enshrined in a statement that came to be known as the "Manhattan principles." See: Wildlife Conservation Society, "One World, One Health™" at <http://www.wcs.org/conservation-challenges/wildlife-health/wildlife-humans-and-livestock/one-world-one-health.aspx>. Accessed on: October 15, 2013.

<sup>76</sup> FAO, OIE, WHO, UNSIC, UNICEF, The World Bank, "Contributing to One World, One Health: A Strategic Framework for Reducing Risks of Infectious Diseases at the Animal–Human–Ecosystems Interface" (14 October 2008).

<sup>77</sup> Quoted in Chien, "How did international agencies?": 220.

<sup>78</sup> *Ibid.*: 223.

overdetermine the actual practices of building new forms of organizing scientific activity. Although One Health was an important resource for justifying the move beyond the microbiological scale and into the influenza epicentre, the forms and practices that resulted retained variability. In this dissertation, I retain the complexity of this multiplicity by focusing on two venues for conducting One Health research in China: a *Center* and a *Workshop*.

### *First Venue: A Center*

In 2007, the United Nations Food and Agriculture Organization sent Vincent Martin, a French veterinarian, to Beijing to establish a center of research and diplomacy for the control of avian influenza. Martin was an officer within the recently established section of FAO known as the Emergency Center for Transboundary Animal Diseases, and avian flu had just been brought to the forefront of their work. When avian flu had first appeared in 1997, FAO did not treat the disease as a high priority, and it is hardly mentioned in the documents of the Emergency Prevention System (EMPRES) for Transboundary Animal and Plant Pests and Diseases from that year. At the time, rinderpest eradication was the central agenda. Martin himself had most recently worked in Africa on the last stages of the rinderpest eradication program, which was certified in 2011, well after he had moved on to bird flu. It was only in 2004 when influenza H5N1 “re-emerged,” nearly simultaneously, in numerous southeast Asian countries, that FAO acted. Martin told me:

“We are an old, very slow organization! But avian flu really forced us to change some things. So we created new structures, offices called Emergency Control of Transboundary Animal Diseases which were rapidly deployed: this one in Beijing, one in Bangkok, one in Rome, one in Africa. New kinds of collaboration were begun.”

The regional and country-level ECTAD offices are funded almost entirely by donor countries, not by FAO's internal funds or by the host countries. In the case of the China ECTAD office, nearly all of its funding comes from the United States Agency for International Development. Ben Hickler, in his dissertation on the FAO's avian influenza control programs in southeast Asia, argues that FAO officials saw the threat of an avian influenza pandemic as an opportunity to attract novel streams of funding from donor countries for “pro-poor” livestock development initiatives. According to an interview he conducted with Subhash Morzaria, ECTAD Regional Manager for Asia and the Pacific, the 2004 re-emergence of the virus in southeast Asia was not enough to attract funding;

“It was only when it spilled over from Asia and it went into Russia and into Europe and suddenly countries started getting panicky and said, “On no, it's going to come and hit us!” Then the funding started rolling in.”<sup>79</sup>

The ECTAD office in Beijing was established under an FAO function entitled Technical Cooperation Programme (TCP). According to the TCP Manual, “The Technical Cooperation Programme (TCP) was launched in 1976 and is a part of FAO's Regular Programme, financed from the assessed contributions of its Members. The Programme aims to provide FAO's technical expertise to its Member countries through targeted, short term,

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<sup>79</sup> Quoted in Hickler, "Epidemic Oversight": 179.

catalytic projects. These projects address technical problems in the field of agriculture, fisheries, forestry and rural livelihood that prevent Member countries, either individually or collectively, from implementing their development programmes. . . . TCP projects aim to fill critical technical gaps by providing technical inputs that are not available locally, or that project beneficiaries cannot access through their own means, or through local support systems.”<sup>80</sup>

The establishment of the FAO ECTAD office furthers a shift in the FAO from a modernization and development program towards mechanisms of emergency response. The FAO was founded along with the UN in the aftermath of the Second World War. The FAO's primary mandate was in the provision of scientific and technical assistance to transform agricultural and food problems in the developing world. Although food aid distribution was also managed by FAO, this was always seen as a "transitional strategy" toward the long-term goal of "self-sustaining growth of the less developed countries to the point where they find it possible to meet their food requirements."<sup>81</sup>

However, during the 1970s a series of famines in Africa begin to shift the FAO's work towards humanitarian relief. When donor countries restricted financing in the 1980s, FAO Director James Ingram restructured the World Food Program as an emergency response food aid program. The ECTAD offices, and more broadly the response to avian influenza, mark a further mutation of the ideas of development and emergency towards problems of *preparedness* rather than humanitarian relief. Akira Iriye has argued that international organizations, including international governmental agencies such as FAO, form the institutional basis for building "global consciousness" and the "global idea." It is important to add, however, that there are many global ideas: FAO's shifting priorities exemplify the distinction drawn by Andrew Lakoff between "two regimes of global health": humanitarian biomedicine, focused on providing medical care for the "neglected diseases that affect poor countries"; and global health security, focused on preparing for emerging diseases that "threaten wealthy countries".<sup>82</sup> The FAO ECTAD office also shows that these two regimes do not exist in abstract opposition, but are rather distinct principles of justification sometimes brought into contingent settlements. The control of bird flu in Asia could be both an effort to modernize Asian agricultural systems, and at the same time to prepare for or prevent the next pandemic.<sup>83</sup>

### *Second Venue: A Workshop*

Perhaps the breadth and length of the workshop's name--"The 3rd International Workshop on Community-based Data Synthesis, Analysis and Modeling of Highly Pathogenic Avian Influenza H5N1 in Asia"--was an attempt to somehow specify a common object despite the diversity of its participants. Some studied viruses under microscopes in laboratories, others studied the pathologies of chickens on farms; some studied wild birds as they migrated by satellite, others studied numbers with models housed on computers. Still others examined what was referred to as the "socio-economies" of livestock production and marketing. They came

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80 FAO, *TCP Manual: Managing the decentralized Technical Cooperation Programme*, June 2009: 5.

81 Olav Stokke, *The UN and Development: from aid to cooperation* (South Bend: Indiana University Press, 2009).

82 Andrew Lakoff, "Two regimes of global health" *Humanity: An international journal of human rights* Vol. 1, No. 1 (2010): 59-79.

83 A critique of this "pro-poor"/preparedness synthesis has been made by researchers associated with the STEPS Centre. See, for example, Paul Forster, *The political economy of avian influenza in Asia*, STEPS Working Paper 17 (Brighton: STEPS Centre, 2009).



from the United States Geological Service, the United Nations Food and Agriculture Organization, and China's Ministry of Agriculture, State Forestry Administration, and Academy of Sciences; they arrived from universities in Southeast Asia, Europe or the United States.

These workshops (there have now been a total of five) are the gathering places for a diverse, but relatively consistent group of researchers investigating highly pathogenic avian influenza (H5N1) "in Asia." That is to say, they are concerned with understanding avian influenza *in situ*, in its site and situation, rather than in a petri dish or in the bodies of laboratory animals. As phylogeographer and Workshop participant Robert Wallace told me, they share the goal of "embedding molecular processes in the larger ecology." Participants in the workshops conducted their own research, often in collaboration with other participants. However, the workshop was not an organizational body for directly planning or directing these research endeavors (in the manner of a university laboratory). I attended the third workshop, where I also presented a paper, and I worked with a number of the participants in various other settings, sometimes collaboratively but more often adjacently.

The group first gathered under the banner of the "Workshop on Community-based Data Synthesis" in 2008 in Bangkok, Thailand. At the time, the prime movers of the group included Xiangming Xiao, a Chinese-born Professor at the University of Oklahoma who specialized in remote sensing and geospatial mapping, and Marius Gilbert, a Belgian ecological modeler. Gilbert had worked on modeling the 2004 avian influenza outbreaks in Thailand and had recently sought the assistance of Xiao. Others at the first meeting who will figure in the dissertation include Scott Newman, a veterinarian and wild bird migrations specialist employed by the FAO; Robert Wallace, the Marxist phylogeographer; and Vincent Martin, the French livestock veterinarian from FAO ECTAD-China. At the first meeting, according to Wallace, the room was laid out in a square of four long tables, marking a contrast with the conference-style of podium and arranged chairs of most avian influenza meetings (including the later incarnations of the Workshop as well). The dynamic of the unfamiliar arranged in an architecture of familiarity had produced questioning conversations and unconventional ideas.

The participants clearly shared an antipathy to microbiological reductions of the epidemic. Wallace recollects that the 1st workshop was a "meeting of the minds" because "everyone was looking for something else."<sup>84</sup> But if they shared a view of what was wrong with most research on avian influenza, what exactly they were building together was less certain. Participants spoke of "the group," "those workshops" or sometimes "the NIH group," referencing the U.S. National Institutes of Health who was the primary source of funding for the workshops. I found that the vagueness of appellations barely concealed their concern about the existential nature of the group, despite the common popularity of frameworks such as "One Health" among participants. At the end of the 3rd workshop, held at the Beijing Landmark hotel near the Ministry of Agriculture, the participants held an open discussion about possible collaborations and future directions.

"What are we?" Scott Newman, wildlife specialist, asked bluntly.

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<sup>84</sup> "This is so far beyond what most influenza research is about, most of it is molecular based, vaccine based, experiments in either poultry or, notoriously, in ferrets, and the idea of embedding that molecular process in a greater, *history*, in a greater geography or ecology, is largely absent. And so, it was a meeting of the minds, that first Bangkok workshop, because even though people there, agriculture folks, social folks, [...] everyone at that point were looking for something else."

Xiangming Xiao suggested the organization of the research group could be described as a "Federation." A French woman, a veterinarian working in Vietnam, proposed that the Federation could, as one of its prerogatives, issue expert recommendations after each workshop meeting. For example, she added that the group could stipulate the state of knowledge about avian influenza and recommend how policy should accord with it. But Newman wondered aloud how a "Federation" could make recommendations. He argued that most people came to work on avian influenza to meet objectives of their own, or those of their home institutions. In the end, "it just happens that everyone is in the same room here."

Marius Gilbert, the ecological and geo-spatial risk modeler and one of the founders of the group argued that it was the *object* that brought people together. Everyone was working on the same thing, he said: understanding transmission, virulence, the origins of the virus . . .

Newman interrupted him, skeptically: "OK, but you said about four different things there."

### *Mapping the Influenza Epicentre*

*"Now when I was a little chap I had a passion for maps. I would look for hours at South America, or Africa, or Australia, and lose myself in all the glories of exploration. At that time there were many blank spaces on the earth, and when I saw one that looked particularly inviting on a map (but they all look that) I would put my finger on it and say, 'When I grow up I will go there.'"*

-Joseph Conrad, *Heart of Darkness*

The offices of the FAO Emergency Center for Transboundary Animal Diseases are on the fifteenth floor of a building in the diplomatic district of Beijing, just east of the old central city. As Vincent Martin finished a heated conversation with a member of his staff over some pending "approvals", I stood and looked out the window of his office. The back two walls behind Vincent's desk were sheer glass, affording a view across and over the diplomatic district and much of the historic city of Beijing. The grid-work avenues and narrow *hutong* alleys all looked so orderly and regular from up here. The sounds of the honking horns and traffic hardly reached me. There is a small hill behind the Forbidden City, formerly part of the imperial grounds. Because it was forbidden for anyone to look at the imperial palace from a viewpoint higher than the emperor's, that hill was once the highest point in Beijing. Today, I reflected, the structures of an "international" modernity--high rise office buildings--present the most transcendent views.

Vincent returned to his office carrying two paper cups with espresso, "*the best espresso in Beijing*," and three map print-outs, each showing a portion of China near the Yangtze river. Sitting on a long couch, I pored over the maps, which were mottled in color from blue to deep red, signifying a variable distribution of risk for avian influenza outbreaks. "Look here, just to the south of Wuhan, its all red. And here," Vincent pointed to a small dot on the map, deep in the center of the red zone, "this is where a human case was reported last year." These maps were made to represent the results of a study that Vincent was preparing for publication; but he also used them to identify locations of potential significance for further monitoring and research. Always hoping to build interdisciplinary collaborations, he suggested to me that anthropological research at the heart of the reddest portion of the map could provide important findings about "poultry market chains" involved in the circulation of viruses. Unlike the blank spaces on the

map that fascinated the colonial imagination of Conrad's narrator, it was not a lack but a dangerous surplus that formed the attractiveness of these red zones, posing distinctive problems of knowledge, ethics, and politics.

The maps are an important product of the effort to turn attention to the ecologies of the influenza epicenter. The concept of the influenza epicentre developed by Kennedy Shortridge always contained reference to the environments, cultures, agricultural practices, and ecologies of southern China. However, these references took the form of anecdote, description, or citation. In this sense, the environments remained external to the scientific object: the virus in the laboratory. Following the 2004 spread of avian influenza into southeast Asia, scientists began to turn these environments into scientific objects in their own right.

For Shortridge and his Hong Kong lab, the influenza pandemic can be understood as the product of a quality inherent to the influenza virus. He described "pandemicity"--the capacity for producing pandemics--as a *trait* held by some viruses and not by others. Pandemicity is a complex trait, a product not of a single "pandemic" gene but of interactions among the genetic material of the influenza virus:

"The factors giving rise to each pandemic virus may be different, pandemicity probably being a polygenic trait."<sup>85</sup>

When he identified southern China as an influenza epicenter, he suggested that qualities of the southern Chinese environment, including cultural and agricultural activities, made the emergence of these pandemic viruses possible. He described in pastoral poetic detail the villages of southern China, nestled among "the intricate water ways of the Pearl River Delta," with their ponds and rice paddies, and children playing among the ducks, pigs and water buffalo. In the 1982 paper, Shortridge drew particular attention to the "age-old techniques" of rice farming, which involves placing domestic ducks in the paddies.

"They make use of the food preferences of domestic ducks which help protect the growing rice from insect and shellfish pests and carry out weeding, intercultivation, and manuring. The practice reduces farmer's dependence on chemical insecticides, herbicides, fertilisers, and mechanical farming aids and provides a close bird/water/rice/man association that varies with the seasons of rice growing."<sup>86</sup>

Shortridge identifies the source of influenza pandemicity in Chinese rice paddy agriculture and animal domestication. Beyond personal anecdotes and reports by travelers, Shortridge's main reference for his descriptions of rice paddy agriculture is a citation to Volume Six of Joseph Needham's *Science and Civilizations* project on "Biological Pest Control".<sup>87</sup> Not exactly in the spirit of Needham's questioning of historical development and modernity, Shortridge lumps these various kinds of descriptions (anecdote, traveler account, text) into a static notion of "age-old techniques" of the Chinese.<sup>88</sup> Shortridge stands in a "minor tradition" of modern disease experts who articulated the importance of the ecologies, or natural histories of infectious diseases.

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<sup>85</sup> Shortridge, "Avian influenza viruses in Hong Kong": 12.

<sup>86</sup> Shortridge, "An influenza epicentre?": 812.

<sup>87</sup> Joseph Needham, "Biological pest control." In: H. Hsing-Tsung, *Science and Civilization in Ancient China* Vol. 6, *Biology and biological technology*, Part 1, *Botany* (Cambridge: Cambridge University Press, 1986).

<sup>88</sup> Shortridge, *op cit*.

However, much like many of these ecologically minded virologists and pathologists, he never proposed to conduct any research into these domains, positioning cultural practices and the farmed environments as unquestioned contexts and constants to the actual research object: an object made in lab studies of the phylogeny and microbiology of influenza viruses.<sup>89</sup>

After H5N1 spread into southeast Asia, scientists began to develop what they called an "ecological approach" that turned environment, wildlife, human-animal relations, agricultural practices, and culture each into objects of research. These research projects shifted the location of pandemicity from viral phylogeny to disease ecology.<sup>90</sup> The history of medicine has explored numerous shifts in the epistemic ordering of the normal and the pathological, from focus on relation (contagion) to environment (*foyer de infection*) to agent (microbe). The shift in research on avian influenza should not be understood as a return to Hippocratic or 19th century environmentalist understandings of disease causation, that figure the environment as a direct determining force on the health of the body. Rather, these scientists drew on the principles and practices of ecology and parasitology to articulate a dynamic relationship between hosts, pathogens, and environments.<sup>91</sup>

Marius Gilbert, one of the founders of the NIH group, is an ecologist at the Center for Biological Control and Spatial Ecology at the Free University of Brussels. His early research focused on modeling of pest dynamics, such as the invasions of the horse-chestnut leaf miner (*Cameraria ohridella*), a leaf-mining moth. Soon after the 2004 expansion of the highly pathogenic H5N1 epidemic to southeast Asia, Gilbert began to develop his work on the flu virus in Thailand.

H5N1 first struck Thailand in January 2004, roughly simultaneous with its arrival in other Southeast Asian countries. A second outbreak (referred to in Thailand as the "Second Wave") began on July 2004 and continued to grow in severity and extension throughout the summer. During the early phases of the epidemic, the Ministry of Public Health and the Ministry of Agriculture and Cooperatives fought over the appropriate policy approach, hindering the effective control of the virus. By the end of September, however, they began to cooperate on the implementation of an active surveillance and control program known as the "X-Ray". The X-Ray surveillance program aimed to provide a "composite picture of H5N1 situation in Thailand" at a particular moment. Establishing more than one thousand Surveillance and Rapid Response Teams (SRRTS), these teams worked with the existing public health infrastructure, in particular by transforming the over seven hundred thousand Village Health Volunteers into a grassroots surveillance system.<sup>92</sup> The VHVs investigated and reported any unusual poultry or human infection within their villages, reporting to the SRRTS for subsequent verification by laboratory analysis. As a result, during the month of October, 2004 when the first X-Ray surveillance was conducted, 150,648 birds were sampled, of which 724 were found positive for H5N1.<sup>93</sup> Throughout the X-Ray survey, the Thai Department of Livestock Development, Ministry of Agriculture reported outbreaks verified by laboratory diagnosis to OIE.

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<sup>89</sup> Warwick Anderson, "Natural histories of infectious disease: ecological vision in twentieth-century biomedical science" *Osiris* 2nd Series, Vol. 19 (2004): 39-61. On Shortridge, see Frederic Keck, "Sentinelles Sanitaires."

<sup>90</sup> Thanks to Frederic Keck, in a personal communication, for this turn of phrase.

<sup>91</sup> Cf. Canguilhem, "The living and its milieu" *Knowledge of Life* (New York: Fordham University Press, 2008).

<sup>92</sup> Rachel M. Safman, "Avian influenza control in Thailand: Balancing the interests of different poultry producers." In: Ian Scoones, *Avian Influenza: Science, Policy and Politics* (New York: Routledge, 2010): 177ff.

<sup>93</sup> World Health Organization, "Role of Village Health Volunteers in Avian Influenza Surveillance in Thailand", January 2007.

The X-Ray surveillance program and the resulting database of outbreak reports provided Gilbert's team with detailed data on the geographic location and timing of poultry outbreaks during a one-month period. Most information about influenza outbreaks has been based on passive surveillance systems that only receive ad hoc reports of outbreaks from farmers or local governments. The X-Ray program, a form of active surveillance, by contrast "X-rayed" the entire territory of Thailand in a one-month period, providing much more detailed and comprehensive information on the circulation of the virus. Gilbert's study propose to identify the risk factors for outbreaks of highly pathogenic avian influenza (HPAI) by statistically analyzing the associations between laboratory-confirmed HPAI occurrence and a series of poultry and environmental variables.<sup>94</sup>

The study concluded free-grazing ducks were the most significant risk predictor for HPAI infection in a sub-district. Despite the fact that the highest absolute number of outbreaks occurred in chicken farms, the *distribution* of outbreaks throughout the country did not correlate with the distribution of chicken farming, of either industrial or backyard type.

“Although most HPAI outbreaks during the second epidemic in Thailand occurred in chickens, the spatial distribution of these outbreaks does not correspond to areas with high densities of chickens. For example, northeastern Thailand has many native chickens that are not protected by biosecurity measures. However, apart from incidental HPAI outbreaks, this disease never showed a marked increase in this area [ . . . ]. Instead, the distribution pattern suggests an important role of free-grazing ducks in rice paddies as in the central plains of Thailand.”<sup>95</sup>

In addition to this conclusion, Gilbert, et al make an important conceptual move by embedding this mode of husbandry--free-grazing ducks--in the landscape itself. They draw on a concept of tradition to justify the fixing of a relationship between a mode of husbandry and a form of landscape.

"Traditional free-grazing duck husbandry in Thailand is characterized by the practice of frequent rotation of duck flocks in rice paddy fields after the harvest, in which they are moved from 1 field to another every 2 days to feed on leftover rice grains, insects, and snails. Duck husbandry involves frequent field movements of flocks that are brought together in shelters often located within villages; with marketing of live birds and eggs extending beyond villages, apparently healthy ducks may play an important role in virus transmission, which explains the observed spatial pattern of HPAI."<sup>96</sup>

Identifying the significance of free-grazing ducks for highly pathogenic avian influenza risk and embedding free-grazing ducks in wet-rice paddy agriculture systems enables an extension of their scientific object in two ways. First, by embedding risk in the landscape of wet-rice agricultural systems, their model describes a geography of risk common to much of the Asian region. As Francesca Bray has argued, wet-rice paddy cultivation through large parts of Asia marks a distinctive regional mode of agricultural production, including a mode of historical

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<sup>94</sup> Results of the first Thailand study was published as Gilbert, et al (February 2006) "Free-grazing ducks and highly pathogenic avian influenza, Thailand" *Emerging Infectious Diseases* 12(2): 227-234.

<sup>95</sup> Gilbert, et al, *op cit*.

<sup>96</sup> *Ibid.*: 232.

development that differs from the European and American model of mechanization and increasing scale.<sup>97</sup> For Gilbert and his co-authors, the same ecological specificity of wet-rice cultivation and its links to distinctive cultural practices are figured as a source of influenza risk. In this way, a region is constituted on the basis of pathogenic danger. The paper published by Gilbert and co-authors adds, suggestively, that "all currently affected [by HPAI H5N1] countries are known for their rice and duck production."<sup>98</sup>

The great significance of substituting wet-rice landscapes for free-grazing duck practices lies in the technical capacities the substitution enables. The concept becomes an instrument because it makes possible an analysis of influenza risk from a new view-point: from seven thousand kilometers overhead.

The strong association between ducks and rice crops facilitates application of remote sensing to identify rice-crop areas and patterns that may sustain forms of duck husbandry prone to HPAI outbreaks.<sup>99</sup>

In the next paper published by the group, Gilbert collaborated with Xiangming Xiao, an expert analyst of satellite remote sensing data, to turn the risk-factor of free-grazing ducks into what he later termed the more "generalisable" risk factor of "intensive rice cropping."<sup>100</sup> Xiao was born in China, but studied in the United States and is now Professor at the University of Oklahoma and Director of the Center for Spatial Analysis there. With image data collected by the Moderate Resolution Imaging Spectroradiometer (MODIS) sensor on NASA's Terra satellite, Gilbert and Xiao developed an algorithm to predict rice-cropping. Their results show a strong correlation between intensive rice-cropping (paddies planted two or more times per year) and avian influenza outbreaks in both Thailand and Vietnam. By substituting satellite generated data about rice-cropping land use for information about poultry type and intensity, Gilbert et al (2008) argue that they have increased both the general utility and the resolution of the model.

"An applied result of this study is that the distribution of rice cropping intensity can readily be established at any time and be used to complement traditional duck census data. Remote sensing data are available at a much greater spatial and temporal resolution than traditional censuses, thus allowing a fine-scale risk mapping."<sup>101</sup>

Satellite imaging transforms the object of influenza science, producing a novel knowledge domain distinct from the molecular phylogenies of sampled viruses analyzed in the laboratory, or the statistical populations of poultry censuses. Describing this viewpoint as only a "*complement*"

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<sup>97</sup> Francesca Bray, *The Rice Economies*.

<sup>98</sup> *Ibid.*: 233.

<sup>99</sup> *Ibid.*

<sup>100</sup> Marius Gilbert, "Spatial Modeling of H5N1: A Review" The 3rd International Workshop on Community-based Data Synthesis, Analysis and Modeling of Highly Pathogenic Avian Influenza H5N1 in Asia. November 2011. Beijing, China.

<sup>101</sup> Marius Gilbert, Xiangming Xiao, Dirk U. Pfeiffer, M. Epprecht, Stephen Boles, Christina Czarnecki, Prasit Chaitaweesub, Wantanee Kalpravidh, Phan Q. Minh, M. J. Otte, Vincent Martin, and Jan Slingenbergh, "Mapping H5N1 highly pathogenic avian influenza risk in Southeast Asia" *Proceedings of the National Academy of Sciences* Vol. 105, No. 12 (2008): 469-4774. See also: Marius Gilbert, Xiangming Xiao, Prasit Chaitaweesub, Wantanee Kalpravidh, Sith Premashthira, Stephen Boles, Jan Slingenbergh, "Avian influenza, domestic ducks, and rice agriculture in Thailand" *Agriculture, Ecosystems and Environment* Vol. 119 (2007) 409-415.

to duck census data, however, does not fully elucidate the significance of this new practice. During my fieldwork I found that remote sensing data was prized not only because it could provide greater resolution than censuses, but also because these censuses often did not exist, were inaccessible or were distorted. Certainly, Thailand's X-Ray survey was exceptional for its comprehensive active surveillance of outbreaks and thorough survey of poultry holdings. The comprehensiveness of the data made the Thai study ideal for a model that could be extrapolated to other sites. But I found when the researchers aimed to develop a geo-spatial risk map of China that drew upon the insights of the model, the *objective* of the map--its proposed outcomes and subsequent uses--was quite different.

### *A field guide*

With the support and collaboration of Vincent Martin and the FAO ECTAD office in Beijing, Xiao and Gilbert produced a geo-spatial risk map of China--what they understood to be a risk map of the influenza epicentre. Although the methods and organization of the study are practically identical to the Thailand and Vietnam risk models, aspects of the *situation* in which the China study was undertaken and, as a result, differences in the way the study was *used*, together make it a fairly distinct work. This difference involved shift in objective, from model-building toward the development of what can be called a "field guide" to the influenza epicenter.

How did the situation change? Gilbert's Thailand study was based on data collected during the X-ray Survey conducted after the second wave epidemic in 2005. Following large-scale culling and "stamping out" of outbreaks, as well as subsequent changes to the forms in which poultry are farmed and marketed, H5N1 virus was eliminated from Thailand and has not returned. The Thailand study was based on archival data that, in a sense, represented an archival situation. As a result, and Gilbert made this clear in his presentation to the 3rd Workshop, the primary goal of the study was to produce "generalisable" tools and facts about avian influenza risk. Through the authority attributed to his models by scientific publication, Gilbert hoped to construct scientific facts that would be true not only for Thailand, but also elsewhere. Gilbert et al (2008) write:

"The model of HPAI H5N1 virus risk developed in Thailand with the data from the second epidemic wave maintains its predictive power when applied to other epidemic waves or other regions, indicating that the model can be extrapolated in space and time."<sup>102</sup>

To this end, Gilbert and his team "validated" the study by using the model to develop and test risk maps for Cambodia and Laos.

In the extrapolation of the model to China, however, although articles could still be published and facts constructed, the use of the results was quite different. First of all, the object was now the influenza epicentre and not only a peripheral and occasional victim of avian influenza epidemics. The goal of the model was to produce an ecological depiction of H5N1 risk that would break part the "influenza epicenter" into zones of higher and lower risk, refining previous hypotheses that associated the epicentre with southern China as a whole.

In addition, while HPAI H5N1 was an archival problem in Thailand, in China avian flu was an ongoing--and fast approaching the status of endemic--problem. Therefore, the team

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<sup>102</sup> Gilbert, et al, "Mapping H5N1" (2008): 4771.

producing the geo-spatial risk model were not only interested in building models that could lead to general facts about influenza risk (such as the importance of free-grazing ducks). Rather, they hoped to identify specific risk zones in China. They hoped to develop an analytic of the influenza epicenter that could directly orient scientific research and practical intervention on pandemic flu at its hypothetical source.

But the need for a field guide to the influenza epicentre was also driven by something else considered by influenza experts as a research problem: the lack of information about the location of flu outbreaks in China, due to either technical problems of detection or political problems of obfuscation. Here was another important contrast with the Thai study, shaped as it was by the thorough data of the X-Ray survey. By the middle of my fieldwork in spring of 2011, FAO officials noted a basic discrepancy in the published reports of avian influenza H5N1 in China. It had been almost two years since China had reported an outbreak of avian influenza in poultry to the Office International de Epizootique (OIE). And yet, the Harbin National Key Laboratory for Avian Influenza, in the process of conducting active surveillance necessary to ensure the efficacy of their vaccines, frequently isolated H5N1 viruses--including novel ("emergent") clades and sub-clades. As Martin et al put it in the paper eventually published from the geo-spatial risk study, the Harbin Virology Institute's surveillance data

“has provided evidence that, despite a reduction in reported HPAI H5N1 outbreaks, some parts of the country still offer a favorable breeding ground for influenza viruses to circulate and potentially novel strains to emerge, representing a threat for the generation of new influenza pandemic strains. [This particularly applies to the southern part of the country, which has historically been referred to as a hypothetical influenza epicenter where agricultural and cultural practices place man and animals in close proximity].<sup>103</sup>”

The discrepancy is a direct consequence of China's mass vaccination policy, which both transformed the dynamics of the epidemic itself and resulted in new sources and kinds of data collected about the epidemic.

Poultry breeders in China are known to have vaccinated for avian influenza since at least as early as the 1990s. The Harbin Lab, led by its director Chen Hualan, began vaccine production with an inactivated vaccine produced with a low pathogenic homologue virus, an H5N2 virus from England provided by Dr. Dennis Alexander (A/turkey/England/N-28/73). This vaccine was originally licensed for use in August 2003, only for chickens raised in special zones of Guangdong Province for export to Hong Kong and Macao. By the end of 2003, the vaccine was approved by the Chinese Veterinary Drug Evaluation Committee for general use. Quite soon after it was approved, large-scale poultry outbreaks occurred throughout southern China and southeast Asia, and China's veterinary epidemic prevention bureaus began to use the vaccine to control disease in the areas where virus was circulating and in “buffer zones”. Although the vaccine did help to control disease spread, the seed virus for the vaccine was found to “exhibit antigenic diversity with the prevalent H5N1 strains circulating in China at the time”--in other words, the vaccine's efficacy was compromised because it did not closely match the virus strain.

Chen and her lab began work on a vaccine produced through “reverse genetics.” The first step was the construction in the laboratory of “reassortant” viruses made by combining the

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<sup>103</sup> Brackets in original. Vincent Martin, Dirk U. Pfeiffer, Xiaoyan Zhou, Xiangming Xiao, Diann J. Prosser, Fusheng Guo, Marius Gilbert, "Spatial distribution and risk factors of highly pathogenic avian influenza (HPAI) H5N1 in China" *PLOS Pathogens* Vol. 7, No. 3: 1-11.



internal genes from a low-pathogenic, but high-growth virus (A/Puerto Rico/8/34) with the Hemagglutinin and neuraminidases genes from the first isolated H5N1 virus (Goose/Guangdong/1/96). Chen christened this vaccine re-1, for reassortant 1. In 2005, the Chinese government, concerned about expanding outbreaks, as well as about the apparent use of unlicensed or ineffective vaccines by farmers, mandated universal vaccination of poultry. As Chen states in one article, "Epidemiological studies [in 2005] indicated that all of the prior outbreaks had occurred in farms that did not vaccinate or vaccinated with unqualified vaccines."<sup>104</sup> By universal immunization, the new policy literally meant "100 percent" of poultry (including chickens, ducks, geese and other birds raised domestically). The policy states under the section 'Immunization scope and requirements': "Institute compulsory immunization of all domestic poultry (*jiaqin*), immunization coverage should reach 100%."<sup>105</sup> The policy divided immunization strategy into two modes: large-scale, industrial farms were required to vaccinate poultry themselves; immunization would be verified by post-vaccination serology conducted by the Ministry of Agriculture, through the county-level Animal CDC. Small-scale, and especially so-called "backyard" (*sanyang*) farms, would be vaccinated during bi-annual immunization campaigns conducted by county-level Animal CDC.<sup>106</sup> The immunization "campaigns" are a governmental technique with a history in other "mass" campaigns during the Mao era, such as the mass mobilizations to eradicate agricultural pests or remove vectors of disease such as the snail that harbors schistosomiasis.<sup>107</sup> The national-level state provided vaccines for free, typically only charging a small fee for the immunization service.

The mass vaccination program transformed the kind and quantity of information collected about the avian influenza epidemic, as well as obviously transforming the dynamics of the epidemic itself. Two parts of the Ministry of Agriculture developed distinct post-vaccination surveillance programs. The China Animal Disease Control Center of the Ministry of Agriculture developed a surveillance program to assess the extent of immunization in the national poultry population and ensure that "herd immunity" was achieved. Each year, the Ministry of Agriculture issued a national HPAI surveillance plan. The implementation of this plan was organized at the province level, and carried out by laboratories at the national, province, and prefectural level, as well as by the national monitoring station network (*guojia cebao zhan*). All sampling results are entered into a central computerized system run by CADC called the

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<sup>104</sup> H. Chen, "Avian influenza vaccination: the experience of China" *Review of Science and Technology of the Office Internationale de Epizootiques*.

<sup>105</sup> "对所有家禽实行强制免疫，免疫密度应达到 100%（有特殊要求需不免疫的家禽除外）。" In: 农业部，关于印发《加强高致病性禽流感免疫的操作规范》的通知，农医发[2005]27 号，November 11, 2005. [Ministry of Agriculture, Press release on "Operational Instructions for Strengthening Highly Pathogenic Avian Influenza Immunization"].

<sup>106</sup> According to the 2005 policy, toward "*sanyang*" or "backyard" farms, "Each annual spring, fall institute once total immunization according to the immunization schedule." This policy still continues today. As the policy from 2013 states it, "规模养殖场可按下述推荐免疫程序进行免疫，对散养家禽在春秋两季各实施一次集中免疫，每月对新补栏的家禽要及时补免。" See: 农业部关于印发《2013 年国家动物疫病强制免疫计划》的通知

[Ministry of Agriculture Press Release on "2013 National Plan for Compulsory Immunization of Animal Epidemic Disease." At: [http://www.moa.gov.cn/govpublic/SYJ/201303/t20130307\\_3248256.htm](http://www.moa.gov.cn/govpublic/SYJ/201303/t20130307_3248256.htm)

Accessed on: October 15, 2013.

<sup>107</sup> See, for example, Joshua S. Horn, *Away with all pests: An English surgeon in in People's China, 1954-1969* (Monthly Review Press, 1970); Joan Kaufman, "SARS and China's health-care response: better to be both red and expert!" in Arthur Kleinman and James D. Watson, eds., *SARS in China: Prelude to Pandemic?* (Stanford: Stanford University Press, 2005).

"Nationwide Animal Disease Surveillance and Epidemic Information System" for integrated analysis. In 2010, for example, the surveillance program collected over 4.3 million serological samples, and 347,000 viral samples.<sup>108</sup>

At the same time, the Harbin lab developed its own post-vaccination surveillance program. The objective of this surveillance program was quite different, as its purpose was not to assess the coverage of the immunization campaign, but to detect vaccine failure--that is, situations where the circulating strain has become antigenically divergent from the current vaccine. Over the course of the eight years of universal vaccination, the Harbin lab did discover antigenic diversity in a number of cases, leading them to develop new forms of the reassortant vaccine. Indeed, during the course of the vaccination campaign, smaller and smaller differences have become significant because they have been demonstrated to cause vaccine failure. The classification of the virus has consequently become more and more refined, from "type" (e.g. H5N1), to clade; and what started as clade 1, 2, and so on, has now reached the level of clade 2.2.2.4. The "new" influenza virus is today defined, we could say, as the externality to the vaccine's efficacy.

During a Spring 2011 conference on vaccination, the results of the two different post-vaccination surveillance programs came directly into conflict. Wang Hongwei, from China Animal Disease Control Center, first reported that China had achieved herd immunity--defined as 70 % positive rate in post-vaccination serology--in every province. However, later that same day, Chen Hualan of the Harbin Avian Influenza Lab presented a report on the work of her lab in the development of vaccine s and there surveillance program. She then stated that "Vaccination coverage is definitely not as high as 90 per cent," she said.

"In broilers over 80 per cent were not vaccinated. In ducks, over 70 per cent were not vaccinated according to our surveillance."

Given the high-level ministerial and international character of the conference, I was curious if Chen Haulan's remarks would be interpreted as the dissenting view of a whistleblower. Instead, however, when discussion returned to the topic later that afternoon, her speech-act was resituated as a position within a scientific controversy, and specifically, a question about the approaches to statistical sampling.

Chen: "When we went to farmers to collect samples, we see the same result [as ACDC]. When we go to the markets we see different results. For me its very easy to explain. The local people in charge of vaccination, they know which farmers are vaccinating properly, so they bring us to those farmers, its also where they are collecting samples."

Vincent Martin: (smiling) "Not targeted vaccination, but targeted post-vaccination surveillance (audience laughter)."

A member of the Chinese ACDC: "Backyard and grazing farms, they maybe only get one shot. If you have been to Chinese markets, you will see dozens of chickens, not a lot, never over a hundred. Our surveillance rates are also lower than that reported by local government.

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<sup>108</sup> Wang Hongwei, "HPAI Surveillance and data management in China." Presentation at China and Vietnam Forum on HPAI risk management and control. 8 March 2011. Beijing, China.

Agriculture ministry is also concerned about this. [There are] different models of agriculture in China, how do you sample? If you sample commercial farms, you might have a high rate, but if you sample backyard farmers the rate will be lower. In the policies, before the birds enter the market, there is no requirement to vaccinate. The over 70 percent titre is because these numbers are monitored at the farms. The birds sampled by Dr. Chen probably hadn't been vaccinated for over four months."

The two surveillance apparatuses are guided by quite different means and ends. The ACDC surveillance intends to use sampling devices to provide a representative assessment of the mass vaccination program. To do so, they take samples in systematic and regular distribution across the country, organizing through the county level ACDC to travel to farms. On the other hand, the Harbin lab surveillance *intends to find the gaps in the vaccination program*. This surveillance needs to find any viruses which are circulating unimpeded by vaccines. To find these viruses, they target surveillance at locations suspected as high-risk for infection, such as live poultry markets, because this is where they have habitually been able to find viruses.

For the ACDC and Ministry of Agriculture leaders, the lack of outbreaks was a consequence of the success of mass vaccination. But international scientists often invoked a counter-interpretation. Martin usually commented on the evidence of circulating virus to suggest that vaccination was not only preventing outbreaks: it was also "masking" knowledge about the distribution of risk. In his ironic comment quoted above, the ACDC's apparently systematic surveillance can also be described as "targeted": targeted by county-level ACDC's to find only well-vaccinated flocks. Some other international scientists expressed suspicion that outbreaks in fact were occurring, but were covered up. The problem, one of them told me in 2011, is that a whole year had passed without any outbreak reports. This had put China in a "bind," he said: reporting outbreaks *now* would make the country's influenza control appear to be regressing.

"Couldn't you just report one or two?" he asked me rhetorically. "There have been reported outbreaks in Cambodia, Vietnam, Korea, Japan; but none in China?"

Without reports of outbreaks, there was no way to know in which areas of the country viruses might emerge, despite the confirmation from the Harbin Institute's surveillance that was actively circulating in "some parts of the country." In such a situation, the maps of risk produced by the risk model come to substitute for reports of disease as a tool for orienting research and interventions. The China geo-spatial risk model was developed by testing the correlation between risk factors identified as significant in the Thai study--for example chicken and duck populations and rice cropping intensity--and archival data on influenza distribution. The archival data was broadly divided into two types: outbreaks of HPAI H5N1; and positive samples of HPAI H5N1 collected by the Harbin National Lab's surveillance system between 2007 and 2009. Both of these data streams were collated or collected at a national level and published in the Ministry of Agriculture's official "Veterinary Bulletin," a monthly report.

In early March 2011, just as the geo-spatial risk map paper was going to press, I helped Martin organize a bi-lateral meeting between China and Vietnam on the topic of avian influenza vaccination. As was his wont, Martin planned for the conference to have an argument, arranging the invited talks as if they were chapters in a book. Along with others at FAO, Martin hoped to convince China's Ministry of Agriculture to shift from a policy of mass vaccination to a policy that they called "risk-based targeted vaccination." There were specific rationales given for this shift: economic rationales (the enormous expense of a mass vaccination program) as well as scientific rationales (as Martin reminded everyone, the goal of the H5N1 response remained

*eradication*, but evidence indicated that mass vaccination could never *eradicate* the H5N1 virus because of the "silent" circulation of virus). Vietnam had already begun to conduct risk-based vaccination under the guidance of an FAO expert team, which was why they were invited. At the conference, Vincent's own presentation highlighted the recently completed geo-spatial risk study as a guide to this new approach.

"China is huge," Vincent told the assembled crowd in a hotel in Beijing, "And we still don't really know where are the main risk zones." With an image of the risk maps projected on a large screen hanging from the ceiling, Vincent continued:

"The red areas are high risk, and this shows it is not all of south China, but that it is associated with particular risk predictors. . . . Thanks to this analysis you can concentrate efforts, you can use this map to overlay outbreaks, human cases. You can see that lots of parts of China have very little risk."

Vincent's use of the risk model aims to provide a practical guide to control efforts. But it also reveals the political and ethical contexts in which the scientific act of making the risk model takes place. While officials from the Ministry of Agriculture declared the *success* of the mass vaccination policy based on the evidence that there were no outbreaks, Martin used the risk map to recommend a "concentration" of vaccination effort on the basis of risk. This was not only a scientific controversy. It also showed how Martin attempted to construct knowledge of China that worked toward *political independence* from official reports.

The potential for this independence was further heightened by the techniques of satellite imaging, which hypothetically enabled risk to be mapped without reliance on the poultry surveys conducted under national jurisdiction. Pierre Bourdieu has famously identified a link between an "objectivist" epistemology and a transformation in the *social* relationship to the object. Objectification, in Bourdieu's view, is not only a certain relationship of knowing subject to object of knowledge, but also a social *exteriority* or *distance* from the object cultivated through the "scholastic" forms of life. In his introduction to *The Logic of Practice*, Bourdieu tells of a moment in which the problem of objectivity was brought home to him. He was in France, looking at photographs he had taken during his fieldwork in Algeria. He came to a photograph he had taken of a stone jar, decorated with snakes, that he had taken inside of a house. "I took those photographs in the course of field-work in the Collo region, and their high quality, although I had no flash-gun, was due to the fact that the roof of the house into which they had been built had been destroyed when the occupants were expelled by the French Army." The anecdote captures Bourdieu's view that the clarity of objectivity is only possible from a position *outside* of the world, one that treats its objects as if there was no longer life in them.<sup>109</sup>

#### *At the epicentre: Adjacency*

The influenza epicentre as a scientific object was transformed by the study. Based on the risk model, Martin and his colleagues concluded that southern China--the classical influenza epicentre--could be divided into three distinct zones, each with different levels of risk. Whereas the Hong Kong studies had typically, emphasized the importance of the nearby Pearl River Delta region, Martin, et al (2010) argue that the new model "supports the hypothesis of a wider and slightly displaced epicenter of influenza viruses, not only concentrated around the Pearl River

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<sup>109</sup> Pierre Bourdieu, *The Logic of Practice* (Palo Alto: Stanford University Press, 1990): 3.

Delta in Guangdong province but extending south of the Yangtze River and including provinces such as Jiangxi province where internal segments of the 1996 geese HPAI H5N1 virus may have originated.” As we looked at the pre-publication risk maps together in his office that spring, Vincent's finger came to rest on a patch of deep red in the middle of Jiangxi Province, Poyang Lake's water tinted with high risk. Only a few months later, as I got down off the bus in the city of Wucheng, I was standing squarely amidst this hypothetical influenza epicenter.

In Wucheng I planned to visit the headquarters of the Jiangxi (Province) Poyang Lake Environmental Protection Zone. The large metal gate to the Zone stood at the far end of the town. A foreigner visiting in the late spring--the off-season for bird watching--occasioned a bit of surprise. The director of the Protection Zone and the manager of its hotel were called to show me the museum. The director was gruff and blunt: throughout our visit to the museum, he spoke only three sentences, each of which in some way reminded me that the museum entrance fee was fifty yuan. The first floor was entirely filled by a large topographical model of the lake. Upstairs were stuffed birds in glass cases, white, red, black, grey, waders and geese and cranes. The manager of the hotel, friendly and smiling, explained to me in technical detail how the birds were cut open and stuffed. Not many wildlife preserves have so many stuffed birds, because they are very difficult to maintain, he told me. As we left, the director, nervously checking his watch, promoted a tour of the birding areas of the park, despite the late hour. The grand tour, he noted, would only cost me three hundred yuan.

For all my intention of turning these two men into the subjects of anthropological knowledge, I found myself far more convincingly standing in the position of the tourist. I did take the bird tour the next morning, driving out with Yu into the grasslands to chase the last few lingering waterbirds. But despite Yu's joyful smiles and exclamations when we spotted a tern or rook, my doubts were difficult to eradicate: *had I come here only to look at birds, and mostly stuffed ones at that?*

My own ethical difficulty in constructing myself as knowing subject showed me that the anthropologist faces a similar ethical situation as the scientist: in the field, the epistemological relation to the object is also always a social relation to an other. One can never be confident that one's presumption to make someone else into an object will not be overturned by the other's object-making. For this reason, this dissertation does not attempt to critique scientific objectivism through an immersive return to "local knowledge." Such simplistic attempts to oppose the global with the local leave the anthropologist with words and experiences but no way to identify significance: more the tourist than the scholar. My travel to Poyang Lake should not be understood as an effort to contrast the distant objectivism of science with the nearness of lived experience. Indeed, my only reason for going to Poyang Lake was because it had been rendered into a significant object by scientific workshops and global risk models. Instead, I cultivated as best I could a position of *adjacency* to scientists and hotel managers, veterinarians and farmers alike. Adjacency, writes Paul Rabinow, is a form of untimeliness whose "goal is identifying, understanding, and formulating something actual neither by directly identifying with it nor by making it exotic."<sup>110</sup> The scientific work taking place on Poyang Lake in offices in Beijing or the laboratories of Harbin had become as much a real part of the lake as the birds themselves. I needed to move between these multiple sites and forms of practice, not set one against the other as reality to representation, or life-world to system.<sup>111</sup>

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<sup>110</sup> Paul Rabinow, *Marking time: On the anthropology of the contemporary*; see also Rabinow, *Reflections on fieldwork in Morocco*.

<sup>111</sup> See Marcus "Ethnography in/of the world system" for the often cited articulation of multi-sited fieldwork.

A position of adjacency allows a reconsideration of the scientific work on avian influenza in China that does not end with a critique of the "god-trick" of Science disembodied from contexts of practice and political situations.<sup>112</sup> Where Bourdieu argues there is an *absolute social* break between scholastic objectivity and lived practice, I suggest instead that there is an *ethical situation* involving the reconstruction of epistemological, political and social relationships produced by conducting flu research on China's grounds. From my position standing adjacent to scientists and farmers alike, the risk models appear not as a total break with Chinese forms of life, but as a practical or strategic "move" within a complex ethical situation.

As the conduct of research *on* China's avian influenza outbreak came to be conducted *in* China, the epistemological relation of knowing subject to object of knowledge was joined with ethical and political relations to Chinese subjects. Even as FAO experts such as Martin expressed frustration with dependency on Chinese affiliates, employees and collaborators, he constantly worked within such relationships and reflected on how to manage them. Steven Shapin has described how laboratory managers reflected on the rise of industrial science in 20th century America. Shapin argues that these managers considered the new norms and forms of science as a *practical* problem to be worked over, rather than an absolute crisis of morality and truth.<sup>113</sup> In a similar way, although most descriptions of avian flu in China figure China as an absolute block to scientific truth, researchers in China constantly worked with Chinese partners to produce scientific knowledge. Along the way, they denounced, critiqued and reconstructed the ethical problems that they encountered, a practical, historically situated activity in an ethical domain that should not be confused with an absolute and ahistorical moral boundary between science and politics.

Building, validating and using the geo-spatial risk model required scientists from Europe and America, based around the NIH group or the FAO Emergency Centre, to form relationship with government officials, Chinese "national" scientists, even hotel managers and farmers. It required going into the influenza epicentre, not only understood as a material site, but also as a setting for relationships of exchange and communication.<sup>114</sup> Rather than a moral or political breakdown between the global and the local, bird flu science formed an event around which the normative uncertainty of new transnational forms of scientific activity could become objects of ethical practice. Building global health on China's grounds refers to the doubled consequences of this transnational situation: on the one hand, global health researchers constructed a science of pandemic influenza by mapping and analyzing the material grounds of China's landscapes. But as they did so, the form of global health was also conditioned and shaped by the way the researchers accounted for the practices of Chinese officials, scientists and farmers, transforming the ethical and political "grounds" of their claims to knowledge.

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<sup>112</sup> Donna Haraway, "Situated knowledges: The science question in feminism and the privilege of partial perspective" *Feminist Studies* Vol. 14, No. 3 (Fall 1988): 575-599.

<sup>113</sup> Stephen Shapin, *The Scientific Life: A moral history of a late modern vocation* (Chicago: University of Chicago Press, 2010); Shapin, "Who is the industrial scientist? : commentary from academic sociology and from the shop-floor in the United States, ca. 1900-ca. 1970" in Massimo Mazzotti, ed., *Knowledge as social order: rethinking the sociology of Barry Barnes* (Brulington, Vt.: Ashgate, 2008).

<sup>114</sup> Historian Fa-ti Fan has discussed similar issues in Fan, *British naturalists in Qing China: Science, empire and colonial encounter* (Cambridge, MA: Harvard University Press, 2004). See also Fan, "Redrawing the map: Science in twentieth-century China" *Isis* Vol. 98, No. 3 (September 2007): 524-538.

## Chapter Two

### Techniques of Consanguinity and Affinity

“Does China need another SARS?” Vincent Martin asked me in February 2011 in Beijing, after explaining his frustration with the recent lack of influenza outbreak reports made by China's Ministry of Agriculture. “Does China need another SARS in order to take reporting of diseases seriously?” He handed me a published article written in English by an official from China's Center for Disease Control (CDC), an institution under the Ministry of Health with responsibility for human population health. The Health Ministry had emerged transformed from the SARS outbreak, he said. In Martin's view, the article was evidence of this: the author discusses a disease surveillance system, documenting both successes and failures, and demonstrating a willingness to expose both the good and the bad for review in the domain of English-language scientific publication. The outbreak of highly pathogenic avian influenza H5N1 should have had the same effect on the Ministry of Agriculture, Martin continued, but somehow reforms had stalled.

During my fieldwork, foreign influenza experts working in China often placed the avian influenza epizootic in a narrative series with the 2003 SARS epidemic. These narratives figured China--or in Martin's case, more specifically the Chinese Ministry of Agriculture--as an obscurant to the global knowledge of influenza surveillance. As I described in the last chapter, the effort to build global influenza surveillance capable of "forecasting" pandemics dates to the establishment of the World Influenza Center in 1947. However, the difficulty of establishing the compatibility and accessibility of samples or sequences collected by various national governments has often hindered the efficacy of these forecasts.<sup>115</sup>

According to many analysts and observers, the SARS epidemic revealed an absolute breakdown between China's national government and international or global health governance. As David Heymann and Guenael Rodier, epidemiologists with the WHO, write in a widely cited article, "The SARS experience . . . . made one lesson clear early in its course: inadequate surveillance and response capacity in a single country can endanger national populations and the public health security of the entire world."<sup>116</sup> Legal scholar David Fidler argues that the SARS outbreak revealed the urgency of shifting from a Westphalian, international framework of public health governance toward a "post-Westphalian" *global* health governance. China's refusals to forthrightly communicate information about the ongoing SARS epidemic put Chinese national sovereignty in direct confrontation with the post-Westphalian era of disease control.<sup>117</sup>

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<sup>115</sup> See French influenza expert Claude Hannoun's reflection in: Hannoun with Susan Craddock, "Hong Kong Flu (1968) Revisited Forty Years Later" in Tamara Giles-Verenick, Susan Craddock, and Jennifer Lee Gunn, eds., *Influenza and public health: Learning from past pandemics* (Washington, DC: Earthscan, 2010).

<sup>116</sup> David Heymann and Guenael Rodier, "Global surveillance, national surveillance, and SARS" *Emerging Infectious Diseases* Vol. 10, No. 2 (February 2004): 173-5. See also David Heymann and Guenael Rodier, "SARS: a global response to an international threat" *Brown Journal of World Affairs* Vol 10, No. 2 (Winter/Spring 2004): 185-197.

<sup>117</sup> David Fidler, "SARS: Political pathology of the first post-Westphalian pathogen" *Journal of Law, Medicine and Ethics* Vol. 31 (2003): 485-505; Fidler, book.

Although international flu experts often criticized China with language drawn from the SARS affair, they were also at the same time engaged in diverse and productive transactions with Chinese counterparts. Many of these transactions moved samples and sequences across boundaries declared in other contexts to be impermeable. Taking the criticisms of China's obscurity as a face-value representation of how things are would remove a momentary judgment from the situation of its use. Not only would it rely on only one actor's viewpoint on the relationship, it would also rely on only one moment in that actor's engagement with the relationship. In this chapter, I re-situate these judgments within actual, uncertain and dynamic transnational relationships of collaboration, affiliation and exchange.

Recent scholarship on science in China rejects narratives of "global" or "Western" scientific transfers prompting "local" responses.<sup>118</sup> Michael Hathaway, in an article on rainforest conservation in southwestern Yunnan Province, proposes instead to describe what he calls "transnational work" to conserve the forest. As a concept, transnational work "reveals the complex and shifting dynamics of developmental encounters," directing ethnographic attention toward how "links are formed, maintained, and transformed."<sup>119</sup> In an article reviewing scholarship on science in China, Fa-ti Fan discusses Chinese émigrés who studied in North America but have now returned to Taiwan, Hong Kong, or mainland China. "Whether one calls the phenomenon a brain drain, an intellectual migration, or the import of talent," he writes, "the eventual outcome is much more complex than a one-way traffic of intellect eastwards across the Pacific." Fan proposes empirical study of "transnational networks of science" to replace the increasingly inadequate distinctions of modernity/tradition, West/non-West, and global/local.<sup>120</sup>

Relying on a "network metaphor," however, does not do justice to the variability and contestability of relations of affinity in transnational scientific work. In a conceptual article that draws on his monograph on kuru disease in Papua New Guinea, Warwick Anderson proposes an analysis of scientific knowledge and value that focuses on relationships of exchange.<sup>121</sup> Anderson adopts philosopher John Dewey's notion that value is situational and interpersonal, rather than contained within social structure or norms of social solidarity. In Anderson's view, values such as global transparency or national particularity are *products* of exchanges, rather than inherent ideals. It follows that attention to the forms and patterns of transaction, drawing from a lengthy anthropological tradition, should be the objective of an anthropology of science in transnational domains.

A focus on concrete acts of exchange also draws attention to the *materiality* of the exchanged objects.<sup>122</sup> In the case of influenza, the circulation of three objects is important: *reports* of outbreaks; *samples* of viruses; and *sequences* of virus samples. While the sample is a physical object requiring infrastructures such as cold chains to maintain its viability, the *report* and the *sequence* are pieces of information that are housed and circulated in a quite different manner. Historians have shown that 20th century developments in biology that remade life on the model of an information science, primarily through understanding DNA as the "code" of life

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<sup>118</sup> See especially Benjamin Elman, *On their own terms: science in China, 1550-1900* (Cambridge, Ma.: Harvard University Press, 2005).

<sup>119</sup> Michael Hathaway, "Global environmental encounters in Southwest China: Fleeting intersections and "transnational work" *The Journal of Asian Studies* Vol. 69, No. 2 (May 2010): 427-451.

<sup>120</sup> Fan, "Redrawing the map," *op cit*.

<sup>121</sup> Warwick Anderson, "Objectivity and its discontents" *Social Studies of Science*.

<sup>122</sup> Anderson cites Nancy Munn's anthropology of material practices in the kula ring. Munn, *The fame of Gawa: a symbolic study of value transformation in a Massim (Papua New Guinea) society* (Cambridge: Cambridge University Press, 1986).



and through the application of computing in bioscientific research.<sup>123</sup> More recently, anthropologists have argued that the "informationalization" of life problematizes long-accepted norms of scientific exchange and regimes of biosafety, because life as information can circulate far beyond the regimes designed to secure dangerous materials.<sup>124</sup>

Although focusing on the forms of material exchanges, I do not see value and values as only products of exchange. Normative judgments are not only conditioned by their location amidst transactions, but also recursively reform patterns of transaction. I here follow Foucault's analytic of ethics, which takes as its object subjects in their freedom to form themselves. With anthropologist James Faubion, however, I emphasize how the care of the self depends upon the governance of relationships with others. An approach to transactions as ethical situations moves the object of analysis from a descriptive or typological level (for example, a comparison of structural forms of exchange), toward the way participants in exchanges consider, reflect on, question, critique, propose and practice exchange and the relationships in which exchanges are made.

The development of new forms of transnational scientific exchanges in avian influenza research produced situations of normative uncertainty about the partners, mediums, and materials being exchanged. This chapter does not propound or describe the normative structure of science in an ahistorical sense--indeed, I claim that transnational scientific exchanges are normatively underdetermined, that is, there is no consistent or stable reference for how these exchanges should be conducted.<sup>125</sup> Rather, the chapter analyzes how normative judgments are situated and engaged within situations of normative uncertainty. How are differences between matter and information (sample and sequence) brought into relation with boundaries of science and politics (national and international or global)? It is not enough to simply avoid the distinctions of local and global, West and non-West as a methodological premise. Rather, through analysis of how transnational exchanges are made, reflected on, and judged, it is possible to see not only how these transactions put scientific and political values in question, but also how evaluations of these exchanges re-shape the forms and boundaries of transaction.<sup>126</sup>

### *A Chinese Wall*

Rob Wallace sees politics amidst molecules and microbes. His vehement blog posts against agribusiness, unlike the more commonplace nostalgia for Arcadian farmlands, finds political solutions in the details of molecular epidemiology. He cites Lukacs and Darwin in the same paragraph. Wallace was born in the Bronx to a family of radical scientists, his parents known for

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<sup>123</sup> See Georges Canguilhem, *A vital rationalist* (New York: Zone Books, 2000); Lily Kay, *Who wrote the book of life?: A history of the genetic code* (Palo Alto: Stanford University Press; 2000); Hallam Stevens, "On the means of bio-production: Bioinformatics and how to make knowledge in a high-throughput genomics laboratory" *Biosocieties* Vol. 6, No. 2: 217-242.

<sup>124</sup> Carlo Caduff, "The semiotics of security: Infectious disease research and the biopolitics of informational bodies in the United States" *Cultural Anthropology* Vol. 27, No. 2 (2012); 333-357.

<sup>125</sup> On normative underdetermination, see Paul Rabinow and Gaymon Bennett, *Designing human practices: an experiment with synthetic biology* (Chicago: University of Chicago Press, 2012).

<sup>126</sup> See Lydia Liu and Prasenjit Duara on how "difference" and "nation" are bound up with "the same" and "globality" or "internationality". Liu, *Translingual Practice*, *op cit*; Liu, "Lydia Liu, "The question of meaning-value in the political economy of the sign," in L. Liu, ed., *Tokens of exchange: The problem of translation in global circulation* (Durham, N.C.: Duke University Press, 1999): 37. Duara, *Rescuing History from the Nation*, *op cit*.

the use of quantitative methods and ecological modeling in the analysis of poverty and for denouncing the decline of public health in the United States. Their most widely known book examines how RAND corporation operation-systems modeling led to the decline of fire services in poor neighborhoods of New York City, ultimately causing "epidemics" of building fires, middle-class flight, and property abandonment. This is not a critique of modeling, but a critique of what they describe as the "primitive" models developed by RAND.<sup>127</sup>

Robert Wallace's own dissertation work examined the geography of the AIDS epidemic in the boroughs of New York City. His later work on influenza was more unexpected. In 2005 he moved to Los Angeles where his partner was living and managed to land a job at the laboratory of Walter Fitch at the University of California, Irvine. Fitch is one of the founders of the field of molecular phylogenetics, a field that examines and reconstructs the evolutionary relationships among organisms based on molecular information. Among many contributions, Fitch developed methods for diagramming the gene-based relationships among species, what are known as "phylogenetic trees."<sup>128</sup> When he was hired in 2005, the proposed research centered on predicting the evolution of seasonal, human influenza. But soon after he was hired, the highly pathogenic avian influenza H5N1 was found in wild birds in Europe, increasing attention and concern about a possible pandemic. Wallace was assigned to map the phylogeny of avian influenza H5N1 on the territory of the globe, a project that Fitch and he together would call a "phylogeography" of the virus.

In order to construct this phylogeography, Wallace and his lab-mates needed two points of data about H5N1 virus isolates: genome sequences and geo-spatial locations. They accessed this data by downloading information about H5N1 virus isolates from GenBank, an "open-access" database of nucleotide sequences and their protein translations "with bibliographic and biological annotation," and from the similar Influenza Sequence Database at Los Alamos National Laboratories.<sup>129</sup> One of the first "open-access" scientific platforms, GenBank embodies the norms given in classical definitions of open-access: that data should be available free of charge, and without any significant barriers to re-use.<sup>130</sup> In an article on the history of GenBank, NCBI staff members Jo McEntyre and David J. Lipman note that " GenBank demonstrates that, even in the fiercely competitive world of science, researchers recognize that contributing to large, shared data sets ultimately benefits everyone. . . . Scientists have shown a willingness to place data in a community archive for the common good, knowing that it can be freely used by anyone."<sup>131</sup>

As the H5N1 virus spread around the world, Wallace later told me, influenza data had accumulated "pell mell":

“In one country, you'll have extensive sequencing of all the genomic segments, all from one end to the other; in another country, there's an outbreak, and they just give enough in order to be able to get a handle on whether or not it's H5N1. So, some years in one

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<sup>127</sup> See Deborah Wallace and Roderick Wallace, *A plague on your houses: How New York was burned down and national public health crumbled* (New York: Verso, 1998).

<sup>128</sup> William R. Atchley, "Retrospective: Walter M. Fitch (1929-2011)" *Science* May 2011, 332 (6031): 804.

<sup>129</sup> Subsequently, the Los Alamos ISD was restricted to Los Alamos or other approved researchers, removing this database from the "open-access" domain.

<sup>130</sup> See Jo McEntyre and David P. Lipton, "GenBank: A model community resource?" *Nature Webdebates*, <<http://www.nature.com/nature/debates/e-access/Articles/lipman.html>>

Peter Suber, *Open Access* (Cambridge, Ma.: MIT Press, 2012).

<sup>131</sup> Jo McEntyre and David P. Lipton, *op cit*.

country are better than others, some years they'll just focus on poultry, and not do wild birds, and so you end up with a kind of, catch as you catch can combination, list of sequences, and so its very much a convenience sample, you do what you can, and try to learn something from it.”

Although open-access platforms such as GenBank are often justified in the language of longstanding norms of free scientific exchange or the commons, the technical structure of these systems actually restructure relations of knowledge, power and ethics.<sup>132</sup> Wallace's adoption of the phrase "convenience sample" captures his lab's passive relation to the data. But unlike some form of "convenience sample" which are based on sampling a population close at hand, Wallace's sample also relied on an *externalization* of sample collection onto others. This reveals that the objectivity of open-access is dependent on the practices of others. The scope of Wallace's research was delimited by each country's sampling techniques, surveillance objectives, and willingness to publish on open-access data banks. Although the length and detail of the sequence code was no doubt most important, Wallace added that supplemental information was also valued--and in his case particularly the geo-spatial locations where viruses were sampled. Because of variations in sampling and reporting procedure, the scale of locations reported ranged from city (Hong Kong) to province (Fujian) to country (Vietnam).

Wallace and his colleagues downloaded 192 influenza A H5N1 hemagglutinin and neuraminidase gene sequences from GenBank. These sequences were isolated from a variety of host types for the years 1996-2005, and derived from locations across Asia and Europe. A phylogeography begins with the construction of a phylogenetic tree. The figure of a "tree of life" describing the relations of descent and generation among living beings or species is a tool originally created by the transposition of juridical notions of heredity (i.e. *inheritance*) into the life sciences. The lone image in Darwin's *Origin of the Species* employs an early example of such a figure to describe relationships of speciation in the process of evolutionary descent.<sup>133</sup> The consolidation of the understanding of evolution in terms of genes and later DNA codes radically transformed these trees by enabling the *quantification* of relatedness among tree branches. Contemporary phylogenetic trees are not only descriptions of structural relations among species, but statistical accounts of degrees of relatedness and differentiation over time.<sup>134</sup>

A phylogeography maps a phylogenetic tree onto geographic space. Wallace and his lab partners correlated the genetic variation of each virus sample (quantified in the phylogenetic tree) with the time and place where the virus was sampled. By mapping relations of descent onto spatial location, they claim to be able to map the "migration" of the virus. In publication, the representation of their findings on a Google Earth globe show "migration events" for hemagglutinin, neuraminidase, or both molecules linked by arrows of yellow, orange or green, most thrusting outwards from southern China. Wallace recalls he was amazed when he began to turn the phylogenetic trees into spatial maps.

“It was amazing, the first maps I made out of that, two things, it looked exactly like what we knew about the spread of the virus out of southeast Asia, out of China, into eastern China, up

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<sup>132</sup> Chris Kelty, *Two Bits: The cultural significance of free software* (Durham, NC: Duke University Press, 2008).

<sup>133</sup> Staffan Muller-Wille and Hans-Jorg Rheinberger, *A cultural history of heredity* (Chicago: University of Chicago Press, 2012). It is important to note that the transposition inverted the structure of the tree: where juridical diagrams of descent traced backwards from "Ego" to a multitude of ancestors, figures of evolutionary (species) descent begin from one or a small number of "roots" and end in a multitude of branches.

<sup>134</sup> Ibid.

into lake Qinghai and across Siberia. At the same time, we were [also] able to discover migration events, that had not been in any way explained.”

This was a new method to analyze what Kennedy Shortridge called the "influenza epicentre:" the hypothesis that southern China is the origin of pandemic influenza viruses. In the final article, published in the journal *Proceedings of the National Academy of Science*, Wallace et. al. write that their maps provide a "finer distinction" of the hypothesis that southern China is an influenza epicentre: they specify Guangdong Province as "the prime source of H5N1's diversity and diffusion."

This ascription was not made into a vacuum. When the UC Irvine Press Office issued an announcement summarizing the forthcoming article, news media from around the world picked up the story. For example, Agence France Presse reported

“US researchers have reconstructed the evolution of avian flu and its spread over the past decade from its first origins in southern China, according to a new study. The team from Irvine University in California (sic) combined genetic and geographic data for the H5N1 virus, identifying many of the migration routes through which the strains spread across Asia and then around the globe.” (AFP-quoted on the blog “Effect Measure”).

Within a few days, officials from Guangdong Province Agricultural Department organized a press conference to publicly criticize the findings of the paper. The language they used focused critical attention on the paper's status as credible science, calling the paper "unscientific" and its "findings . . . not based on facts."

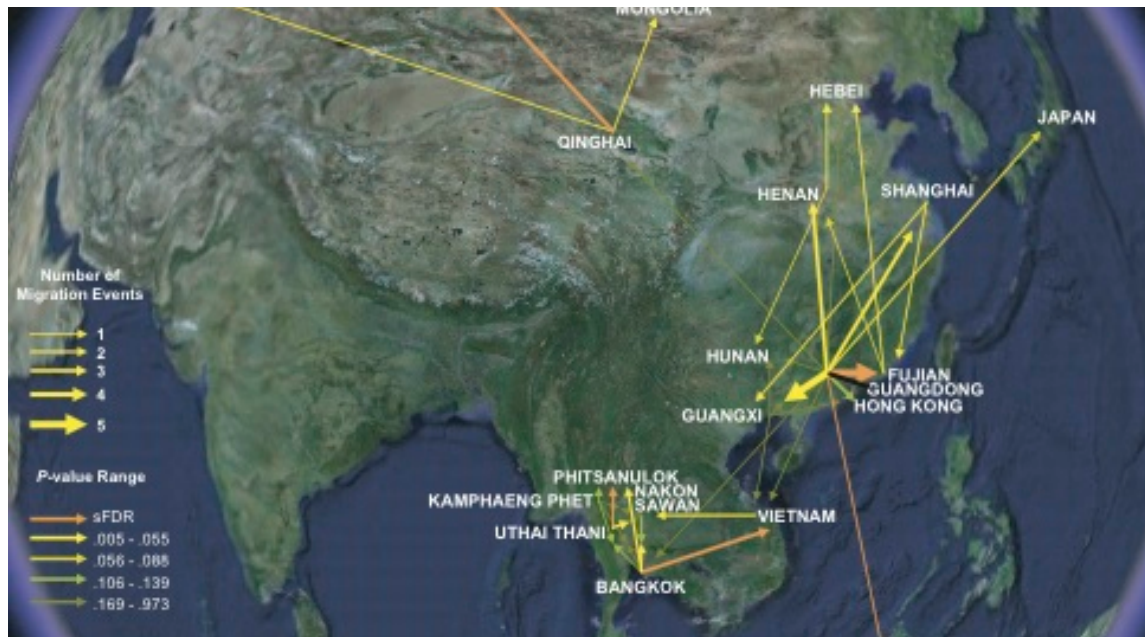


FIGURE 1  
Wallace's map.  
Image from Robert Wallace, et al, *op cit.*

"The findings, which say Guangdong is the source of multiple avian flu virus strains spreading both regionally and internationally, are the wrong conclusion to the evidence and lack credibility," He Xia, a spokesman for the Guangdong Agriculture Department, told the English-language China Daily.<sup>135</sup> The allegedly scientific claim about the geographic source of the virus, understood as an accusation of geopolitical blame, was now repositioned as one side of a polemic dispute.

Wallace entered into and confirmed this polemic debate a few months later. The occasion was the proposed revisions of the nomenclature system used to classify avian influenza H5N1, put forward by a joint working group organized by WHO, FAO and OIE. As I described in Chapter 1, influenza is highly variable, and the system for its classification has become increasingly refined since the virus was discovered in 1931. Each nomenclature system seems to find its limit in the variability of the virus, particularly in the ability of the virus to mark significant difference by evading vaccination regimes. Following the emergence of H5N1, different names had been used by different labs in their publications, among which some had referred to new strains according to the place where they were first isolated: viruses closely related to a strain isolated in Fujian were named "Fujian-like". The WHO/FAO/OIE nomenclature group was established to accomplish two tasks. First, the group hoped to develop a standard naming system that would be used by all laboratories, thereby improving "discussion and comparison of the various lineages." But the group also had a second task: to "remove stigmatizing labeling of clades by geographical reference."<sup>136</sup>

In a text that he formatted himself in the style of a journal or scientific magazine article and posted on a public blog, Wallace linked the WHO/FAO/OIE nomenclature revision with the denunciations that his paper had received from the Guangdong government. He argued that despite some of the scientific advantages of the new nomenclature system, geographic information should not be removed from the naming of diseases.

"Locality has meaning beyond where the pathogen happened to originate. Local conditions imposed by public policy and social practice shape viral evolution." Wallace wrote. He ended with a plea:

"'Qinghai-like', 'Fujian-like', such names should remain intact, if anything as reminders that bird flu has specific origins. The best way that Chinese or any other government can avoid the stigma of nomenclature is to devise and enact means by which to keep the next strains from emerging in the first place. There would then be no virus to name."

Wallace here argues for science as a tool not only for constructing natural facts but also for attributing moral responsibility. Stigma, if scientifically accurate, is simply guilt. For this reason, any limitation of access to data or attempt to restrict publication is perversion, a turning away from truth. In an ideal world, science should guide politics; in a perverted one, politics interferes with science.

The unrestricted exchange of information is a longstanding "norm" of modern science--at least in the sense of an ideal or value, if not a social structure. But the "informationalization" of life has transformed the media of these exchanges, producing new situations of normative uncertainty. Security planners are increasingly concerned about the possibility that biological

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<sup>135</sup> "Authorities deny claim on disease" *People (Renminwang)* March 7, 2007.

[http://english.peopledaily.com.cn/200703/07/eng20070307\\_354948.html](http://english.peopledaily.com.cn/200703/07/eng20070307_354948.html) Accessed on: March 9, 2013.

<sup>136</sup> WHO/FAO/OIE H5N1 Evolution Working Group, "Towards a unified nomenclature system for the highly pathogenic H5N1 avian influenza viruses." Powerpoint presentation.

knowledge, stored in the form of sequences or other codes, can be reconstructed into biological substances. Circulating biological information in an "open" public, they argue, is therefore as dangerous as circulating the biological materials themselves.<sup>137</sup> Recently, U.S. researchers who experimentally created new "highly transmissible" forms of the H5N1 virus were momentarily prohibited from publishing their research because of security concerns. When some scientists complained that the prohibition amounted to improper interference with scientific freedom, however, the papers were allowed to go to press in a slightly modified form.<sup>138</sup> In Wallace's case, we see how the discourse of scientific freedom can shift from the language of rights to become a command addressed toward others. In his essay, Wallace *demand*s that sequence data be made public, denouncing any limit on transparency as "perverting of science for political gain."

But despite the passion of this denunciation of Chinese obscurantism, Wallace's judgment must be understood in terms of two aspects of the situation: first, the logic of exchange embodied in the "open-access" databank; and second, the earlier polemical denunciation by the Guangdong Province officials. Open-access databanks such as GenBank, while promoted in the language of scientific norms of free exchange, significantly transform the relations of knowledge, power and ethics in scientific practice. By providing access to Chinese samples without requiring any relationship to the Chinese laboratories who conducted sampling, the open-access data bank allowed Wallace's findings to return to China in the form of an unannounced and unwelcomed press report. Indeed, in the published journal article Wallace, et al (2008) do not cite or specifically credit the laboratories who originally sampled and sequenced each of the viruses, beyond an anonymous expression of gratitude: "We also thank the hundreds of scientists and technicians involved in sequencing the nucleotide samples used in this study."<sup>139</sup> As I show later in this chapter, the distribution of credit was a central concern for Chinese scientists involved in disputes over the truth of influenza research.

Second, once the Guangdong Province Department of Agriculture had denounced his article as "unscientific," Wallace found himself as the other side of a polemic situation. His response was in part determined by this polemic situation: he simply inverted the Chinese denunciation. When the Chinese questioned the scientificity of his claims, he described Chinese actions as political perversion of science. Both claim and counter-claim accuse the other side of distorting science for personal or political ends. As a result, they share a notion of science as a space purified of politics and interests.

Finally, I argue that these two aspects of the situation--the 'open access' data bank, and the polemic dispute, are connected as two sides of the same coin: obscurity and obstruction are, after all, merely the inverse of transparency and unrestricted access. The question then becomes whether other forms of transaction and other modes of evaluation could appear in transnational research on H5N1 avian influenza in China.

### *Variation on a theme: Polemic and Affinity*

Wallace's confrontation with a Chinese scientist or government official was not the first polemic to take shape amidst the exploding quantity of transnational research on influenza H5N1. In late 2005, members of Robert Webster's lab published in the journal *Virology* the

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<sup>137</sup> Carlo Caduff, "The semiotics of security," *op cit*.

<sup>138</sup> Gaymon Bennett, "H5N1 and the politics of truth," *op cit*.

<sup>139</sup> Wallace, et al, *op cit*: 4478.

results of a study investigating the pathogenicity of avian influenza H5N1. Elena A. Govorkova and her co-authors infected ferrets with thirteen distinct H5N1 isolates (four isolated from humans, and nine isolated from poultry), monitored the health of the infected ferrets, and “sacrificed” animals with signs of severe disease. Many of the ferrets fell sick, some severely. The authors concluded that “These results show that avian H5N1 viruses circulating among birds in Asia can be highly lethal to mammalian hosts and thus can potentially be transmitted from avian species to humans.”<sup>140</sup>

However, Chinese scientists at the Ministry of Agriculture routinely surveying the literature on avian flu discovered that the Govorkova paper used virus samples isolated by a Chinese lab without citation or provision of credit.<sup>141</sup> Although the authors stated that “13 different H5N1/04 influenza viruses isolated from humans and birds in Vietnam, Thailand, Hong Kong, and Indonesia were included in this study,” the study had also included two viruses isolated in mainland China: A/duck/Hunan/1504/04; and A/duck/Guangxi/1304/04.<sup>142</sup> According to an investigation of the controversy published in the *Wall Street Journal*, Chinese administrators realized these samples must have come from the main Chinese laboratory for research on avian flu, the Key Laboratory for Avian Influenza at the Harbin Veterinary Research Institute.

Although both Drs. Webster and Govorkova apologized for what they called a “mix-up,” the incident did not end there. When the WHO requested samples of influenza viruses taken by MOA scientists (at the Harbin lab) from dead wild birds on Qinghai Lake, Dr. Jia Youling, the Chief Veterinary Officer at the MOA's Veterinary Bureau, refused, citing the Govorkova incident and arguing that China had proprietary rights over samples taken by Chinese scientists on Chinese territory. He suggested the isolates could later be shared, but only if an appropriate mechanism for providing credit to Chinese scientists was created.<sup>143</sup>

China was not alone in refusing to provide virus isolates to the central laboratories of the WHO Global Influenza Surveillance Network. In 2007, Indonesia withdrew from the GISN. In a co-authored article explaining the rationale for refusing to share samples, Indonesian Health Minister Siti Fadilah Supari and other Indonesian biomedical experts state that “Since April 2006, incidents that led to Indonesia’s discontentment towards the WHO system for global influenza surveillance came one after another.” These included presentations or publications of laboratory results from studies involving samples of H5N1 isolated in Indonesia without acknowledgement of Indonesian scientists; and above all an Australian pharmaceutical company's production of a vaccine for avian influenza H5N1 based on an “Indonesian (vaccine seed) virus” that had been shared with the WHO laboratory network.

To justify their refusal to distribute samples taken on their territory, the Indonesian government invoked an alternative interpretation of the requirements impinging on sovereign states under the 2005 International Health Regulations. The earliest International Health Regulations are agreements that date to the 19th century and required each sovereign state to report any cases of so-called “pestilential” diseases: the list usually limited to cholera, plague, and yellow fever. As David Fidler argues, they are classic examples of what he calls “Wesphalian” public health: an international agreement aiming to ensure security against disease

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<sup>140</sup> Elena A. Govorkova, et. al., “Lethality to ferrets of H5N1 influenza viruses isolated from humans and poultry in 2004” *Journal of Virology* Vol. 79, No. 4 (2005): 2191.

<sup>141</sup> Nicholas Zamiska, “How academic flap hurt world effort on bird flu” *Wall Street Journal* (27 February 2006).

<sup>142</sup> Govorkova, et al, *op cit.*

<sup>143</sup> Zamiska, *op cit.*



with minimal interference on trade or traffic, but with limited authority to intervene in sovereign states.<sup>144</sup> The IHR (2005), by contrast, requires the reporting of any "public health emergency of international concern," and provides a "decision instrument" to help identify such emergencies. The World Health Assembly meetings in May 2006 and May 2007 called for the sharing of biological samples as the only way of adequately conducting global surveillance during public health emergencies. In justifying their refusal to participate in the global surveillance system, the Indonesia health officials distinguish "public health information" from "biological substances": "public health information means knowledge and facts," they write.<sup>145</sup> Biological substances, by contrast--in this case viruses or viral samples--are considered "resources" that belong to the sovereign state on whose territory they appear. As a result, removal of these resources from that territory should be regulated under the rules of the Convention on Biological Diversity. In subsequent years, Indonesia only allowed samples to be distributed outside of its territory if accompanied by a Standard Material Transfer Agreement that protects copyrights and ensures fair-use of products.<sup>146</sup>

The Indonesians marked "biological substance" as belonging to the nation; they propose the *nationalization* of the viral isolate. Drawing largely on a postcolonial discourse developed in struggles over environmental and biodiversity protection, they critique practices of resource extraction and the inequalities built into the global influenza surveillance and vaccine production system managed by WHO.<sup>147</sup> However, due to the growth of life science research in Asia, China's disputed influenza samples have raised new and distinctive questions about matter and value.<sup>148</sup> In what Nancy Chen calls the "biotech bloom" underway in Asia today, tension exists between governmental efforts to assert national sovereignty over the science of life and its products, and the pursuit of scientific "wealth and power" through collaborations with foreign scientists.<sup>149</sup> Avian influenza--precisely because of its apparently immanent danger to the world--has become one of the central sites for expanding trans-national bioscientific collaborations in Asia. What is notable about the Govorkova affair is that the Chinese did not seek access to "high-tech" products such as vaccines from the "developed" world--indeed, the Harbin lab is one of the world's largest producer of avian influenza vaccines for poultry--but rather sought *credit* for their research contributions.

Polemic can be quickly replaced by negotiated exchanges of matter, information, and credit, or new forms for sharing work. Most of the participants interviewed about the Govorkova affair (on both the Chinese and the American side) indicated that the incident was now "in the past" and that new working relationships were already in practice. The *Wall Street Journal* article ends with a quotation from Dr. Julie Hall, a communicable-disease expert for the WHO based in Beijing.

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<sup>144</sup> Fidler, *op cit.*

<sup>145</sup> Endang R Sedyaningsih, Siti Isfandari, Triono Soendoro, Siti Fadilah Supari, "Towards mutual trust, transparency and equity in virus sharing: The avian influenza case of Indonesia" *Annals of the Academy of Medicine Singapore* Vol 37 (2008): 484-5.

<sup>146</sup> Rachel Irwin, "Indonesia, H5N1, and Global Health Governance" *Global Health Governance* Vol. 3, No. 2 (Spring 2010): n.p.

<sup>147</sup> On environmentalism and biodiversity in Indonesia, see Celia Lowe, *Wild profusion* and Anna Tsing, *Friction*. See also Cori Hayden, *When nature goes public: the making and unmaking of bioprospecting in Mexico* (Princeton: Princeton University Press, 2003).

<sup>148</sup> On the growth of life science research in Asia, see Ong and Chen, eds., *Asian Biotech*; and the articles included in a special issue of *East Asian Science Technology and Society*, Greg Clancey, ed., "Asian Biopoleis."

<sup>149</sup> Nancy Chen, "Biotech Bloom" in Ong and Chen, eds., *Asian Biotech*.



“It's like a marriage,” Dr. Hall says. “You have to keep working at it.”<sup>150</sup> Dr. Hall is certainly correct to draw on the language of *affinity* to describe the formation of trans-national scientific collaborations. She is also astute in calling attention to a meta-level of practice that works on the *relationships* that organize scientific practice, in addition to the objects of scientific knowledge. But although the idiom of “working at it” raises the right questions, it remains trapped within its metaphoric connotation of marriage counseling in the American suburbs. As anthropology has long shown, marriage itself is a highly variable form.<sup>151</sup> The question is precisely *how* relationships of affinity are formed and claimed, and how they are made into objects of strategic practice and critical reflection.<sup>152</sup>

*Part Two: Detour and Access*

Running along the header of every document issued by the Food and Agriculture Organization, Emergency Center for Transboundary Animal Diseases-China (ECTAD-China) is



FIGURE 2: Empty your cup

a remarkable logo. On a red ground, drawn in white silhouette, a chicken and a duck stand face to face. In their hand-like wings, the birds hold glasses of red wine aloft and barely touching as if only a moment ago each had urged their counterpart to drink up in a celebratory toast.

The logo was made by ECTAD-China's Senior Technical Coordinator, Vincent Martin, with the help of a design company, and expresses normative ambivalence towards relationships of communion and communication.<sup>153</sup> For anyone familiar with theories of influenza emergence

150 Nicholas Zamiska, "How academic flap hurt world effort on bird flu" *Wall Street Journal* (27 February 2006).

151 For example, Lewis Henry Morgan, *Systems of consanguinity and affinity of the human family* (Nebraska University Press, 1997[1868]; Claude Levi-Strauss, *Elementary forms of kinship* (Boston: Beacon Press, 1969).

152 Drawing on Bourdieu's engagement with structuralist theories of kinship in Bourdieu, *The Logic of Practice* (Princeton: Princeton University Press, 1980).

153 By communion I mean relationships organized around shared substance, which has its paradigmatic form in the ritual collective consumption of food. See W Robertson-Smith, *Religion of the Semites* (New Brunswick, N.J.: Transaction Publishers, 2005). Where communion involves the "interpenetration or merging of subjects," communication involves their "individuation." Tim Ingold, *Evolution and social life* (Cambridge: Cambridge University Press, 1986). Communication demands difference as its precondition: otherwise there is only repetition

during this period, the clinking of glasses by duck and chicken carries an unsettling irony, because research suggests that contact between ducks and chickens is central to the emergence and persistence of H5N1 avian influenza.<sup>154</sup> Yet, the logo also evokes, more happily perhaps, the importance of collaboration as an ideal for Martin and the ECTAD office. When I first met Martin, I was struck by his use of the word "collaboration" as a dominant norm guiding his work in China. Telling me about "One World, One Health" principles, he noted that he faced two challenges, which at the time seemed to me surprisingly heterogeneous: veterinarians don't want to work with medical doctors; and China doesn't want to share influenza sequence data with the FAO. As the logo seems to iconize, Martin's approach to China's unwillingness to share sequence data was not denunciation, but rather the labor of building relationships. In this reading, the wine shared by the two birds stands for the shared work across disciplines or sovereign nations. And in the many FAO ECTAD meetings or events I attended in Beijing, often held jointly with the Chinese Ministry of Agriculture, this metaphor often approached the literal, when Martin or the Chinese hosts invariably stood and asked us, in the Chinese phrase, to *ganbei* (empty our cups).

Drinking, and especially drinking *baijiu* (white liquor), is well known as an important part of banqueting in Chinese business and bureaucratic circles. Banquets are sites for cultivating *ganqing* (sentiment-relations)<sup>155</sup> among participants and extending *guanxi* relationship networks.<sup>156</sup> Not simply instrumental, they are sites in which reciprocal exchange of 'toasts' (*jingjiu*) produces new spaces of shared sentiment that can persist long after the glow of the alcohol fades away. Banquets as a social practice are not restricted to the business world, but are also common within even professional and "scientific" sectors such as medicine<sup>157</sup>, public health<sup>158</sup>, and the state veterinary services that I followed.

Discussions of *guanxi* and banqueting are ubiquitous in the anthropological literature. Usually depicted as a particularly "Chinese" cultural form--even if one that has grown in importance during the post-Mao era--little attention has been given to the role of banqueting in *transnational* spaces, beyond anthropologists reflecting on their own experience as a "foreign guest" at banquets. But as I learned from talking with members of ECTAD-China and during my participation at many of their events, banquets were important parts of getting their work in China done--including stimulating exchanges of sequences and samples. Martin told me, for example, that the ECTAD-China logo was partly an allusion to the "*baijiu* parties" they conducted to get their projects approved in various provinces. These *baijiu* parties were important sites for breaking through boundaries of difference, including political boundaries, in order to make "collaboration" possible. More importantly, they reworked transnational relations of communication without resorting to the unconditional demand for transparency.

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of the same. But as Ingold remarks, the anthropology of sacrifice has shown that techniques of communion and communication "may be present simultaneously, expressing respectively the conjunction and disjunction of man and God."

<sup>154</sup> See, for example, Marius Gilbert, et al, "Free-grazing ducks and Highly Pathogenic Avian Influenza, Thailand" *Emerging Infectious Diseases* 12(2) 2006.

<sup>155</sup> Andrew Kipnis, *Producing Guanxi: Sentiment, Self and Subculture in a North China Village* (Durham, NC: Duke University Press, 1997).

<sup>156</sup> Mayfair Yang, *Gifts, Favors, and Banquets: The Chinese Art of Social Relationships* (Ithaca, NY: Cornell University Press, 1994); Yunxiang Yan, *The Flow of Gifts: Reciprocity and Social Networks in a Chinese Village* (Stanford: Stanford University Press, 1996).

<sup>157</sup> Judith Farquhar, *Appetites: Food and Sex in Post-Socialist China* (Durham, NC: Duke University Press, 2002).

<sup>158</sup> Katherine A. Mason, "To your health!": Toasting, Intoxication, and Gendered Critique among Banqueting Women" *The China Journal* No. 69 (2013): 108-134.

One of the first projects of the ECTAD-China office was a 2009 research trip to Poyang Lake to collect samples of H5N1 virus. The first request for research clearance sent to the Ministry of Agriculture was flatly refused. But Martin was not deterred, nor did he make a public row about transparency. "You talk to a few people, drink a little *baijiu*, and all of a sudden you're among the birds at the lake," he told me, as we sat in his office in Fall 2010. A report later published in the first issue of *China HPAI Highlights*, the newsletter of the ECTAD office, reported the mission a success, specifically in the terms of "collaboration."

"The project, funded by Sweden, collected samples from domestic ducks farmed near Poyang Lake and was very successful in bringing together mission members with Jiangxi Province veterinary services and local veterinary authorities to work collaboratively in the field. All parties were ultimately able to agree on the methodology; and, proper sample collection techniques were demonstrated and used in executing the project. Additionally, the project was planned and implemented successfully in a relatively short period of time. The working relationships formed as a result of this project are very valuable to FAO's continued work in China to combat HPAI. This field mission can be considered a success on many fronts, and we believe that similar field data collection efforts in the future are more likely to be accepted and successful as a result of this project. Approximately 60 epidemiologists and laboratory staff were trained during this mission."<sup>159</sup>

The "baijiu parties," then, are activities that precede and condition the cultivation of "working relationships" and the possibility of subsequent exchanges, comparable perhaps to the "opening gifts" of the Kula system described by Malinowski.<sup>160</sup> The ECTAD-China office structured these subsequent exchanges in a particular form. The form of transaction reflected the FAO's broader mission to encourage development through transfer of science and technology, and particularly the organization's more recent reorientation of technical assistance toward a logic of emergency response. In the case of the Poyang Lake research project, the "training" of epidemiologists and laboratory staff produced a hierarchical and pedagogical form of "collaboration." But developmentalism and "technical cooperation" are here resituated as one side of a reciprocal exchange. As the Poyang Lake project described above shows, "training" is exchanged for access to research sites and materials, and in particular, for taking biological samples from birds.

A field experiment designed by Martin to produce a "network analysis" of the live bird markets of southern China also involved the exchange of training for access to research sites and pathogen sampling. Martin selected the provinces where the study was carried out in 2009--Guangxi, Hunan, and Yunnan--based on two criteria: first, because these provinces were identified by the geo-spatial risk models as high risk for avian influenza<sup>161</sup>; and second, because those provinces agreed to the training, when wealthier provinces (such as Guangdong or Shanghai municipality) refused offers of technical assistance.<sup>162</sup> In each province, two Chinese

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<sup>159</sup> *Avian Influenza HighLights from China* (January 2009). [Later re-classified as *China HPAI Highlights* Vol. 1 (January 2009).

<sup>160</sup> Bronislaw Malinowski, *Argonauts of the Western Pacific* (New York: E.P. Dutton and Co., 1922).

<sup>161</sup> See discussion in Chapter 1.

<sup>162</sup> On the autonomy and confidence of wealthy Chinese provinces and municipalities in the domain of public health, see Katherine Mason, "Becoming modern after SARS: Battling the H1N1 pandemic and the politics of backwardness in the Pearl River Delta" *Behemoth* No. 3 (2010): 9-35.

veterinarians employed at the ECTAD-China office in Beijing conducted the training of provincial and municipal veterinarians in basic sampling procedures. On a second day, the samples taken at the actual market during training exercises doubled as sample collection for the study. These samples were sent to the Harbin National Key Laboratory for sequencing, and the Harbin Lab reported results to the ECTAD office (without providing access to samples themselves). Meanwhile, one of the ECTAD veterinarians conducted surveys of marketers in order to find where birds came from and where they went. In doing so, although herself Chinese, she relied on the Province and municipal trainees to communicate with the farmers who spoke only in regional dialects. In both cases, the act of training embodied an exchange: for in the process of conducting the training, the samples were taken and survey answers collected that later became the data used in the study.

The collection of data for the live bird market study (usually referred to in abbreviated form as "the LBM study") by Chinese veterinarians employed by the ECTAD office raises the point that collaboration worked across a series of distinctions between "China" and "international" rather than a single encompassing boundary. The distinction between "international" and "national" is drawn throughout the administrative structure of the FAO. As an international organization, the FAO works almost without exception in collaborations with specific *national* states, and agreements for "technical cooperation" programs are contingent on invitations from a sovereign nation state. The ECTAD system began with a single office in Rome, the city that houses the central office of the FAO (Rome is to FAO what Geneva is to WHO). Only later were "regional" (e.g. Bangkok or Nairobi) and "national" offices set up in China and Vietnam. But the "international" and the "national" is also a marker distinguishing two kinds of staff members in the office. In the ECTAD-China office, other than Martin, every member of the ECTAD staff is a Chinese citizen and is referred to as "National" staff. When hiring calls were posted for such positions they specified the attribute "National" in the job title, meaning that they sought a citizen of the host nation. For example, Gao Lili, who conducted the surveys for the market network analysis study, held the official title of "National Veterinary epidemiologist." Non-Chinese who were brought to China to conduct "missions", assessments, lectures, or training courses were by contrast usually marked as "international experts."<sup>163</sup>

Inside the ECTAD-China office itself, the layout articulated an idealized version of the relationship between "international" and "national" staff. The main office is a single open room containing eight or ten cubicles. The "national" staff work at these cubicles, mostly on computers. One side of the room is lined with windows. On one end of the room, a glass office with a transparent, but closed door contains the desk of Cai Haifeng, the most senior of the national staff. Next to Cai's office another glass room contains a conference table. On the other end of the main room, a small opening leads into Martin's corner office, separated from the rest of the office by only a short baffle passage rather than a door. This passageway gives the "international" Senior Technical Coordinator<sup>164</sup> a physical independence from the rest of the office--sound and sight are blocked--while maintaining an "opening" for communication. Sometimes a staff-member would enter with a question or piece of news, or Vincent would pass outward to pick up a print-out or follow-up on an e-mail with a staff member. Unlike the glass

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<sup>163</sup> For example, see "International Health Experts Visiting China" *China HPAI Highlights* Vol. 9 (September 2009). The article reports that "Three animal health international experts . . . . visited China . . . ."

<sup>164</sup> Martin's "international" status is actually double: not only is he marked as international in contrast to the national staff of his own office, but also the ECTAD-China office itself is bureaucratically classified underneath the "international" ECTAD office rather than the FAO-China national office.

walls of the conference room which expressed an ideal of transparency and a lack of boundaries, the Senior Technical Coordinator's office facilitated communication while reinforcing the distinction between "international" and "national" status.

The boundary between "international" and "national" structured the scientific activities of the office, as well. During the LBM study, all data collection and training missions were conducted by senior "national" staff members, Cai Haifeng and Gao Lili. Laboratory sequencing was conducted by the Harbin Key Laboratory for Avian Influenza. Once sequences and surveys were back in the Beijing office, however, the analysis was conducted by Vincent Martin with the help of Ricardo Margalhaes, an "international veterinary epidemiologist based in Queensland University."<sup>165</sup> Margalhaes only came to China for brief visits of five days or less, which he organizes in order to accomplish all of the tasks that require collaboration. He works on the rest of the numbers back in Australia. In this way, the ECTAD office's organization reflected the division of labor often noted by postcolonial scholars of science and development, in which materials and data are provided by "national" scientists while analysis, writing and publication are undertaken by "international" scientists (usually from Europe, North America or Australia).<sup>166</sup> Indeed, when Gao Lili explained the results of the study to me, she further cemented the separation between the knowledge practices of the national and the international scientists. She had collected the data and went into detail describing her experiences at the markets. She also confidently told me the *results* of the study which she took as established fact. However, she noted that she "won't speculate or comment on the statistical methodology employed by Vincent and Ricardo."

### *The predicament of culture*

Banquet dinners and the drinking of *baijiu* in transnational encounters did not merely break down barriers. It is more accurate to say that they marked differences in a way that enabled communion and communication across difference. Participants in transnational banquets figure banqueting as particularly Chinese. Moreover, this figure is deployed in ways that actively changes how transnational relationships are understood and practiced. On the part of Chinese participants, the ability of "foreign guests" to participate in, and better yet, to master the arts of politesse involved in *baijiu* drinking reflects recognition of China. "He can drink *baijiu*, and he can even speak some Chinese," one provincial veterinary officer told me about Martin, approvingly, explaining why he liked working with him. On the other hand, for Martin himself, Chinese "culture" is an ambivalent but central part of his work, both attractive and troubling.

For most of his time working at ECTAD-China, Martin lived in one of the oldest sections of the center city, in a *hutong*<sup>167</sup> near the Confucius and Lama Buddhist Temples. His wife and children lived with him in a modern "villa" nestled inside the maze of alleys and courtyard homes there. Later he moved to a courtyard near the North Lake in the very heart of the city.

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<sup>165</sup> "Applying Social Network Analysis (SNA) techniques to Live bird market surveillance and market chain analysis" *China HPAI highlights* Vol. 16 (April 2010).

<sup>166</sup> See for example Celia Lowe, *Wild Profusion: Biodiversity Conservation in an Indonesian Archipelago* (Princeton, NJ: Princeton University Press).

<sup>167</sup> Beijing's *hutongs* are small alleys lined with courtyard homes, most of which were divided up and redistributed to multiple families after the communist revolution. Recently, some have been reclaimed or bought over and full-size courtyard residences are reappearing.

An ancient structure, the inside had been painted blinding white and the wooden beams varnished by the English real estate investor who rented it to him. Glass walls close off the rooms from the inner courtyard. The furniture continued the theme of *chinoiserie* but was juxtaposed with a collection of African sculptures Martin had collected while working on the eradication of rinderpest in Kenya. One evening in early December 2011 we sat together enjoying French wine and cheese in the main room. My fieldwork was soon coming to an end and Vincent seemed tired. "These *hutong* buildings are impossible to heat," he muttered and pulled a space heater closer to us. He told me he was planning to leave China in June for good, to take up a position in the main office in Rome. His family had already returned to Europe.

When he reflected on his four years in China, Martin told me, he found a "contradiction" in his work with Chinese counterparts, a contradiction that ran as far as his own office. On the one hand there were projects like the Poyang Lake sampling project, where the *baijiu* parties led to unprecedented access to sampling in a key avian influenza site. But in other moments, he was frustrated that outbreaks still were not being publicly reported and other projects were rejected. For example, in March of 2011 Martin returned from a vacation in Thailand to find a Foot and Mouth Disease (FMD) crisis on his hands. Outbreaks had been reported in Mongolia, Southeastern Russia, and even in North Korea. But China had not reported a single case--despite the fact that the highly contagious virus had been found at all of the bordering regions around China's northeastern province of Helongjiang. "I think we are going backwards here," he lamented at the time.

Martin saw this contradiction as in part shaped by "culture." In one e-mail exchange, I had pointed out how my research studied science as a cultural practice. In his response, doubtless conscious he was speaking to an anthropologist, Vincent picked up the language of culture to identify Chinese "cultural practices" as a blockage to full and free communication.

"I agree with you regarding the cultural practices and there was another dimension I was thinking about lately which is the way countries and government deals with sanitary information and their own cultural approach to what we call "transparency". I am not sure it would lead to any sort of interesting findings but I am really thinking sometimes (especially when we talk about China) that this notion of transparency and information sharing might need to be revisited in light of cultural practices (or maybe it has already been done). In the same vein, the cultural and ethical approach to science is also of interest: we know that hiding a disease or not sharing virus sequences is easier (in the short term) than doing it and I was also wondering whether we are missing a point, as western countries, when we pretend that everybody should play the game and be transparent."

The notion of cultural difference was a tool to explain China's seemingly contradictory approach to "transparency," to make it reasonable and to encourage approaches to organizing science that went beyond the norm of transparency. Francois Jullien has remarked on the "indirect" approach to meaning in Chinese discourse, which he claims allows us to reflect on the boundaries within which Western thought has grown. In particular, Jullien argues that Chinese "detours" in discourse reveal a mode of thinking that does not prioritize language as a representation, more or

less transparent, of essence or nature.<sup>168</sup> I do not reach to Jullien's *Detour and Access* in order to confirm or bolster my observations of "the Chinese" with theoretical and philosophical antecedents. Rather, I aim to elucidate how Martin himself, in a move that resembles Jullien's own more than a little, attempted to make the difference of Chinese culture "intelligible." The "contradiction" Martin faced in China was not only a blockage but could also be made productive. Understanding the Other in terms of cultural difference opened an entire register of practical work. In fact, by building relationships that recognized cultural difference, things could be communicated that apparently couldn't be said.

After the FMD crisis in March of 2011, the EU wanted to develop a training program for Foot and Mouth Disease, but China's Ministry of Agriculture refused without explanation. At the request of the diplomatic representative of the EU, a Frenchwoman who strongly supported the work of ECTAD, Martin got involved. As he was preparing the materials for the course, he told me:

"I found out that you cannot say "FMD" here. When you talk about FMD people just say, what are you talking about? But there are some people I know at MOA that I can talk to sometimes. It is not like there is simply no possibility of people talking, in certain situations they might speak very frankly. *You have to talk like its a joke or whatever.* And so I talked to these people I know and said, what is it about FMD, what is the problem with FMD? And they said there was no problem. But the problem it turns out was with the training program, because they said: we don't want to have experts fly in and say things to us we already know, or are too basic. Its true, the Chinese are too polite, they would nod their heads and smile and at the end of it say "Thank you very much" but at the same time in the back would be saying how ridiculous this was. In fact I agree, I once saw an EU training where I was like, guys, why are you talking at this general level? There are concrete things that the China MOA side want. So I got them to agree to a program on the model of the FETPV that would be incorporated within the FETPV training as one week within the April training. This is really my role. In other counties, EU will offer a lot of money for technical assistance for FMD without a lot of specifics attached and people will take it. But here it will be refused. So I first go to the China MOA and find out, what is it that you can say, what is it that you want? Then I go to the EU and try to get them to say and offer exactly that."

When Martin organized the "China Vietnam Forum on HPAI Management and Control," he again used strategies of detour that distinguished between private and public speech. On the one hand, the entire meeting was ordered and structured to make an argument. He hoped to convince China to replace compulsory universal vaccination with "targeted, risk-based vaccination." This was a response to an FAO international directive to all countries to specify an "exit strategy" for avian influenza H5N1. Who Martin invited and who he didn't reflected his effort to make the whole Forum into a persuasive discourse. One international expert was not invited because he consistently made strong claims for the importance of compulsory universal vaccination. And Vietnam was invited only because the FAO had recently initiated an experimental "risk-based" vaccination program there.

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<sup>168</sup> Francois Jullien, *Detour and Access: Strategies of Meaning in China and Greece* (New York: Zone Books, 2000).

In a sense, the "objective" of the meeting was completed before the meeting even began. On the last day of the meeting, the participants at the Forum were supposed to discuss and agree on a list of "Recommendations." Vincent actually wrote the recommendations in the few days before the meeting started.

"I had started writing the Recommendations over the weekend and then [China's Chief Veterinary Officer] Sun Yan called on the phone and said, "I think you should write some recommendations." I answered, feigning surprise, "What, you think I should right them *now*?" Sun Yan had been a bit shy about it, but I quickly agreed (of course, I was already writing them). Then I had to work late on Monday night to make sure I could get them done and send it around to Sun Yan to look over before the meeting. Then, the minute the meeting started, I sent a copy to the Ministry of Agriculture to get their comments, and they changed a few things."

Sure enough, at the end of the second day of the Forum, a photocopied pamphlet was passed around by the junior staff members of the FAO ECTAD office that stated in bold letters: RECOMMENDATIONS.

### *Recapitulation*

When the novel H7N9 avian influenza virus emerged this spring in Shanghai, narratives about China's position within the global health regime took on an unprecedented rosy color. *Scientist* magazine, among many others, praised China's "exemplary openness" in sharing of virus samples and sequence data. Within days of reporting the outbreak, Chinese scientists had posted numerous sequences on a new sequence bank known as the Global Initiative for Sharing All Influenza Data (GISAID). Although not mentioned in the news report, the creation of the GISAID sequence bank marks an important distinction from previously existing "open-access" banks like GenBank. The GISAID sequence bank was designed to bind the norm of transparency to symbolic and material recognition of the transactions of matter and information. The databank has even been actively supported by Indonesia as an improvement on other modes of transacting influenza information. GISAID is free-of-charge and in principle available to anyone, yet must be distinguished from "open-access" sequence banks because the system requires any user to "positively identify themselves" and agree to its basic principles of what it calls "scientific etiquette":

Admission to GISAID's publicly accessible platform is free-of-charge. It is accessible to anyone who agrees to its basic premise of upholding a scientific etiquette, acknowledging the originating laboratories providing the specimen and submitting laboratories who generate the sequence data, and ensuring fair exploitation of results coming from the data, and that no restrictions shall be attached to data that have been submitted to GISAID.<sup>169</sup>

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<sup>169</sup> The Global Initiative on Sharing All Influenza Data, <<http://platform.gisaid.org/epi3/start>>. See Peter Bogner, Ilaria Capua, David J. Lipman, Nancy J. Cox, et al, "A global initiative on sharing avian influenza data" *Nature* Vol. 442, No. 981 (31 August 2006).



A small incident soon after the Chinese scientists uploaded sequences onto the GISAID platform revealed how the form of disputes over influenza transactions was changing. The Chinese complained that other researchers (in Europe and Japan) were planning to use the uploaded sequences to publish papers of their own before the Chinese themselves had published. They threatened to halt sharing of virus samples and sequence information in response. Ultimately, through the mediation of GISAID, a compromise solution was found that allowed the Chinese team to publish hours before the Japanese-led team. Precedence in hand, the distribution of samples and uploading of sequences resumed.<sup>170</sup>

Through these norms of what they call "scientific etiquette," GISAID institutes a regime in which information is made to circulate in exchange for guarantees of credit and credibility. By all appearances, the discordances of "politics" and "culture" that troubled global health in China have thus been neatly resolved, or transvalued into the balanced reciprocity of sequences for scientific credit.<sup>171</sup> Latour and Woolgar, in their ethnography *Laboratory Life*, argued that scientific activity was driven by what they called "cycles of credit", in which scientific actors sought to accumulate credibility (through fact production) which could then be invested, like capital, into the further pursuit of credibility. Drawing on market models, they argue that scientific activity is not governed by norms. Rather, in scientific activity the only *telos* is increasing the turnaround of the cycle.<sup>172</sup> But as can be seen in the case of GISAID, the institution of norms is a precondition for credit to effectively circulate at all. The "global" is, in the end, itself a particular ethos.

This raises the anthropological question of how such norms are ethically cultivated and inhabited. How do Chinese enter into "international" or "global" spaces? How does the pursuit of credit and credibility relate to the boundaries between the "national" and the "international"? I discussed how Vincent Martin worked in and through "cultural difference" to develop relations of communication and exchange with Chinese counterparts. But the "national" staff members approached the boundary of "China" and "the international" from the other side. The career of Gao Lili, National Veterinary Epidemiologist, shows how a Chinese veterinarian cultivated a "global" ethos in herself. Gao's career also shows that the boundary between "international" and "national" could be productive of both value and discordance.

Gao was born in Henan and took a bachelor's degree at Henan Agricultural University, graduating in 2000. After graduating, she began working as a lecturer at the Henan Institute of Science and Technology. She soon enrolled in a Master's Degree in Preventive Veterinary Medicine at the China Academy of Agriculture Science, studying with Chen Hualan at the National Key Laboratory for Avian Influenza in Harbin. In 2005, following the re-emergence of H5N1 virus, she was sent abroad to study for a Master in Veterinary Public Health, a degree not offered in China, attending a joint program between the Free University of Berlin and Changmai University in Thailand.

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<sup>170</sup> Declan Butler and David Cyranoski, "Flu papers spark row over credit for data; Rush to publish on H7N9 upsets Chinese scientists" *Nature* Vol. 497, No. 1447 (2 May 2013): 14-15.

<sup>171</sup> On balanced reciprocity, see Sahlins, *Stone Age Economics* (Chicago: Aldine-Aherton, 1972). Sahlins distinguishes "balanced reciprocity" by the fact that the exchange is accomplished without delay, and therefore there is no debt or social obligation, in contrast to the "true gift" or generalized reciprocity.

<sup>172</sup> Bruno Latour and Steve Woolgar, *Laboratory life: the construction of scientific facts* (Princeton, N.J.: Princeton University Press, 1986).

When we worked together at conferences or training programs, Gao frequently noted the different educational style that she had discovered abroad. In China, veterinary science is taught almost entirely through textbooks, as Gao knew well, having taught in Henan for a number of years. But in Berlin, the professors assigned students to conduct field investigations, for example asking them to go to slaughterhouses to learn about food safety regulations and how they were implemented in practice. Second, in Germany the classes were much smaller than is typical in China, and classes were structured in a seminar style, with students and the professor seated around a round table discussing the topic of the day. Finally, rather than lecturing facts and theories for the students to commit to memory by rote, the professor would often begin with a problem and ask the students to come up with a solution.

Gao valued this mode of pedagogy very highly, and adopted its language in making "China" into the object of her criticism. But she also cultivated a position *between* national and international that enabled her to produce scientific values. While conducting surveys for the LMB study, Gao one day encountered a "middleman" transporter who sold poultry from his truck to wholesale vendors at the market. The transporter happened to be from somewhere near her hometown in the northern province of Henan. Because they spoke the same regional dialect, they spoke in an intimate and free manner--something not even possible, Gao suggested, in the Chinese *lingua franca* or "common speech" (*putonghua*). They were both far from home. The transporter told her something very unexpected: he bought his poultry in the north, sometimes as far north as Helongjiang, and sold them in the southern cities such as Guangxi.

For Gao, this encounter deeply changed her understanding of poultry trade dynamics, and the encounter became an often retold story in the ECTAD office. It gained veridictional status of a kind--although it was not supported by the network analytics and was not included in the published paper. Instead, it was the kind of fact that circulated around the ECTAD office and guided how they understood the results of the network analysis study. Sometimes it was repeated in public presentations. At the China Vietnam Forum, for example, Vincent presented the results of the network analysis study, stating that although "we could see already there was some long distance trade going on in several provinces" the analytics help to quantify the *degree* or importance of these market-chains within a network structure. But Vincent's phrasing is not specific enough: for they could only look for this long distance trade once Lili had spoken with the transporter. Gao's ability to translate from the dialect to the national and to the global produced truths that added a level of meaning and understanding to the analytic maps of the LBM network study.

These two mechanisms of value--on the one hand, the international as source of value for Chinese like Gao; and on the other, her ability to employ her mediatory position between national and international to produce new values--were in tension. Having accumulated enough value in credit through her work of translation, in the late summer of 2011, while Martin was away on vacation, Gao suddenly left ECTAD. She had been recruited by another international veterinary expert, who had visited China on an FAO mission, to study for a Ph.D. in veterinary epidemiology at the University of Prince Edward Island in Canada. The practice of translating China into knowledge significant in an international domain was at the same time a practice of self-cultivation, one that ultimately could cross over the boundary between 'national' and 'international'. When Martin returned to Beijing and found that she had left, he was shocked and disappointed. In his eyes, she had revealed herself to be "opportunistic" by moving onward as soon as a better situation appeared. But it was the ECTAD-China office itself that had, paradoxically, constructed the opportunities.

The movement onto China's grounds through the establishment of the ECTAD-China office transformed the boundaries of 'international' and 'national', enabling new kinds of relationships and new forms of communication. ECTAD-China acted as a "synapse" that encouraged movement of information, materials, and even people outward toward zones marked as "international." Producing this motion required working through an acknowledgement of differences--labeled by participants as "cultural"--rather than demanding the expansion of transparency and identity. And although it was highly successful at putting things in motion, mobility was not without its own troubles and uncertainties.

*Coda: Crossing the Chinese epizootic wall*

When Robert Wallace, the phylogeographer, first read Marius Gilbert's articles describing his geo-spatial risk models in southeast Asia, he excitedly contacted Gilbert and proposed working together. As a result Wallace was invited to participate in the first meetings of the NIH Workshop in Bangkok, Thailand. As I described in Chapter 1, Wallace considered the first NIH workshop to be a "meeting of the minds" because of its potential to raise unusual scientific questions. But if the meeting in Thailand opened new ways of practicing the science of avian influenza, the meeting reasserted questions about the position of China within such a science. As Wallace put it during a discussion with other participants at the 2010 Workshop, "Last year, China was a big question mark."<sup>173</sup> He later elaborated to me:

"What came out of that discussion in Bangkok, and I think Scott [Newman] brought it up, is that a lot of the new H5N1, various, sub-strains, and new avian influenzas were coming out of southern China, and yet here we don't have entree into that . . . and so there was a sense of, "Oh, we're talking about everything but the most important thing of all on the table which is what's going on in China."

By 2010, however, the NIH workshop was held in Beijing, China. New forms of transaction were developed with Chinese scientists and officials. The contrast with Wallace's earlier experience of "open-access" and polemic is striking. Wallace's reaction reveals that something changes when the study of avian flu *on China's grounds* replaces the study of avian flu from *outside* China:

" When it was announced, when I was invited, that the second meeting was to be in Beijing, that kind of blew my mind. They [Scott Newman and Xiangming Xiao] had started this year-long negotiation, to work with, and we actually ended up going to look at Lake Qinghai, I mean in my mind that was cut off by an epizootic Chinese wall, we were able to get in there now, not only were we starting to research stuff, [but we were also] collaborating with people, with Chinese scientists, I found that absolutely extraordinary.

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173 "Discussion Session (Session 8)." *The 2nd International Workshop on Community-based Data Synthesis, Analysis and Modeling of Highly Pathogenic Avian Influenza H5N1 in Asia*. June 1, 2010. Beijing, China.

In the next two chapters, I explore how the members of the NIH Workshop group moved onto China's grounds, developed new forms of collaboration, and encountered unexpected problems. I follow them as they traveled to Poyang Lake.

## Chapter Three

### The Lake as Pilot

Out in the Poyang Lake's marshlands, roads skirt along the top of high earthen ridges, with cultivated fields and villages on one side and open marsh or open water on the other. These ridges, that seem to mark the line between a space cultivated by man and a space left to wild nature, are themselves a relatively recent historical artifact, constructed in the 1950s by the Chinese Communist Party's "mass mobilization" of peasant labor. Built for land reclamation and flood control, the embankments exemplify the Maoist vision of transforming nature through human collective labor and sheer spiritual will, often without the aid of machinery.<sup>174</sup> They also visibly reveal a key argument of this chapter: the distinction of wild and domestic is a not only a symbolic category, but also a material and historical product of human practices.

After Mao's death and following the winds of reform, Poyang Lake's nature was, like so many other things in that moment, given new form. The recently rehabilitated status of "expert" science after the Cultural Revolution, heralded by Deng Xiaoping's 1975 "Outline Report" on the Chinese Academy of Sciences<sup>175</sup>, inspired scientists Ding Wenning and Zhou Fuzhang of CAS Animal Research Center to investigate whether Poyang Lake was the over-wintering site of the rare and majestic White or Siberian Crane. In winter of 1980, they discovered ninety-one white cranes at Poyang and began to promote the idea of protecting habitat at the lake for migratory wild birds. The discovery intersected with a broader reconsideration of the lake as a natural resource to be empirically studied, managed and conserved, a philosophy developed in Yu Guangyuan's concept of "national resource economy" (*guotu jingji*) and demonstrated by the 1988 publication *Studies on Poyang Lake* (based on a series of investigations conducted by Jiangxi Province between 1980 and 1987).<sup>176</sup> As a result of these investigations, one part of the lake was set aside as a "Migratory Bird Reserve" in 1980, gaining National status in 1988. Following the United States National Park model, human access was legally restricted to tourism and scientific research in certain protected zones.<sup>177</sup> At the same time, however, other parts of the lake were designated as suitable for an agricultural production base, where duck farming was considered among the "legitimate uses" of the lake's natural resources, such as its plentiful wetlands, waterways and rice paddies.<sup>178</sup> Wild and domestic birds, properly managed and protected from harmful exploitation, could live together harmoniously, if roughly separated into distinct zones, argued the authors of *Studies on Poyang Lake*.

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<sup>174</sup> Judith Shapiro, *Mao's War Against Nature*; Chris Coggins, *The Tiger and the Pangolin*.

<sup>175</sup> Deng's "Outline Report" to the CAS argued that science and technology were part of the productive forces of society, the material base rather than the ideological superstructure, and therefore were legitimate and valuable activities in need of rehabilitation and support. See Yu-Farn Wang, *China's Science and technology policy, 1949-1989* (Brookfield, Vt.: Avebury, 1993).

<sup>176</sup> 《鄱阳湖研究》编委会著，鄱阳湖研究，上海科学技术出版社：1988。"Studies on Poyang Lake" Editorial Committee, *Studies on Poyang Lake* (Shanghai: Shanghai Kexue Chubanshe, 1988). The preface is written by Yu Guangyuan.

<sup>177</sup> On national parks in China, see Coggins, *op cit*.

<sup>178</sup> "Studies on Poyang Lake": 458.

After the emergence of avian influenza and amidst fears of a global pandemic, however, influenza researchers identified Poyang Lake as a possible "influenza epicenter". The Chen et al 2006 paper I discussed in Chapter One, authored by scientists from Hong Kong and China, was the first publication to name Poyang Lake as a possible source of pandemic flu. The study specifically blamed the contacts between wild birds and domestic birds at the lake for producing and transporting emergent avian influenza viruses. In this chapter, I describe an international team of researchers who set out to make these contacts between wild and domestic birds into a scientific object. Where previous research teams (such as Shortridge, or Chen et al) had conducted laboratory research and *speculated* about the contagious relationships joining wild to domestic birds, the international team I describe intended to develop what they called an "ecological research perspective" that would make these contacts into a precisely delimited research object--what they called the "wild bird-domestic poultry disease interface." They hoped that this research object would not only provide important data about influenza emergence, but could also provide "forecasts" of "hot spots" and "hot times"--in other words, they hoped to construct a kind of early warning system to detect the emergence of future influenza pandemics. Finally, they believed that their work at Poyang Lake was a "pilot" for a new mode of scientific practice--a demonstration of the organizational and collaborative relationships necessary for "One Health" science. This chapter tells the story of how they undertook to apply the concept of the "wild bird-domestic poultry disease interface" to the actual site of Poyang Lake, testing out new forms of organizing scientific practice and its objects in their movement to the field.

When scientists conduct research in the field, they are constructing knowledge in a mode distinctive from the laboratory. Where the laboratory sciences aim to produce universal knowledge with no ties to its place of origin, field sciences construct knowledge that is based in the particularity of a place. But how does the particularity of the field site figure in the knowledge claims that they make? Although knowledge made in the field is fundamentally place-based, field scientists have developed a wide range of practices to generalize the validity of their knowledge claims. The most well known of these are practices that attempt to reconstruct the field on the model of the laboratory, by establishing lab-like "field stations", introducing methods of quantification and standardization into field observations, or recontextualizing field environments within lab apparatus. Robert Kohler argues that the field sciences bridge lab and field, built and natural environments, tabletop and outdoor practices, "with their different conventions of establishing credibility and trust."<sup>179</sup> Karen Knorr-Cettina redefines the notion of laboratory as a *practice* rather than "the physical space in which experiments are conducted." She writes that "laboratory practice entails the detachment of objects from their natural environment and their installation in a new phenomenal field defined by social agents," whether on an academic campus or in the woods.<sup>180</sup>

However, field scientists have also developed a variety of other techniques for "scaling up" the field site to other levels of generality or extension without resort to the lab. In this chapter, by tracing how an international team of researchers set out to make the contacts between wild and domestic birds at Poyang Lake into a scientific object, I analyze how they positioned this field site within their truth discourses, that is, how they figured Poyang Lake within what Michel Foucault has called a "mode of veridiction." How are claims about the field given

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<sup>179</sup> Robert Kohler, *Landscapes and Labscapes: Exploring the lab-field border in biology* (Chicago: University of Chicago Press, 2002): 32.

<sup>180</sup> Karen Knorr-Cettina, *Epistemic cultures: how the sciences make knowledge* (Cambridge, Ma.: Harvard University Press, 1999).

broader generality? What function does the field site play in a claim to truth? How do claims made on the basis of field research relate to previous or subsequent scientific activity?

In the 1880s, Stephen A. Forbes wrote an essay entitled "The Lake as Microcosm" in which he argued that lakes provided the ideal settings for exploring his incipient ideas about "communities" of living beings. A lake, he wrote "is a little world within itself--a microcosm within which all the elemental forces are at work and the play of life goes on in full."<sup>181</sup> Forbes' ideas about the interactions among living beings would become foundational for the early science of ecology. He also nicely exemplifies how a particular place can stand for a broader object, in this case as microcosmic whole.

Poyang Lake was not seen as a microcosm in which a "complete and independent equilibrium of organic activity" could be studied. Rather, Poyang was positioned first as an epicentre of influenza emergence and later as a pilot study for 'One Health'. This chapter traces how Poyang lake came to occupy these positions within scientific discourses. Much like Forbes' microcosmic lake, the way in which Poyang Lake was positioned in scientific discourses is related to emerging conceptualizations of nature, life and danger. Finally, I argue that the making of scientific knowledge at Poyang Lake contributes to an understanding of field science as an activity defined not only by being outside of the laboratory, but constructed in active--and sometimes resentful--opposition to the perceived prestige accorded to laboratory knowledge.

### *Nature's threats*

Drive north from Berkeley on the I-80 towards Sacramento, cross the bridge at Crockett into Vallejo, take the shore road past the majestic estates of the former naval base on Mare Island, and pull into a long driveway cut far into the waving yellow grasses of the marsh, and you find yourself at the Western Field Station of the United States Geological Survey. John Takekawa works here, amidst a warm clutter of maps and field guides about birds. Finding Takekawa at his desk in spring of 2013, I sensed the casual vibrancy of the backwoods field scientist, as comfortable bagging birds in the wild as writing in the office or lab, as if still living in the tradition of the old Californian naturalists of the early twentieth century. Surrounded by abandoned military barracks and next to a vacant research complex, however, the atmosphere was tinged with the fading glory of American scientific and military might, and felt far from the heady hopes of John Muir.

"At least we have some marsh nearby," Takekawa laughed, explaining that his field station was established at the site through an arrangement with the U.S. Fish and Wild Life Service, who owned the land, seventeen years earlier. A few years after he moved in, the Navy forced Fish and Wildlife to leave after a dispute over remaining toxics and unexploded ordinance on the tract of land, leaving Takekawa's Field Station alone. The empty Fish and Wildlife building now housed a noisy population of nesting sparrows in its eaves.

Takekawa is a specialist in what he affectionately refers to as "birdwork," conducting research on the migratory and other movement patterns of wild birds. Tracking bird migration was an observation-based gentleman science until the late 19th century. In 1890, Danish biologist Hans Christian C. Mortensen experimented with attaching small metal rings to the legs of migratory birds, a technique that came to be called "ringing" or "banding." When banded birds are later recaptured or found dead, the information about band-point and recovery-point

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<sup>181</sup> Cited in Daniel W. Schneider, "Local Knowledge, Environmental Politics, and the Founding of Ecology in the United States: Stephen Forbes and "The Lake as a Microcosm" (1887)" *Isis*, vol. 91, No. 4 (2000): 681-705.

provide valuable information about migration routes. Since the 1980s, "birdworkers" have used techniques of satellite telemetry in addition to traditional banding, furthering the "detachment" of birds in flight from their actual environment into data points that can be analyzed and manipulated in the laboratory.<sup>182</sup>

Although a bird expert, Takekawa had never before worked on bird diseases, not to mention avian influenza. He was recruited for bird flu research by Diann Prosser, a wetland ecologist and ornithologist, and Scott Newman, who Takekawa describes as a "doctor doctor." Newman holds a veterinary degree from Tufts University and a Ph.D. on seabirds from UC Davis, which allowed him to build connections across often separate and sometimes antagonistic life sciences. When I watched Newman at work as a researcher and as an educator, I was struck by how he utilized these forms of expertise to drive a commitment to protecting wildlife, and particularly, wild birds. When I asked him about his conservation values, however, he emphasized that "at the end of the day, its the science that tells the story."

"I do approach this with a conservation background, but if you don't have good science, you're done, you have nothing to stand on, you need to get credibility to get anything done."

A growing number of historical studies have shown the complicated politics of conservation, resource-use, knowledge and professionalization that accompanied the birth and growth of ecology and wildlife management. Together these studies identify a powerful *tension* in the career of ecology, not only between scientific fact and normative value,<sup>183</sup> but more specifically between nature understood as a domain of human self-assertions (including those of scientific knowledge) and nature considered as a value to be preserved.<sup>184</sup> While there has recently been a series of philosophical and political deconstructions of the value of "wild nature",<sup>185</sup> I take a second-order approach to analyze the concrete manner in which tensions appear and are worked over in scientific practice.

At the turn of the 20th century, as the American frontier began to disappear and "wild nature" seemed increasingly bounded by industry and settlement, debates arose between advocates of *preservation* and *conservation* of natural areas or resources. John Muir, who called himself a preservationist, saw wilderness as a spiritual space, the rocks and craters of the Sierra mountains "like the wall of some celestial city." "I feel like preaching these mountains like an apostle," Muir wrote.<sup>186</sup> Gifford Pinchot, trained in techniques of scientific forestry in Europe, advocated practices of conservation that included controlled human use. "Forestry is tree farming," he argued.<sup>187</sup> Yet the difference between Muir and Pinchot, between preservation and conservation, can be overblown. Muir himself relied on and developed scientific techniques for

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<sup>182</sup> Alexandra Cleminson and Silke Nebel, "Bird banding" *Nature Education Knowledge* Vol. 3, No 8 (2012): 1.

<sup>183</sup> Max Weber, "Science as a vocation" in Gerth and Mills, eds., *From Max Weber: Essays in sociology* (New York: Oxford University Press, 1946).

<sup>184</sup> See Hans Blumenberg, *The legitimacy of the modern age* (Cambridge, Ma.: MIT Press, 1983).

<sup>185</sup> See, for example, William Cronon, "Getting back to the wrong nature" in Cronon, *Uncommon ground: rethinking the human place in nature* (New York: W.W. Norton and Co., 1996); Bruno Latour, *The politics of nature: how to bring the sciences into democracy* (Cambridge, Ma.: Harvard University Press, 2002).

<sup>186</sup> Stephen Fox, *The American conservation movement: John Muir and his legacy* (Boston: Little-Brown, 1981): 12.

<sup>187</sup> Char Miller, *Gifford Pinchot and the making of modern conservation* (Washington, D.C.: Island Press/Shearwater Books, 2001).



studying the spaces he described as "sacred" wilderness. And Pinchot imputed values to his forests, such as aesthetics, that went far beyond economic utility.

Historians identify multiple roots toward the modern science of ecology: from Stephen Forbes and his Illinois lakes; to the New England fisheries scientists; to the mountains and forests of California. From its inception in each of these sites, ecology was closely connected with practices of nature conservation and preservation. For example, Stephen Forbes developed concepts about the communities of life on the Illinois River floodplain while engaged in struggles to protect the floodplains from privatization and drainage.<sup>188</sup> Following the Second World War, the academic tracing of food chains and the "web of life" became closely tied to political critiques of the weapons industry, nuclear testing, industrial pollution and chemical pesticides in works such as Rachel Carson's *Silent Spring*.<sup>189</sup>

Newman's early work adopted what by late-20th century America had become one characteristic resolution of the tension inherent to the modern idea of nature, resolving the tension into the expert practice of conservation science. In one project, working with scientists from government agencies, NGOs, and universities, Newman's collaborative team studied the long-term impact of the Exxon Valdez spill on the health of Alaskan seabirds. It was a classic set-up: scientific assessment by professional experts would identify how human activities harmed the health of a vulnerable species.<sup>190</sup> The emergence of avian influenza H5N1, however, challenged this configuration of value and method. When the epizootic began spreading out of China in 2004, and in particular following a large-scale outbreak among wild birds at the remote Qinghai Lake, some experts blamed transmission of the virus on the migration of wild birds. Although today much more is known, there remain conflicting attributions of responsibility for virus transmission to wild bird migration and to commercial poultry transport. Newman and Takekawa, in a paper I discuss further below, have aptly characterized this novel problematization of conservation science: wild birds, they write, are now not only victims, but quite possibly also *vectors* of viral danger.

The situation led to a breakdown of Newman's version of conservation science. Newman initially set out to work on wild birds largely motivated by the sensibility that they had become scapegoats for the virus. Some Chinese studies had argued that the role of wild birds meant that avian influenza was not the responsibility of China's poultry industry. Since according to their studies the virus originated in wild bird reservoirs in Siberia, China's poultry were victims of viral transmission following bird migrations, not sources of viral emergence.<sup>191</sup> Newman's work can therefore be understood as an effort to clarify the role of wild birds in avian influenza epizootic. Although he would come to understand wild birds as vectors of H5N1 virus, he never stopped viewing them as victims. Although wild birds were the reservoir for avian influenza viruses, in his view they were not the source of *highly pathogenic* avian influenza viruses such as the H5N1 strain. At stake is an argument about when nature itself becomes pathological. For

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<sup>188</sup> Daniel W. Schneider, *op cit*.

<sup>189</sup> Brian Lindseth, "Nuclear war, radioactive rats, and the ecology of exterminism" in Ryan Hediger, ed., *Animals and war* (Brill, forthcoming).

<sup>190</sup> Gregory H. Golet, Pamela E. Seiser, A. David McGuire, Daniel D. Roby, Julian B. Fischer, Katherine J. Kuletz, David B. Irons, Thomas A. Dean, Stephen C. Jewett, Scott H. Newman, "Long-term direct and indirect effects of the 'Exxon Valdez' oil spill on pigeon guillemots in Prince William Sound, Alaska" *Marine Ecology Progress Series* Vol. 241 (2002): 287-304.

<sup>191</sup> Lu Liang, Bing Xu, Yanlei Chen, Yang Liu, Wuchun Cao, Liqun Fang, Limin Feng, Michael F. Goodchild, Peng Gong, "Combining Spatial-Temporal and Phylogenetic Analysis Approaches for Improved Understanding on Global H5N1 Transmission" *PLOS One* Vol. 5, No. 10 (2010).

Newman, wild birds themselves did not endanger life. It was only through contacts with domestic birds--that is, as a consequence of human activities--that wild birds became vectors of death and danger.

The appearance of new threats to life is one of the fundamental components of the critiques of modernity that gather around the "risk society" debates. In Ulrich Beck's original and still classical formulation, the very success of scientific rationalization has led to the emergence of new, unanticipated risks, such as cancers caused by pesticides used in agriculture. As a result, science has begun to forsake its "monopoly on rationality" and forge reciprocal relations with other social actors.<sup>192</sup> Beck's arguments intersected with work on the anthropology and sociology of science, leading to a series of debates on science, society, nature and new environmental or health risks.<sup>193</sup> During these debates, critics of Beck argued that the "risk society" thesis does not fully question the form of scientific knowledge. Beck retains a naive conception of common, shared nature.<sup>194</sup> Where Latour sees an ontological problem, however, I argue there is a more important *normative* problem. The science of ecology has forged a precarious tension between a scientific knowledge of the patterned relationships among living beings and a *normative* conception of "healthy" or good or stable modes of organizing these relationships. Recently, the idea of wild nature as "untouched by man" has even been replaced by notions of homeostasis, collapse and sustainability. But ecological discourse still relies on fundamentally normative distinctions of the healthy and the pathological--even if only claiming that stability is better than instability.<sup>195</sup>

As I discussed in Chapter 1, the "One Health" framework proposed by the Wildlife Conservation Society and later adopted by a joint ministerial conference of the WHO, FAO, and OIE, drew attention to the diseases shared among wild, domestic and human populations. One Health pointed to the pathological conditions produced by human-caused transformations of the environment, and made the "health of life on Earth" into the responsibility of human actors and organizations. In the following section, I present a 'pathway' towards One Health which delineates the varying backgrounds and resources brought together under the concept. I do so because it was the multiplicity of these backgrounds that enabled the diversity of approaches to One Health, including the particular approach to One Health undertaken by Newman, Takekawa and other members of the NIH Workshop team in research at Poyang Lake.

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<sup>192</sup> Ulrich Beck, *Risk Society: Towards a new modernity* (London: Sage Publications, 1992).

<sup>193</sup> Bruno Latour, *We have never been modern* (Cambridge, Ma.: Harvard University Press, 1993); Ulrich Beck, Anthony Giddens, Scott Lash, *Reflexive modernization: politics, tradition and aesthetics in the modern social order* (Sanford, Ca.: Stanford University Press, 1994); Helga Nowotny, Peter Scott, and Michael Gibbons, *Re-thinking science: knowledge and the public in an age of uncertainty* (Cambridge: Polity Press, 2001).

<sup>194</sup> Latour, *op cit*; Brian Wynne, "May the sheep safely graze?" in Scott Lash, Bronislaw Szerszynski, and Brian Wynne, eds., *Risk, Environment and Modernity: Towards a New Ecology* (New York: SAGE Publications, 1996). Latour writes in his critical appraisal of Beck's "cosmopolitics": "For the sociologist, nature, the world, the cosmos, is simply there; and since humans share basic characteristics, our view of the world is, at baseline, the same everywhere." See Rabinow's earlier critique of cultural anthropology's presumption of shared nature, "Humanism as nihilism: the bracketing of truth and seriousness in American cultural anthropology" in Norma Haan, Robert N. Bellah, Paul Rabinow, and William Sullivan, eds., *Social science as moral inquiry* (New York: Columbia University Press, 1983).

<sup>195</sup> On the normal and the normative, see Canguilhem, *Normal and the Pathological* (New York: Zone Books, 1989).

## Pathways

Genealogy, in Michel Foucault's sense of the term, is organized under the project of the history of the present. A genealogy intends to reveal the contingency of the established and unquestioned forms of the present. A genealogical account is employed in the mode of war, a negative dialectic without resolution, as the Saxon genealogists against the Norman conquerors, or the anti-psychiatry activists against the asylum, or Foucault himself against the prison.<sup>196</sup> A pathway is a genealogy for the anthropology of the contemporary.<sup>197</sup> As such, a pathway calls for less smashing of statues and desecration of idols. This is because the concept of the "contemporary" is a tool for taking up objects of inquiry within a temporal frame that, while rejecting the dead-weight of History, disrupts the comfort with "novelty" and "contingency" that has recently dominated both academic and popular discourses. The contemporary is neither reducible to the past (the presumption of historical explanation), nor wholly new. The contemporary is assembled out of parts existing in multiple tempos: some may be emergent, others residual; some herald the future, others turn towards past traditions. A pathway through the contemporary aims to specify the disparate elements, removed in time and space, that together form the "whole" of the contemporary. But two things must be remembered: in the contemporary assemblage, the parts are not unified but retain their disparate forces, motions, and directions; and the "whole" is not a whole that stands there to be discovered but is constructed by the blazing of the pathway, a reduction of historical complexity that is completed according to the parameters of significance chosen by the pathmaker.

Taking the metaphor of pathway seriously, we can imagine trails blazed through the same forest according to parameters of speed, in order to make particular points accessible, in order to illuminate particular vistas, and so on. In this case, the pathway that I draw is shaped by my field experience among the researchers studying Poyang Lake, a habitualized sensibility rather than logical principle of what counts as significant. Based on that experience, my main objective here is to illuminate the multiple, positioned approaches possible under the framework of "One Health". My argument is that these multiple potentials derive from the multiple trails and footpaths leading to the causeway of contemporary One Health. In some ways, the pathway approach bears comparison to Hans-Jorg Rheinberger and Stefan Muller-Wille's proposal of a history of science in the form of an "hourglass," in which a diverse and varied "epistemological space" ultimately gives rise to a highly specific "epistemic thing" before breaking apart into multiple experimental futures. Unlike the hourglass, however, the pathway cannot safely place all of the elements in the past, and therefore the shape of its routes cannot be predetermined.<sup>198</sup> My own pathway leads into the same causeway as the others, and it is only from walking on that pathway, so to speak, that the others can be retraced and charted.

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<sup>196</sup> The most important references are Foucault, "Nietzsche, Genealogy, History"; Foucault, *Society must be defended*; and Foucault, *Discipline and punish: the birth of the prison*. See also Rabinow and Dreyfus, *Michel Foucault: Beyond structuralism and hermeneutics*.

<sup>197</sup> On the anthropology of the contemporary, see Rabinow, *French DNA: Trouble in Purgatory* (Chicago: University of Chicago Press, 2002); Rabinow, *Marking time: on the anthropology of the contemporary* (Princeton: Princeton University Press, 2008); Rabinow, *The accompaniment: assembling the contemporary* (Chicago: University of Chicago Press, 2011). On pathways, see Rabinow and Stavrianakis, *Demands of the day: on the logic of anthropological inquiry* (Chicago: University of Chicago Press, 2013).

<sup>198</sup> See Stefan Muller-Wille and Hans-Jorg Rheinberger, *A cultural history of heredity*. Chicago: University of Chicago Press, 2012.

## *Natural History of Infectious Disease*

Warwick Anderson has shown that despite the tendency towards biological reductionism in 20th century infectious disease research, a number of infectious disease experts continuously reiterated the importance of understanding disease-causing pathogens in environmental, evolutionary and ecological relationships.<sup>199</sup> One major source for this view was the science of host-parasite relationships, such as in the work of the most well known of these scientists, Rene Dubos. For others, it was work in veterinary pathology that revealed the importance of intermediary hosts or animal reservoirs as sources of human pathogens. There were continuities with but also differences from colonial medical traditions of 'tropical medicine,' which had always been concerned with disease in particular milieu. Anderson makes clear that these approaches were distinct from an earlier "neo-Hippocratic" theories in which environments directly caused or influenced the course of disease. In the work of Australian MacFarlane Burnet, for example, the emphasis was on biological and inter-species interactions, not environmental "living conditions."<sup>200</sup> Kennedy Shortridge, the Australian microbiologist specializing in collecting influenza viruses in Hong Kong, stands firmly in this tradition. Like the others, although he constantly affirmed the importance of China's agricultural and ecological conditions in producing new influenza viruses, he made the agro-ecology come into his lab. Shortridge's ecology was known through its products: the microbes under the microscope.

## *Wildlife/livestock interface*

In recent decades, the conservation field has been riven by debates over the role of local populations in the management of conservation areas. The once classical "National Parks" model, based on the experience of the United States, is now widely criticized for its limited ability to account for local populations, relying on largely ineffective relocation schemes, fencing and policing. Livestock herding on national park lands in Africa was a key site for these debates. Anthropologists and other social scientists, for example, called attention to the discrepancy between concepts of "wild" nature imposed by conservation law and indigenous notions of life and nature, calling on conservation experts to view natural places, such as forests or grasslands, as "social, historical, biographical spaces."<sup>201</sup> However, conservation scientists and experts, at first primarily based in Africa, approached the problem of these disputes in another way: by developing concepts for practical work on the problem. Rather than continuing to rely on the separation of wild from domestic animals, these experts developed the concept of the "wildlife-livestock interface" to orient scientific work on the particular challenges of places where wildlife and livestock exist together. A 1985 conference organized by the Inter-Africa Bureau of Animal

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<sup>199</sup> Warwick Anderson, "Natural histories of infectious disease: Ecological vision in twentieth-century biomedical science" *Osiris* 2nd series, Vol 19 (2004): 39-61.

<sup>200</sup> On the rise of a medical science of "living conditions" in 19th century France see Rabinow, *French Modern: norms and forms of the social environment* (Cambridge, Ma.: MIT Press, 1989).

<sup>201</sup> Anna Tsing, *Friction: an ethnography of global connection* (Princeton, N.J.: Princeton University Press, 2005). See also Roderick P. Neumann, *Imposing Wilderness: Struggles over livelihood and nature preservation in Africa*, University of California Press, 1998; and for a classic critique of wilderness see William Cronon, "The trouble with wilderness; or, getting back to the wrong nature" in William Cronon, ed., *Uncommon Ground: Rethinking the Human Place in Nature*, New York: W. W. Norton & Co., 1995, 69-90.

Resources and held in Kenya, for example, centered on "Wildlife/Livestock Interfaces on Rangelands."

Although disease was included as one of the issues at stake at the "wildlife/livestock interface," it was only one among several and subordinated to a more general concept of conflict and survival. In a 1993 paper, J.R. Grootenhuis, a participant in the 1985 meeting, and R.O. Olubayo provide a simple definition of the concept: "The wildlife-livestock interface is defined as an area where both wildlife and live stock are common." Grootenhuis and Olubayo further add that the interface is primarily a space of conflict between wildlife and livestock. They understand conflict in a Darwinian sense of struggle over chances of survival, and provide three domains of conflict, one of which invokes disease: "Wildlife is in conflict with livestock in three obvious ways: through competition for the same natural resources in space and food, as a disease reservoir for livestock and through predation."

### *Disease Emergence*

Historian Nicholas B. King has documented the historical development and institutionalization of what he calls the "emerging infections worldview," and my account here largely draws from his work.<sup>202</sup> In 1989, Stephen S. Morse, a virologist at Rockefeller University, chaired a conference to address the theme of what he referred to as "Emerging Viruses." Defining emergence as "the introduction or expansion of viruses into a new host population", Morse argued that emergence is caused not only by viral mutation and evolution, but also by many other *environmental* factors. In addition to the evolutionary appearance of "*de novo*" viruses, then, there are two other significant sources of viral emergence: the introduction of an existing virus from another species; and the introduction of a virus from one (relatively isolated) portion of the human population into another. Morse coined the term "viral traffic" to "represent processes involving the access, introduction, or dissemination of viruses to their hosts, as distinct from the evolutionary aspects."<sup>203</sup>

In 1991, Morse's views became the basis for a meeting of the U.S. Institute of Medicine in Washington, D.C. The meeting (including experts in virology, tropical medicine, public health) centered discussion on what was seen as a new, but increasingly coherent problem: the appearance of novel diseases (such as HIV or Ebola) and the resurgence of antibiotic resistant strains of existing diseases. The IOM meeting extended the problematic of emergence beyond viruses to include any infectious disease. Subsequently published as an IOM report, *Emerging Infections* articulated three principles of disease emergence:

- 1) A global distribution of danger: because of transportation interconnectivity, the U.S. was now "threatened" by diseases once thought relegated to other parts of the world
- 2) A diagnosis of factors of emergence: from growing populations to warfare, human behaviors such as sexual activity, human transformation of the natural environment, and failures in public health.

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<sup>202</sup> Nicholas B. King, "Security, Disease, Commerce" *Social Studies of Science* Vol. 32 No. 5-6 (2002); "The scale politics of emerging diseases" *Osiris* (2004): 62-76;

<sup>203</sup> Stephen S. Morse, "The origin of "new" viral diseases" *Journal of Environmental Science and Health, Part C: Environmental Carcinogenesis and Ecotoxicology Reviews* Vol. 9, No. 2 (1991): 207-228.

3) A technical solution: increased public health surveillance and scientific research, with the goal of providing "early warning" of disease emergence.

Viewed from the perspective of contemporary discourses of "One Health," the expertise represented in the 1991 IOM meeting is striking for its limitations: although "environment" was considered to be a primary "factor" of disease emergence, there were no ecologists or veterinarians participating in the meeting.

### *Food Emergency*

Ben Hickler, in his dissertation "Epidemic Oversight", has shown that the emergence of avian influenza--and particularly its spread to Europe--was mobilized by the United Nations, and in particular the Food and Agriculture Organization, to justify large-scale investment in the livestock industries of the third world, and especially in Southeast Asia.<sup>204</sup> FAO's original mandate was to ensure global food security and to promote rural livelihoods through development. According to Hickler's account, using insurance reasoning, FAO leaders aimed to persuade wealthy nations that it was in their economic advantage to monitor and improve livestock industry in the third world. The FAO continued its "pro-poor" rural development programs through a strategic alliance with the language of global emerging infections. One caveat should be added to Hickler's account, though: as I brought up in Chapter 1, the FAO began to reorganize itself in the 1970s from an organization devoted to agricultural development toward the ability to respond to emergencies. The shift from developmental aid to humanitarian relief can be seen clearly in the changing functions of the FAO administered World Food Program.

"The main focus of the World Food Programme was on transforming food in kind into economic and social development through projects geared toward agricultural and rural development and human resource development. However, in the 1970s, a process of change was driven by humanitarian catastrophes."<sup>205</sup>

In addition to the driving force of catastrophic events, cutbacks in funding from the U.S. and other donor countries led James Ingram, Executive Director of the World Food Program, to revision the WFP as an emergency response food-aid program. As part of this 1980s reform, the FAO developed the Global Information and Early Warning System for Food and Agriculture to monitor and communicate information about famines and droughts, and to assist the coordination of food aid distribution. When the FAO later joined with WHO and OIE to create the Global Early Warning System in response to the avian flu epidemic, they were on familiar footing.

### *Intersecting Paths*

During the early 2000s, these pathways began to intersect. More and more texts written by conservation experts about the wildlife/livestock interface turned to the problem of disease. Veterinarians and wildlife specialists began to be invited to meetings on emerging diseases. The

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<sup>204</sup> Benjamin Hickler, "Epidemic oversight: Emerging infections and rural livelihoods in the Mekong" Ph.D. Dissertation, Department of Anthropology, History, and Social Medicine, University of California, San Francisco (2010).

<sup>205</sup> Stokke, *op cit.*

avian influenza epizootic was an important catalyst for these intersections. The original 'One World, One Health' meeting was held by the Wildlife Conservation Society in direct response to the outbreak, although participation was largely from the wildlife, conservation, and veterinary sectors. Within a few years the WHO/FAO/OIE had constructed a joint framework that adopted the concept of "One Health." Yet the One Health framework was extremely broad. In her article, Chien argues that this broad generality productively enabled new forms of inter-agency collaboration, but also threatened to render One Health merely "ceremonial."<sup>206</sup> In the rest of the chapter, I analyze how the NIH Workshop team adopted the mantle of "One Health" for research at Poyang Lake--and how Poyang Lake became a *demonstration* of one *approach* to One Health science.

### *Toward Poyang Lake*

Thomas Kuhn's notion of 'paradigm' refers to a key experiment--often a breakthrough achievement--that becomes an exemplar or model for further research. Normal science subsequently continues by developing and refining the questions of the paradigmatic model experiment, drawing on a sense of shared norms, intellectual problems, and basic assumptions with the model, until anomalies build up and give birth to a new breakthrough paradigm.<sup>207</sup>

However, the scientists who set out to conduct research at Poyang Lake did not situate their work in relation to a paradigm. To be more specific, although each of their individual research projects--such as satellite mapping of land-use, or tracking of bird migration--did not propose much new conceptually or methodologically in their own fields, the *integration* of these research projects into a single research programme guided by an "ecological research perspective" was understood to be an unprecedented undertaking. Was it then itself a new paradigm? The scientists' self-consciousness of the novelty of their work suggests the need for a more precise and actor-oriented understanding of scientific change. In what follows, I describe how scientists situated their work in relation to pasts and futures of scientific knowledge. I pay particular attention to how Poyang Lake as a field site is figured by these claims.

### *New epicentre*

In the spring of 2005, thousands of wild birds were found dead on the shores of the remote Lake Qinghai, high in China's western mountain plateaus. Laboratory tests confirmed the birds were victims of highly pathogenic avian influenza H5N1. Newman later told a journalist that the deaths, ultimately totaling over six thousand, made this the "largest bird die-off in history."<sup>208</sup> Yet, surprisingly, there were no poultry farms anywhere near the lake. Scientists from the Chinese Academy of Sciences (CAS) Institute of Zoology, Institute of Microbiology, and Computer Network Information Center, along with Wuhan Institute of Virology traveled to the lake to investigate the deaths. According to the standard understanding of influenza emergence at the time, avian influenza virus was supposed to be held in wild bird (especially wild waterfowl) reservoirs in a low pathogenic form. Once transmitted to poultry, the virus could gain pathogenicity but only rarely and sporadically transmitted back to wild birds. At Qinghai,

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<sup>206</sup> Chien, *op cit.*

<sup>207</sup> Thomas Kuhn, *The structure of scientific revolutions* (Chicago: University of Chicago Press, 1970). See also Ian Hacking, *Representing and intervening, op cit.*

<sup>208</sup> Li Jiao, "In China's backcountry, tracking lethal bird flu" *Science* Vol. 330, No. 6002 (2010): 313.

the scale of the die-off meant that highly pathogenic forms of H5N1 virus were actively circulating among wild birds. As a result, the centrality of domestic poultry within the accepted model of influenza emergence must be in error. The urgency of developing a new model was only heightened when migratory birds began to drop from the sky over Europe, infected with H5N1, showing that wild birds could carry the virus over long distances.

Diann Prosser, a wild bird expert at the USGS, was in China on an NSF scientific exchange program for graduate students when the first reports of the massive wild bird mortality on Qinghai Lake started to come in. She immediately traveled to the lake but was unable to get access. The following year, however, along with Takekawa, Newman and Xiangming Xiao, she began to collaborate with the Chinese scientists to monitor wild bird migration at the lake. Tracking over five hundred birds over a couple of years, they produced maps showing the migration patterns of ruddy shelducks and bar-headed geese in the Central Asian Flyway, which runs from South Asia to Mongolia and Siberia. The research was driven by (and funded with) the urgency of preventing the next pandemic. However, with no poultry farms in the region, the scientists began to wonder where the H5N1 virus had come from in the first place.

Interest in Poyang Lake derived from the search for the source of viruses that had infected wild birds at Qinghai, a source that, if discovered, would mark the true epicentre of influenza emergence. Two articles published by Chinese teams raised interest in Poyang Lake and were usually referenced in explanations of why research was conducted there. The first article--Chen, et al, "Establishment of multiple sublineages of H5N1 influenza virus in southern China: Implications for pandemic control"<sup>209</sup>--reported the isolation of H5N1 virus from healthy wild birds at the lake. The authors postulated that H5N1 viruses may have been carried by wild birds from Poyang to Qinghai lake, and therefore Poyang was implicated as a possible source for the 2005 Qinghai die-off. The second paper--Mukhtar, et al, "Origin of highly pathogenic H5N1 avian influenza virus in China and genetic characterization of donor and recipient viruses"<sup>210</sup>--was a laboratory study of the phylogeny of the earliest known H5N1 virus, isolated from a domestic goose in Guangdong province in 1996 (and therefore known as A/H5N1/Goose/Guangdong/96). Tracing the sources of the virus, Mukhtar, et al, argue that two viruses isolated from around Nanchang City, Jiangxi Province--the provincial capital which sits on the edge of the Poyang Lake wetlands--contain internal genes that " were closely similar to A/Goose/Guangdong/1/96 (H5N1) virus, the first highly pathogenic avian influenza (HPAI) virus of subtype H5N1 isolated in Asia." The first research projects set up by Takekawa and Newman at Poyang Lake intended to test whether or not wild bird migration routes linked Poyang to Qinghai Lake.<sup>211</sup>

### *Counter-paradigm*

The closest thing to a paradigm, in the sense of a previous study that was referenced both in conversation and in published articles as a background for the Poyang Lake research, was the Chen et al paper, "Multiple sublineages." The paper was often cited as a justification for

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<sup>209</sup> Chen, H. et al, "Establishment of multiple sublineages of H5N1 influenza virus in southern China: Implications for pandemic control" *PNAS* Vol. 103, No. 8 (February 21, 2006): 2845-2850.

<sup>210</sup> Mukhtar, et al, "Origin of highly pathogenic H5N1 avian influenza virus in China and genetic characterization of donor and recipient viruses" *Journal of Genetic Virology* Vol. 88, Pt. 11 (November 2007): 3094-3099.

<sup>211</sup> See John Y Takekawa, et al, "Migration of waterfowl in the East Asian flyway and spatial relationship to HPAI H5N1 outbreaks" *Avian Diseases* Vol. 54 (2010): 466-476.



conducting research at Poyang Lake, or in recollections of what piqued scientific interest in the lake. However, members of the group frequently referenced the paper as an example of how research into influenza should *not* be done--the opposite of shared norms or methods of research. Chen's team had isolated positive samples of highly pathogenic H5N1 virus from live, apparently healthy wild birds--a feat rarely accomplished elsewhere. Everyone knew this might implicate the particular significance of Poyang Lake as a source for long-distance transport of the virus through wild bird migration. However, in the Chen et al publication, these wild birds were categorized only as "migratory ducks", with a note at the bottom of the page adding that "Migratory ducks included falcated teal (*Anas falcata*), mallard ducks (*Anas platyrhynchos*), and spot-billed ducks (*Anas poecilorhyncha*).

When John Takekawa referenced the paper as the background for the team's research at Poyang Lake, he fumed. "A lot of the sampling has been done without designation," he complained.

"Now, its fine if you can get to species level, its better than you started, I mean initially it was just like "duck". And there's like *huge* differences in species, right? And so all this is to us [wild bird specialists], common in that you look at a bird and you know that "Well, that's a different bird, and its different from this one over here, cause its doing this bit of behavior, its completely, its not going to be found in that habitat, all of those things you automatically know, and you hardly think about it, you don't realize that over there a virologist is thinking, "That's a duck". You know? A tree's a tree. And that redwood and that oak tree, its all the same."

Takekawa's comments mark a distinction between virology and ecology, or more generally, laboratory and field sciences. Takekawa and his collaborators understood their scientific practice to take place within a context in which the study of viruses in laboratories claimed a position of epistemological and political superiority over field sciences like ecology. This imbalance among approaches in avian influenza research has been noted by other scholars. Chien, in her study of international agencies and One Health, reports that the international response to avian influenza was at first dominated by biomedical research and solutions. By 2011, however, she cites an FAO scientist who claimed everything about the disease was already known, but still far too little about transmission. She also describes the emerging efforts at that time to constitute what she calls an "ecological frame," differentiated from "technical/biomedical" and "societal" frames, for understanding the disease.

During my fieldwork among FAO experts and the NIH Workshop group, I was situated amidst the effort to overturn the biomedical, virological, and laboratory paradigm of influenza research. Those who wanted to overturn the biomedical and laboratory paradigm, I found, were driven by feelings of resentment. Laboratory and especially virological studies were seen as dominating what counts as scientific truth and how credit and credibility in science is distributed. Again, John Takekawa made this point clear:

Yeah I think that you'll see that, that in general ecology is called like a soft science. You see that overall. You look at, a Nobel prize for ecology, you ever seen one? There's like never. You'll get a Nobel prize for chemistry, virology, medicine, all those hard sciences, so ecology already starts from a disadvantage, and then, even when you have expensive, detailed studies with it, there's lack of control in the experimentation, and there's a lot

more in lab work that you could control and have more refined analyses, which may or may not be still better science in my view. So it ends up that in the higher journals, you might see publications in *Science* or *Nature* that, for the field there in, its not necessarily rocket-shaking big things, but because its the right field, it'll go in, and the highest, the best ecology paper, you put it in there and it won't get accepted because its "oh, its another ecology paper". And I think that has a negative effect on, well, this links to the whole thing about impact factor, journal prestige, who reads papers, what's read, and like give you an example of say birdwork, the [research we published on] wild birds and influenza, the highest, the top wild bird journals, that we read, have an impact factor of 2. You know? They don't even get to 3. And those journals are read by the world's experts in wild bird ecology. There's no question. There's like five journals, all of them under 2, around 2 in impact factor, so it tells you that, of course, the right people read, the ecologists who are concerned about this, but because it doesn't go to a wider populace, then, its sort of discredited, and your science isn't considered as good, even though it could be the very best in the world. Whereas a virologist is the opposite case, right?, you can have a low-level paper in some consideration in that area, get published in *Science*, and suddenly its really hot stuff, where relative to your field . . . ."

Tellingly, although the NIH Workshop team has been featured twice in the "News" sections of *Science* and *Nature* because of the broad interest of their research on avian influenza in China, no articles written by the Workshop team have been published in these top journals. Many of their articles have been passed around to up to six journals before getting published. In one incident, a paper submitted by Marius Gilbert was rejected because of its "speculative data". But when Takekawa and one of his post-docs wrote a letter to a journal criticizing a virologist's speculations about ecology, the editor refused to publish their letter, saying there is no "sin" in failing to account for claims about ecological relationships.

Kuhn's scientific revolutions seem to come about almost by chance--enough anomalies accumulate and the possibilities for revolt begin to increase. But what of the revolutionary? In the case of the Workshop team, the construction of a different mode of "integrating" scientific knowledge was explicitly counter-paradigmatic, and only encouraged by perceptions that their work was not adequately recognized. Nietzsche described the affect of *ressentiment* as an externalization of responsibility for ones failures, an attempt to level the strong by denouncing their evils, and a dwelling in the pleasure of victimhood.<sup>212</sup> But Takekawa's feelings of resentment are open and productive, fueling the drive to differentiate himself from the "reductionism" of the powerful.

### *Framework, Perspective and Approach*

Among the international agencies, "One Health" is described as a "framework" for organizing inter-agency communication and coordination. Ben Hickler argues, drawing on an interview with Annu Lehtinen, a regional coordinator in the UN, that a ground plan for this inter-agency coordination even predates the adoption of the "One Health" framework. In addition to global surveillance programs such as GLEWS, a series of inter-agency coordination initiatives developed at the country level in 2004, through a temporary institution called United Nations System Influenza Coordination (UNSIC). At the beginning, UNSIC was developed to help the

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<sup>212</sup> Friedrich Nietzsche, *On the Genealogy of Morals* Trans. Walter Kauffmann (New York: Vintage Books, 1989).

agencies organize their own preparedness plans. However, they soon became platforms for coordinating the various agencies around the problem of pandemic prevention.<sup>213</sup> In this sense, the framework of One Health was like the frame placed around a painting immediately before exhibition, providing structural support and decorative gleam but hardly altering the content of the work.

The scientists took up the notion of One Health by referring to a "One Health approach"<sup>214</sup> or sometimes an "ecological research perspective,"<sup>215</sup> rather than a framework. The pathway I described above, which emphasized the multiple roots and persistent heterogeneity gathered under One Health, reveals some of its analytic benefits here. For despite the unifying rhetoric of One Health, the scientists retained the particularity of a single one of the pathways in constructing their "approach" to One Health and the ecosystem: the wildlife-livestock interface. As they introduced this research in conference presentations, the NIH Workshop team described how they hoped to develop an "integrated study of wild waterfowl-domestic poultry-human interface"<sup>216</sup> at Poyang Lake. In a 2010 review article, members of the Poyang Lake research team argued that a "key factor integral to the evolution of low pathogenic avian influenza into highly pathogenic avian influenza is the interaction between wild birds and poultry."<sup>217</sup> Research was therefore organized in order to answer the questions: "Where, when and how do wild birds interface with poultry and humans?"<sup>218</sup>

A shift in the mode of representation for analytically presenting the object of danger and knowledge has accompanied the change from surveillance of viral emergence toward an "ecological research perspective."<sup>219</sup> The viral emergence of avian influenza is usually depicted as a diagram of transmission relationships among species hosts. In a diagram accompanying a 1992 paper by Robert Webster, et al., a duck is positioned in the center of a circle of other animal and human hosts, with radial arrows pointing outwards towards the circle's edge, two of which point in both directions (indicating bi-directional transmission). Other arrows link the species positioned on the circumference. The accompanying text explains that "The working hypothesis is that wild aquatic birds are the primordial reservoir of all influenza viruses for avian and mammalian species. Transmission of influenza virus has been demonstrated between pigs and humans (solid lines). There is extensive evidence for transmission between wild ducks and other species, and the five different host groups are based on phylogenetic analysis of the NPs of a large number of different influenza viruses."<sup>220</sup>

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<sup>213</sup> Hickler, *op cit*.

<sup>214</sup> Cf. Scott Newman, Boripat, Siriaroonat, and Xiangming Xiao, "A One Health Approach to Understanding Dynamics of Avian Influenza in Poyang Lake, China" Presentation at *Ecohealth 2012*, Kunming China, October 15-18 2012.

<sup>215</sup> John Takekawa, Diann Prosser, Scott Newman, et al, "Victims and vectors: Highly pathogenic avian influenza H5N1 and the ecology of wild birds" *Avian Biology Research* 3(2), 2010: 1-23.

<sup>216</sup> Xiangming Xiao, Scott Newman, Tracey McCracken, Ding Changqing, "Wild waterfowl - domestic poultry - human Interfaces: An Integrated Pilot Study in Poyang Lake, Jiangxi, China" Presentation at *Second International Workshop on Community-based Synthesis, Analysis and Modeling of Highly Pathogenic Avian Influenza H5N1 in Asia*. Beijing, China, 31 May to 1 June, 2010.

<sup>217</sup> Takekawa, et al, "Victims and vectors," *op cit*.

<sup>218</sup> Xiangming Xiao, Scott Newman, Tracey McCracken, and Ding Chanqing, "Wild bird-domestic poultry-human interfaces: An integrated pilot study at Poyang Lake" *The 2nd International Workshop on Community-based Data Synthesis, Analysis, and Modeling of Highly Pathogenic Avian Influenza H5N1 in Asia*, Beijing, China. 2010.

<sup>219</sup> Takekawa, et al, "Victims and vectors" *op cit*: 14.

<sup>220</sup> Webster, R.G., Bean, W.J., Gorman, O.T., Chambers, T.M., Kawaoka, Y. Evolution and ecology of influenza A viruses. *Microbiol Rev*, 1992;56:152-179.

A 2006 diagram included in an article by Webster, Malik Peiris, and others from Shortridge's lab adds a temporal dimension that flattens the circle of transmission onto the progress of emergence.<sup>221</sup> Although arrows double back recursively, depicting the multi-directional transmission of viruses between wild birds, domestic birds, and swine, the general course of emergence--figured by numbers that stand for moments or stages--begins in wild bird reservoirs, moves through domestic animals, and ends in humans. To draw once again on Frederick Keck's conceptual vocabulary, the temporality of influenza emergence is mapped onto the classificatory "frontiers of the living."<sup>222</sup>

In the works of the NIH Workshop team, the "wild bird-poultry interface" is represented not with diagrams but with photographic images of actual field research sites, constructing the rhetorical authority of "being there" in the field. Second, these photographs are selected to capture a sensibility of categorical confusion: wild mixed with domestic, human mixed with animal. The "Victims or vectors" paper was an early site of experimentation with concepts and modes of representation.<sup>223</sup> Some pictures depict domestic poultry foraging in landscapes that the caption informs are "natural wetlands." Others show different species in close contact, such as live domestic and wild ducks "housed together ready for slaughter"; or two men riding on the back of a pickup laden with market-bound poultry. Finally, a third trope shows the porousness or transgression of boundaries, such as a woman separated from her poultry by only a low screen of netting.

A recurrent mode of representing the interface throughout publications and PowerPoint presentations has involved the assemblage of four separate photographs in a four-square pattern. There is no systematic arrangement, such as wild animals on one side and domestic on the other. Rather, the whole of the four photographs together provides a sensibility of the contours and contacts of the interface precisely through the *gaps* between the images, calling forth the imaginative synthesis of the viewer.

A diagram used in an early presentation of their research helps to show how they understood the dynamics of the interface. Migratory birds and free-ranging ducks/geese are shown to interact in a central blue circle captioned as "Paddy Rice Fields/Natural Wetlands/Fish Ponds," a zone whose contours are shaped by "Weather/Climate" on the one hand and "Poultry Trade/Human Population" on the other. Drawing on the skills of Xiao and others, geo-spatial imaging of the landscape was one fundamental component of the study, able to map the domain of paddies and wetlands that was the setting of the interface. Wild bird tracking and telemetry, such as that conducted by Newman, Takekawa and Prosser, was another. But they also hoped to include studies of poultry production systems, marketing networks, and even human occupational exposures. When I explained to Scott Newman that I was an anthropologist the first time I met him, he quickly suggested I conduct my research at Poyang Lake.

How would all of these disparate research projects add up to knowledge about the wild bird-poultry interface? The scientists described their goal as the *integration* of distinct research projects into a common knowledge claims. It was this integration that everyone understood to be the novelty of the research. But they didn't refer to this as a new paradigm; their technical name for their novel experiment was a "pilot."

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<sup>221</sup> RG Webster, M Peiris, H Chen, Y Guan, "H5N1 outbreaks and enzootic influenza" *Emerging infectious diseases* Vol. 12, No. 1 (2006): 3-8.

<sup>222</sup> Keck, *op cit*.

<sup>223</sup> Takekawa, et al, "Victimes or vectors" *op cit*.

## *Integration: A pilot*

When Takekawa, Newman, Prosser and Xiangming Xiao began research in China in 2006, they at first aimed to understand specific aspects of avian influenza transmission that may have use for surveillance or control of the disease. As described in a news article in the journal *Science*, the early research of the group--first at Qinghai, later at Poyang Lake--was oriented towards analyzing ecological risk in order to develop an "early warning system" for the pandemic. By 2010, when I first began to follow the group, they were repositioning their research at Poyang Lake as an "integrated pilot study." In common usage, a pilot study refers to a preliminary research foray that precedes the large-scale investments of time and money necessary for validation of results. However, in this case the researchers meant pilot in a slightly different sense: by turning avian flu from an emergency into an *example* of disease emergence, they repositioned the research conducted at Poyang Lake as a model for One Health approaches.

Integration was the central term of the NIH Workshop team's "ecological approach." When I attended the 2011 Workshop held in Beijing, I discovered that the task of integration is, at least on the surface, largely left to models and their modelers. A diagram entitled "A framework for ecology, risk assessment and forecasting of avian influenza" made by a member of the group depicts the organizational centrality of the model within the NIH group. On the left side of the diagram are arrayed a number of distinct ways in which a site--such as the Poyang Lake--can be observed, counted and recorded. This collection of information draws on many technologies and indeed disparate disciplines, ranging from satellite observations of cropping intensity, to tracking of wild birds with mobile radio tags, to sampling of birds for laboratory surveillance of viruses. On the far right side, a gray column assembled actors such as "international organizations," "national states," and "citizens," and identifies their role with act of *decision*. Squarely in the center, between observation and decision, stand the models. These included: "Geospatial analysis, individual-based models, species distribution models, agent-based models, statistic and mathematical epidemiologic models."

At the second workshop, some members of the audience raised skeptical questions about particular assumptions made in the models, but they were brushed aside, or confidently encompassed. One young British modeler began his presentation with the quote "All models are wrong" in boldface type on the first PowerPoint slide, neatly circumventing questions about reductive assumptions. Models are obviously simplifications, he indicated, and are not intended as *representations* of the world (whose truth is judged by its accuracy) but as tools for producing "forecasts" of hot-spots or risk assessments. A second diagram of the use of models presented at the Workshop depicts a diamond of linked squares. At the top, a "data portal" draws in "Hosts (animal, human), agro-ecology, climate, diseases"; on the bottom, a "model portal" draws in "Statistical, EMN, phylogeny, mathematic models". In the middle, between data and model are the products of this synthesis: "Diagnosis, Nowcasting, Forecasting." Rather than a relation between mind and nature, representation and reality, here the model functions to integrate data in order to facilitate decision, diagnosis and forecasting.

The models function to manipulate the data observed in the field, precisely what Knorr-Cettina had described as "laboratory practices." And indeed, a "Workshop" is simply another way of saying "laboratory." But the key question is how the Workshop and the models would enable the "integration" of field experiences and data. And despite the apparent clarity of the diagrams, this was very much in dispute.

During the 2010 Beijing Workshop, questions about modeling led to debates that located the problem of integration not only in the model, but also in the organization of the group itself. Since the 2010 Workshop took place before I had been introduced to the group, the following account is based on detailed notes of a discussion session held at the end of the meeting.<sup>224</sup>

Scott Newman begins by setting the objective for discussion as "data sets integration and identification of gaps in knowledge."

"Focus on joint data analysis - brainstorming. Let's start the discussion with these three topics: What sorts of data exist that have not been integrated yet into models; what are the major gaps in our knowledge, and what additional disciplines to [sic] should we engage in the group.? Let's start with data that exist but are not yet integrated."

Newman's opening makes clear how integration was seen as a key problem for the NIH group. As Marius Gilbert would put it, "an integrated analysis is pretty difficult, more than just assembling some data." In its most simple sense, integration was a question of how to organize the wide varieties of data collection into a single scientific problem. Although models could be tools for this organization, however, they could not accomplish the conceptual work of forming a common problem. The modelers were most clear about this conceptual problem. While Newman went around the room trying to collect accounts of data that had been collected but not yet "integrated" into the models, the modelers kept interrupting with more fundamental questions.

Marius Gilbert broke in: " We all have an idea of the dream dataset. But what are the areas or geographical units where we have a critical mass of data to address a question in a novel way. Can we go around the table and get opinions from each person?"

And later, Robert Wallace added his voice: " I think we should talk about what questions we want to ask. If we do so we will be in a better position to know what we need."

Once broader questions were raised, they quickly became normative as much as technical ones. One FAO administrator hinted at the shifting objectives of the NIH group when he said: " Back to fundamentals: What is the overall goal? Publishing papers? Eradicating flu??"

Scott Newman claimed that there was no need to choose between these goals. "Ultimately, we want better science in service of improved biosecurity and public health. Publications along the way are good to establish credibility. The two go hand in hand."

If publishing papers had now joined "early warning" of pandemic emergence as objectives of the group, the functional position of Poyang Lake within their research programs had shifted: from influenza epicentre or risk "hot spot" to exemplary *pilot*. Marius Gilbert first pointed to Poyang Lake during the discussion as "for sure" a site toward which to "direct attention." Poyang Lake was selected not because of its imminent threat to global health but because, to repeat Gilbert's earlier statement, it was an "area or geographical unit where we have a critical mass of data to address a question in a novel way." Thus, Gilbert pointed to Poyang Lake as *one* possible site for integrated analysis, and mentioned the possibility of *comparative* studies between Poyang

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<sup>224</sup> A copy of these notes is available at the website of Xiangming Xiao's Center for Spatial Analysis at the University of Oklahoma.

Lake and Qinghai Lake, and between the East Asian Flyway (a bird migration route that passes through Poyang) and the Central Asian Flyway (which passes through Qinghai). John Takekawa agreed, using the language of experimental methodology to describe the comparison:

"In my view, the central Asian flyway is kind of a control situation: no poultry in the northern part of the flyway. In contrast there are lots of chickens in the eastern flyway. So we have one flyway with few poultry compared with one that has a lot of poultry—makes for interesting comparison."

The discussion ended with reflections on the usefulness of the Workshop, and linked the problem of integration with the *organization* of the participant researchers. Joachim Otte, a veterinarian and FAO administrator questioned whether the NIH Workshop group could be described as a *collaboration*: "This mutual information exchange is great, but do we have true collaborations? I think we should look at two or three themes, as Marius suggested, and focus on these for the next meeting."

In their discussion of their attempt to institute new ethical relations between the human and the natural sciences, Paul Rabinow and Gaymon Bennett distinguish collaboration from cooperation.

"A cooperative mode of work consists in demarcated tasking on defined problems and objects, with occasional if regular exchange at the interfaces of these problems. Such cooperative exchanges are organized structurally from the outset. . . . A collaborative mode of work, by contrast, proceeds from an interdependent but not predetermined division of labor on shared problems. It entails a common definition of problems (or at least the acceptance of the existence of a problem-space) as well as an agreement to participate in the development and implementation of techniques of remediation."<sup>225</sup>

The term *integration*, therefore, stands as a placeholder for virtual, if not actual, new forms of scientific collaboration. Robert Wallace linked this with practices of communication. "One of the constraints we have is that it is hard to stay in touch. We get involved in our lives, and communication falls off. Some official satellite meetings would be good between the main meetings." Others pointed to "a common source of funding" as a way to maintain more continuous collaboration. But I argue that Poyang Lake as a place made into a *pilot*, was itself a tool for organizing collaborations. As Scott put it, "At Poyang Lake, we intend to continue working there with a multifaceted approach." A place thereby became a technique of testing, or demonstrating, new ways of organizing scientific practice. What was being experimented with at Poyang Lake was as much the form of scientific organization as it was the objects of scientific study. The danger of viral exchanges (between wild and domestic birds) became the material for building experimental forms of scientific exchange and practice.

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<sup>225</sup> Paul Rabinow and Gaymon Bennett, *Designing human practices: an experiment with synthetic biology* (Chicago: University of Chicago Press, 2012).

## Chapter Four

### A Wild Goose Chase

鄱湖鸟，知多少？  
飞时遮尽云和月，  
落时不见湖边草。

The birds of Poyang Lake, do you know their number?  
Flying they cover the clouds and moon  
Alighting the lake grasses are obscured.<sup>226</sup>

Or, perhaps we see the tail feathers of a waterfowl feeding from the bottom of a shallow pool. Is  
it a swan or a goose?

-Thomas Kuhn, *The structure of scientific revolutions*<sup>227</sup>

Scott Newman, an American wildlife veterinarian and migration expert, stared in wonder as swan geese (*Anser cygnoides*) one by one lurched into the sky, then circled in larger and larger rings around the house of the 'breeder' (*yangzhihu*).<sup>228</sup> It was a damp day in the winter of 2007 and a stiff wind blew across the lake. As part of the "integrated pilot study" at Poyang Lake, Newman was on his way to a fieldsite where he hoped to capture migratory birds. The birds would be surgically outfitted with transponders, then tracked by satellite when they returned north to Siberia in the spring.

But the geese he saw above him were not wild; or at least, they were bred, raised, housed, fed and sold commercially as meat by a man surnamed Wang. Newman had found Wang's farm only that morning through what he declared to be a series of chance discoveries. First, he had been impressed by the vast number of poultry farms around Poyang Lake: Driving "from any point A to any point B," he told me in a conversation three years after his field visit, he always saw grey poultry sheds, netted ponds, and flocks of ducks or geese in the canals alongside the road. During their research, Newman and his colleagues stayed at the hotel administered by the Poyang Lake Migratory Bird Preserve in the island town of Wucheng. The Preserve staff were friendly and generous, even helping the research team capture wild birds in mist nets. Over dinner, Newman began to ask the hotel staff about the poultry farms: What breeds and species of bird are being raised? Their answer surprised him: They reported species Newman did not typically see raised as domestic poultry, including swan geese.<sup>229</sup>

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<sup>226</sup> A poem often quoted in news articles about Poyang Lake.

<sup>227</sup> Thomas Kuhn, *The structure of scientific revolutions* (Chicago: University of Chicago Press, 1970): 195.

<sup>228</sup> I translate *yangzhihu* as breeder, rather than farmer, because as will become clear later, the use of the term 'farmer' by scientists and how they subsequently characterize 'farmers' is an important part of my analysis.

<sup>229</sup> According to a paper presented in 2010, species identified on farms in the Poyang Lake area include: swan geese, greylag geese, mallard ducks, spot-billed duck, northern pintail, Eurasian wigeon, common teal, garganey, and ruddy shelduck.



When he visited Wang's farm, the Wang family graciously invited him for lunch, apparently misrecognizing Newman as an American investor. Showing him the flock of swan geese hundreds strong, as well as mallard ducks, Wang proudly told Newman that bird production could easily be increased, and birds could be exported overseas. Wang also emphasized that the wildness of his geese made them particularly valuable.

Newman found himself in a deeply ironic position: Wang's boasts were an influenza expert's fears. When he saw Wang's geese lift off into the sky, everything that he had assumed about the "wild bird-domestic poultry disease interface" seemed to fly up into the air with them.

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In this chapter, I describe the dynamics and discordances of moving knowledge between the field and the lab. In a programmatic essay, Kuklick and Kohler argue that "field sciences" raise distinctive questions about cultural translation, power and "lay knowledge" because "unlike laboratories, natural sites cannot be exclusively scientific domains."<sup>230</sup> Indeed, a growing body of scholarly work has argued that field sciences are less "purified" than the lab, requiring the organization of relations to non-scientists who are "working" the environment, and remaining bound to the specificity of the "terrain." In the first part of the chapter, I analyze Newman's "discovery" of the wild bird farms as a field *encounter*. Newman translated the practices of breeding wild birds into his own concepts, a translation that exposed the limits of his own distinction of wild and domestic. I show that the apparently symbolic distinctions of wild and domestic are first shaped by material practices--not only the practices of domestication, but also practices that reassert the material qualities and signs of "the wild" itself.

In the second part of the chapter, however, I complicate the distinction between field and lab. Drawing on Karen Knorr-Cettina's concept of "laboratory practices" that extend far beyond "the house where experiments are made," I show how techniques of quantification, satellite marking, and virological sampling come to supplant the interlocutive moment of the encounter during follow-up studies.<sup>231</sup> In the process of making laboratory objects, these studies also transform the relationship of the knowing subject to the breeder (*yangzhihu*). The attempt to understand the breeder's practices is replaced with a particular understanding of practice as economic self-interest, figuring the breeder as a species of *homo economicus* known as the "farmer." I contrast this figuration with fieldwork observations of the breeding of wild birds as a practice of self-making that exceeds economic determinations. The epistemological question of the relation between knowing and making life is shown to be grounded in the ethics of the relationship between scientists and the non-scientific practices of working nature.

### *Part One: Encounter*

When Scott Newman told me about his discovery of the farmed wild birds at Poyang Lake, he laughed and recalled how he had "posed" as a foreign buyer. Wang, the breeder, had gone along with the performance, even asking which ports were most convenient for shipments to the United States. That same night, Wang tried to send samples--whole fresh-killed geese from the farm--to Newman in his hotel. The anecdote is revealing. The "discovery" of the breeding of wild birds on poultry farms in Poyang Lake required Newman to forsake his position

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<sup>230</sup> Kuklick and Kohler, *op cit*.

<sup>231</sup> Knorr-Cettina, *Epistemic Cultures, op cit*.

as scientific expert. By playing the role of the American poultry buyer--with all of the symbolic overtones of "foreign capital"--Newman constructed a non-scientific relationship to the breeder that enabled certain kinds of information and understanding to flow. If only for a moment, Newman entered into Wang's world, allowing this world to wash over him until he saw things through Wang's eyes. What had once looked to him like any other wild bird, he now saw as the product of human practices.

Seeing the birds above him as a human creation, Newman recognized the limitations of his own mode of classifying wild and domestic. The study of modes of classification dates from the earliest anthropological works, and the modes of classifying human and non-human, domestic and wild are at the core of the problem. Anthropologists have described how society or culture symbolically constructs the categories of natural kinds, as the reflection of the order of social groups<sup>232</sup>, as relative to culture<sup>233</sup>, or as a process of signification simultaneously marking social differences and natural qualities.<sup>234</sup> A number of studies of historical or fieldwork-based materials analyze the distinct concepts of "nature" in Chinese worlds, including the concept of "wild."<sup>235</sup> However, presuming a distinction between natural "qualities" and cultural "concepts", these approaches often neglect the fact that human practices are constantly transforming the world, remaking nature and its qualitative distinctions.

Philippe Descola has recently developed a critique of idealism in anthropology, arguing that Nature cannot be imagined as a "phantasmagoric analogon of society, a static project of explicit social categories, impervious both to the influence of practice and to the incidence of physical factors on the way people use and perceive their environment."<sup>236</sup> For Descola, the domestic, or the practices that mark the space of the household, is a prime domain in which to examine how practices make and conceive of the relation between human and non-human. It is through the labor of domestication—practices of capturing, breeding, feeding, and so on—that a qualitative difference is cultivated in the continuum of living beings. This material difference of quality is as important as categorical symbolic distinction.<sup>237</sup> The breeder of wild birds, however, approaches the distinction of wild and domestic as a contingent collection of matter and sign to be reassorted. Both symbolic and material dimensions of the distinction become the *object* of practice.<sup>238</sup> The goal is not domestication, *per se*, but rather the manipulation of the distinction of wild and domestic to produce new matters, new meanings, and new values.

During fieldwork at the largest wild bird farm in the Poyang Lake region, I learned that the labor of breeding wild birds worked both at the level of the symbol and the level of material. The farm was started and managed by a resident of Nan Xin township named Liu Fenglian.

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<sup>232</sup> Durkheim and Mauss, *Primitive classification* (Chicago: University of Chicago Press, 1963); Durkheim, *The elementary forms of religious life* (New York: Free Press, 1965).

<sup>233</sup> Franz Boas, *The Mind of Primitive Man* (New York: Collier Books, 1968).

<sup>234</sup> Claude Levi-Strauss, *The savage mind* (Chicago: University of Chicago Press, 1966); Levi-Strauss, *The raw and the cooked* (New York: Harper and Row, 1969).

<sup>235</sup> Carla Suzan Nappi, *The monkey and the inkpot: natural history and its transformations in early modern China* (Cambridge, Ma.: Harvard University Press, 2009); *The tiger and the pangolin: nature, culture and conservation in China* (Honolulu: University of Hawaii Press, 2003).

<sup>236</sup> Phillippe Descola, *In the society of nature: a native ecology in Amazonia* (New York: Cambridge University Press, 1994): 86.

<sup>237</sup> See also Descola, "Constructing natures: symbolic ecology and social practice" in Descola and Gisli Paalson, eds., *Nature and society: Anthropological perspectives* (New York: Routledge, 1996).; Descola, *The Ecology of Others* (Chicago: Prickly Paradigm Press, 2013).

<sup>238</sup> Paul Rabinow, "Artificiality and enlightenment: from sociobiology to biosociality" in Rabinow, *Essays on the anthropology of reason* (Princeton, N.J.: Princeton University Press, 1996).

Born a peasant farmer in a small village, Liu had at one time been the principal of the township high school, and retained close ties to the Nanchang county government. Since the beginning of economic reforms, he had started a wide range of enterprises: he had raised fish and eels in a small pond reserved from the village; he had bred dogs; he had even started an information company. Although he had earned large amounts of money in the past, he had also lost everything in certain speculations. He began to farm wild geese, his most successful enterprise, in 2001. Although beginning at the scale of a household livestock farm, reserving a small plot of land just outside the village, Liu soon formed a company (*gongsi*) that he named "Po Lake Wild Animal Farming Limited Company," abbreviated as "Po Ye" or Po Wild. The company name avoided the word "Poyang" because the lake's complete name had been purchased and registered as a trademark by another firm outside of Jiangxi.<sup>239</sup> In a promotional brochure Liu claims to be the first to raise wild geese and wild ducks in a free-grazing (*fangyang*) style. "Our company courageously seeks innovation (*chuangxin*), bravely explores frontiers, in the whole nation the first to free-graze wild geese and wild ducks in the natural wild (*tianran yewai fangyang*)."

In the beginning, Liu had bought a batch of geese from Shandong province, about three hundred birds, and raised them for sale as meat. But he soon transformed the business to focus largely on the breeding and sale of recently hatched chicks (*miao*). He sold the chicks to other small-scale farmers who themselves raised the birds for meat. In doing so, Liu identified and seized upon a key site of value, shifting the product of farming from meat to the distinctive breed itself.

Liu Fenglian and his son, Haohua, consider what they call the "wildness" or "wild nature" (*yexing*) of their geese to be the primary source or determinant of value. When I first visited the farm, Haohua insisted that industrially farmed chicken has no flavor, and no chicken at all can compare with the "wild taste" (*yewei*)<sup>240</sup> of their birds. On a subsequent day, the Liu family served me a soup made from wild goose raised on their farm, telling me they boiled the soup with only salt and ginger. This preserves the natural "grass flavor" created when the geese forage in the marsh reeds. Cooking regular chicken, they added, requires all kinds of *weijing* (flavor enhancers, such as MSG) to make it taste palatable.

The soup was indeed delicious. But the claims the Liu family make for a distinctive wild taste, including those made in their extensive promotional materials from websites to pamphlets, are sometimes disputed. Master Xiong, the driver from Jiangxi Normal University who brought me on my first visit to the Liu family farm, doubted the taste of a wild and domestic goose could be distinguished. Some buddies of his, he said, had once had the good fortune to eat wild swan, but reported that it "tasted like chicken." Yet it is precisely this circumstance of disputed distinction that drives the efforts of the Liu family to cultivate the quality of wild nature in their birds.

*Demonstrating* the wild nature of the geese is also essential. When they sell geese for meat, they almost invariably sell them live. Moreover, the customers are encouraged to visit the farm to observe the birds flying and foraging amidst the surrounding rice paddies and marsh. Otherwise, customers would not believe the birds are actually wild, Haohua acknowledged. He

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<sup>239</sup> In 2011, during my fieldwork stay with the Liu family, Liu Fenglian and his son both enthusiastically reported that the Nanchang County government had recently purchased the rights to the Poyang trademark as part of broader plans to turn the lake into an "ecological economic development zone" (*shengtai jingji kaifa qu*)

<sup>240</sup> *Yewei* is often translated as "gamey", but I prefer the literal translation here because it avoids the negative connotations that have accumulated around "gamey" flavor in English.

repeated a popular saying about the danger of purchasing fakes: "Hang up a sheep's head outside the shop, but sell butchered dog meat" (*gua yangtou, mai gourou*).

When selling wild goose chicks to prospective farmers, getting these farmers to visit the farm is even more important. The Liu family billed these farm inspections (*kaocha*) as part of a service provided to the purchasers of their chicks. Offered free of charge but with the assumption that the visitors would order a batch of chicks, these inspections included in-depth instruction on how the methods of raising wild geese differed from domestic poultry. Topics ranged from feed to housing, from foraging habits to disease and pharmaceuticals.<sup>241</sup>

These inspection visits were organized to demonstrate the wildness of the birds and the breed. According to the Liu family, the traits that distinguish a wild from a domestic goose include: general external appearance; a growth on the beak of the males, a secondary sex characteristic involved in mating calls; and above all, the ability to fly. At the farm, whenever the geese leapt into the sky, Haohua would nearly leap along with them, proclaiming his wonder and encouraging me to photograph or better take videos of the birds in flight. Promotional materials draw a very close connection between the symbolic value of the birds, their ability to fly, and their wildness. For example, brochures that I helped distribute at the Forestry Exposition proclaim the value of "wild taste" (*yewei*). Images of their wild geese in flight are cut and pasted over a picture of undeveloped sections of Poyang Lake (sections that are not the actual location of the farm).

Environmental NGOs have noted that the consumption of wild animal products has grown rapidly in the past thirty years, which they attribute to the intersection of increasing consumer wealth and the "age-old appeal of consuming wildlife" that has yet to be "stymied by environmental education."<sup>242</sup> Anthropologists have made a more subtle argument about the relationship between wildlife consumption and culture, arguing that notions of "rarity" are important as practices of social distinction<sup>243</sup>, and displays of consumptive excess mark a historical difference with the Mao-era and its metonymic "one big pot of food" (*daguofan*).<sup>244</sup> (Farquhar; Chen; Zhan). However, these accounts share a focus on consumption as a site of symbolic expression, ignoring the interrelation between the marking of symbolic distinctions and the (re)production of material differences. Distinctions are made, or cultivated in breeding practice, not only consumed.

In addition to demonstrating and communicating the wildness of their geese, the Liu family were also concerned to ensure their geese embodied the traits they identified as wild. This was not as simple as selecting a breed of goose from "the wild" or one broadly categorized as "wild". The Liu family found that after four or five generations of human breeding, the geese lose their distinctively "wild" characteristics, including the ability to fly. Liu Haohua described this loss of wildness as degeneration or regression (*tuihua*). To counteract these effects of degeneration, the Liu family relies on techniques for cultivating wildness. These techniques

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<sup>241</sup> Following these prospective farmers around on their inspection tours, I was struck by the similarity of my own early visits, in which I was brought from pen to pen, shown the T-XXS incubator made in Qingdao, and even served samples of meat. The anthropologist is sometimes all too close to becoming a customer.

<sup>242</sup> Linda Ellis and Jennifer L. Turner, "Where the wild things are . . . sold" *China Environment Series* No. 9 (2007): 131-134.

<sup>243</sup> Mayfair Yang, *op cit*, Xin Liu, *The Otherness of Self, op cit*.

<sup>244</sup> Judith Farquhar, *Appetites, op cit.*; Mei Zhan, "Wild consumptions: relocating responsibilities in the time of SARS" in Li Zhang and Aihwa Ong, eds., *Privatizing China: Socialism from afar* (Ithaca: Cornell University Press, 2008).

range in focus from breeding to environmental conditioning, from internal "genes" to the contexts in which the geese live.

First, they carefully manage the breeding of the geese. On a walk around the farm with Haohua one day, we came to a pen holding only ten or twenty ducks. Unlike the other duck pens, however, this pen contained no fencing to cover the top of the pen. Haohua told me that these ducks had "degenerated" and lost their ability to fly due to "inbreeding" (*jinxing fanzhi*), that is, they are the offspring produced by sexual relations between individuals too closely related. To counteract degeneration, the Liu family employs techniques that Haohua told me were analogous to the incest prohibitions of ancient China: those of the same line cannot have sexual relations if they are within three generations of relatedness.

"We don't want them to inbreed. We divide them into three "families" (*jiating*), avoid inbreeding, that's to say, divide them into number 1, number 2, number 3 family. The offspring of family number 1 must be separated from each other. Then, this family number 1 can be bred with family number 3, or with family number 2."

More specifically, *males* from family 1 will not be raised in the same pen with females from family 1. This pen can be referred to as the Family 1 pen. Male offspring of Family 1 will be raised and kept in Family 2 pen or Family 3 pen. Lastly, offspring of successful mate in the pen of family 1 will be classified as family 1. Adopting kinship terminology, the geese are bred matriclocally and matrilineally. Each pen is kept at a ratio of one male to three females. The breeding is, otherwise, not assisted by the farmers. Fertilized eggs are collected and placed in the incubator, a T-XXS made in Qingdao.

However, in addition to breeding techniques, the Liu family also attempts to enhance the wildness of the geese by managing the influence of environment. The main breeding farm, while outside of town, is only about a five minutes drive from the village and is surrounded by rice paddies. The family also built a house there and they live there, along with some of their employees. In order to preserve the wildness of the birds, the Liu family subsequently established what they call an "experimental base" much closer to the shore of Poyang Lake's open waters. Whereas the breeding farm is on the inside of the embankment that keeps the flood waters from human settlement, the goose sheds and pens of the experimental base are on the outside of the embankment. Indeed, during the high flood of 2010, a number of wild geese and ducks escaped and never returned. Haohua sees their non-return as a proof that they maintained their wildness—they were able to once again live *in the wild*. In a low wetland near the lake, they built four low sheds to house the geese. In front of each shed is a long pond where the geese can swim and browse for insects. A village collective built these ponds for raising fish, and Liu Fenglian reserved rights to them after fish farming declined. As Liu Haohua put it, by forcing the birds to accustom themselves to a "wild living environment" (*yewaide yi ge shengcunde huanjing*), their wildness will be improved. Or to quote from their promotional brochure, "To make the human bred swan geese and wild ducks freely move in the waters and wetlands of the Poyang Lake, to allow them to graze for wild foods, will highly preserve their natural wildness (*tianran yexing*), their ability to fly, and their external appearance."

Finally, a third technique involves taking a small number of birds from the Poyang Lake Migratory Bird Reserve. Using a special license, and drawing on connections at the Reserve, the Liu family takes a few geese captured or held at the reserve. These birds contained "outside" genes (*wailaide jiyin*) that, introduced into the flock, helped to protect the wildness of the birds. In its ideal form, the Liu's understood this as an exchange. For a few birds captured at the reserve, the family would trade some of the birds raised on the farm back to the reserve for release. But Haohua once complained to me that the Reserve refused to allow them to release their birds. "Even though they say they want to protect the environment!" Haohua said derisively.

For the Liu family, wildness was not defined as that which was outside of human touch. Neither was it an internal characteristic of certain individual birds or bird species, as if wildness persisted indefinitely in the organism or the species, no matter the context (as lions born and raised in the zoo are sometimes still considered to be "wild animals"). For the Liu family, wildness was a collection of qualities that could be cultivated or lost. The Jiangxi Po Lake Animal Breed CO., LTD was in the seemingly paradoxical business of farming wildness.

The paradox, of course, only exists from a perspective in which farmed and wild are categorically distinct domains. Put another way, the paradox was a product of translation. Scott Newman and his colleagues found breeding of wild birds troubling because of the concept of the "wild bird-domestic poultry interface" that organized their research at Poyang Lake. The farming of wild birds was a categorical contagion that confused distinctions and blurred boundaries, prompting affects of anxiety and identifications of danger.<sup>245</sup> The "discovery," the marking of significance and danger, therefore existed *between* two modes of practice addressed towards Poyang Lake's nature, one which postulates danger at the wild bird-domestic poultry interface (Newman and colleagues); and another which reasorts the wild and domestic by breeding wild birds (Liu and other wild bird breeders). In this sense, the act of discovery is better described as an act of troubled encounter and translation.

### *Part Two: Farmers and other rural personas*

Despite the trouble it caused at first, Newman and his colleagues quickly adapted this conceptual contagion into a new concept of its own: the "farmed wild bird." Four years after Newman's encounter, I joined the members of the Poyang Lake research group for the "Third International Workshop on Community-based Data Synthesis, Analysis and Modeling of Highly Pathogenic Avian Influenza H5N1 in Asia" at the Beijing Landmark Tower Hotel. At the end of the first day of the meeting, Changqing Ding, a Chinese wild bird expert, gave a presentation entitled "Farmed wild ducks and geese: the missing link in understanding the diversity and epidemiology of pathogens transmitted among poultry, wildlife, and people" that reported on research underway at the lake. During the previous years, Newman and his colleagues, both foreign and Chinese, had reoriented the pilot study at Poyang Lake to account for farmed wild birds as a central site of "disease implication" within the ecosystem. Ding described studies that counted and mapped the wild bird farms in the Poyang Lake region, tracked the distance farmed

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<sup>245</sup> Cf. Mary Douglas, *Purity and Danger: An Analysis of Concepts of Purity and Taboo* (New York: Routledge, 2002).

wild birds flew during foraging, and conducted surveys to understand farmed wild bird marketing networks. What had in the moment of encounter been a symbolic contagion of categories, was now implicated as the site for actual intermediary transmissions of viruses between wild bird and domestic poultry populations. The new concept of "farmed wild bird" reconstructed but also reaffirmed the framework of the wild bird-domestic poultry interface. As Newman explained to me, farmed wild birds "could be the link between wild and domestic birds"

They are the perfect intermediary. Because they look identical to their conspecifics, when they are foraging, a wild bird would come right up to them, because phenotypically they are the same. But then, they go home at night, and there are other poultry around at the farm. So there's your transmission!

Robert Kohler and others have unearthed the history of the modern field sciences, showing that as the laboratory ascended to dominance in the determination of scientific truth during the late nineteenth century, "'the field' was simultaneously reconstructed as the residuum of messy, complex, and uncontrollable nature."<sup>246</sup> As a result, the emerging field sciences such as ecology or evolutionary biology constantly renegotiated the boundary of field and lab, bringing materials from the field back to the lab, applying lab techniques of measurement or experiment in field contexts, and even building "labs" in the field.<sup>247</sup>

Some recent philosophical writings about "field science" suggest that the field is an antithesis to the lab. Isabelle Stengers writes that the field scientist has an "appetite" for knowledge that cannot be reconciled with the 'factish' appetite of the lab scientist, because the "field" always remains a specific terrain which can never "claim to represent all the others" or "become . . . capable of supporting a position of judgment."<sup>248</sup> However, historical and anthropological accounts of actual field science show a much more complex interplay between field and lab, documenting the various intellectual and practical pathways taken from the *place* of the field to greater scales of generality. I draw on Hacking and Knorr-Cettina's (admittedly differing) notions of laboratory as *practice*, rather than laboratory as *place*, to focus attention on how the follow-up studies conducted in the aftermath of Newman's encounter intervened on objects and detached them from the field context, preparing them for circulation in scientific models, presentations and publications.

A good example are the field studies of Zhao Delong, who had been a student of Professor Liu at Jiangxi Normal University, before enrolling in a doctoral degree under Xiangming Xiao at Oklahoma. Zhao traveled throughout the Poyang Lake region, mapping the location of wild bird farms and surveying breeders for information about the numbers of "farmed wild birds" and where they foraged. To get around, Zhao employed a driver named Master Xiong, and when I went to conduct my own fieldwork, Professor Liu recommended I hire Master Xiong. "He himself is a peasant (*nongmin*)," Professor Liu told me, indicating that this meant he knew how to find poultry farms and to navigate the countryside. But when Master Xiong came to meet me in the lobby of an inn in Nanchang, I learned that he highly respected the more "scientific" approaches used by Zhao Delong. Hurrying into the lobby, he quickly unfurled a map and asked me where I wanted to go. I told him I wanted to meet some poultry farmers, but I

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<sup>246</sup> Vetter, *op cit*; Kuklick and Kohler, *op cit*.

<sup>247</sup> Kohler, *Landscapes and labs*, *op cit*.

<sup>248</sup> Isabelle Stengers, *Cosmopolitics II* (Minneapolis: University of Minnesota Press, 2011): 228-9.

had no idea where they were. Xiong was disappointed. "Zhao Delong always had a precise plan of which areas to investigate," he said.

Later, as our van bounded over the potholed ridge-roads that separate the wetlands from the paddy fields, Xiong described how Zhao Delong set up a box with an antenna in the van between the two front seats. The box, a satellite transmitter, was connected to a laptop with a map of the region. This allowed Zhao to locate the precise geographic location of the van at any moment. Where there were poultry farms, and in particular where he found "farmed wild birds", Zhao recorded the geographic location on the computer. The epistemic importance of these techniques was driven home to me when I met Professor Liu for a coffee sometime during the middle of my fieldwork. When the professor learned that I hadn't linked by interviews or photographs with Global Positioning System (GPS) coordinates, he was crestfallen. It was as if whatever I had learned was rendered meaningless by the absence of geographic coordination. Trying to encourage me to collect such information in the future, he explained that on the trips he conducted into the lake with Xiangming Xiao and Zhao Delong, they took photographs with a camera that automatically recorded GPS position. Liu then showed me the Geo-Referenced Field Photo Library ("Share your field photos, Show your footprint of travel, and Support monitoring of Planet Earth!"), managed by Xiao, where they uploaded the photos.<sup>249</sup>

In addition to databases such as the Geo-Referenced Photo Library, the "facts" produced by Zhao's studies were intended for presentations and publications. A map presented at the 2010 workshop in Beijing shows one product. In the map, Zhao and Master Xiong's journey have become stars on a map of the lake, a map that removes all other marks of human habitation. However, Zhao has also had a difficult time in turning his results into publications. Master Xiong related one day how Zhao often lamented the difficulty of "writing." Frustration from other members of the NIH group sometimes bubbled over, because Zhao was holding the data that everyone else needed access to for publications.

Knorr-Cetina has described how the laboratory transformation of objects also transforms the subjects of knowledge, making "scientists" and "scientific communities" in varied and particular molds. Likewise, the turning of field encounters into scientific objects--their detachment from natural contexts through practices of photography, coordination with global systems such as satellites, or even writing of papers--also makes scientists in the process. Zhao Delong's story demonstrates the trials and uncertainty of such scientific subjectivation, his own uncertain movement to a university in the United States paralleled by the movement of his observations into maps and databases. It is likely that this uncertainty was enhanced by the fact that the Poyang Lake research was a *pilot study* for One Health modes of scientific practice. In this case, the difficulty of turning research experience into scientific publications was not only an

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<sup>249</sup> The Geo-Referenced Photo Library is online and open-access: <http://www.eomf.ou.edu/photos/>  
The full description reads: A picture is worth a thousand words, and a geo-referenced (geo-tagged) field photo is even better. Everyday, researchers, students and citizens use GPS cameras and smartphones to take photos in the fields, as part of their efforts to document their observations of landscapes, agriculture, forests, natural disasters, and wildlife. This Geo-Referenced Field Photo Library is a community- and citizen- science data portal for people to share, visualize and archive geo-referenced photos from the fields in the world. Users can upload, edit, query and download geo-referenced field photos in the library, and use the photo-based thematic geospatial datasets for the studies of land use and land cover change, the impacts of extreme weather events, and wildlife conservation, etc. Users who provide photos can decide whether individual photos are to be shared or not. A registered user can upload photos into the library and has access to more field photos in the library than a guest user.



individual failure to write, but also related to the systemic challenge of making ecological knowledge into facts deemed valid by laboratory scientists.<sup>250</sup>

But in the case of the field sciences, there is still more to say about the relationships between subjects and objects of knowledge. A pivotal feature of field science is that the object of scientific knowledge is not merely "nature" but a "working nature." As I described in the first part of this chapter, the farmed wild bird made evident how the very object of the NIH groups research at Poyang Lake--the wild bird-domestic poultry interface--was not only modified, but its very distinctions reassorted by wild bird breeders. As a result, laboratory practices applied to field contexts require distinct efforts of objectivation and subjectivation from those in more controlled spaces of the experimental laboratory proper. In the field, as the relation to the objects change, the relations between scientist and other human actors who are "working" the field also takes new forms. In the case of the Poyang Lake research, the new studies shifted the relationship of scientist to those who breed wild birds.

In the moment of *encounter*, Newman had posed as a poultry buyer, reflecting his intuitive awareness that he needed to become understandable to the breeder in order to understand what he was breeding. The practice of translation required an effacement of expert status in order to discover the practices that lay behind the breeding of unexpected new forms. In the moment of follow-up *study*, by contrast, the breeder was determined as a known kind of person who can be a source of standardizable data and managed according to programmatic rules. In Ding Changqing's presentation of survey results, *birds* are linguistically articulated in far more specificity than those who breed them, who have been wholly identified with the place and practices of "the farm": "72% of farms reported domestic poultry being reared on the same premises; chickens (8) and pheasants (2)"; "57% of farmers reported seeing wild birds on their farms with only two farms reporting significant interaction between the wild birds and their flocks."

This known kind of person was the "farmer", the one who stands for the farm where wild birds (or other poultry) are bred and raised. The ascription of the name 'farmer' carried with it an entire set of ideas about character and practice, including markers of the difference between 'scientist' and 'farmer.' Among scientists working on avian influenza in China, discourses about the farmer contained a double-claim of knowledge and ignorance. International laboratory specialists were particularly pleased to admit how little they knew about the "motivations" of the people who were the object of influenza studies and programs. I was approached on multiple occasions for assistance or collaboration precisely because as an anthropologist I was assumed to be an expert on "rural society" and its inhabitants. During a conference on influenza held in Beijing, one world-class expert on the virology of avian influenza stood at the podium and proudly, in a jesting tone, proclaimed his ignorance of everything outside of the four walls of the lab.

At the same time, however, the denial of specialist knowledge about rural inhabitants did not stop the same scientists from assuming what appeared to be common sense facts about "farmers": that they are vessels for economic self-interest. Discussing efforts to promote avian influenza vaccination in China, the same international virologist described a proposed financial indemnity program. "You've got to understand people want to make money," he said, adding.

"And farmers exist for financial incentive."

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<sup>250</sup> See previous chapter for further discussion of this issue.

In adopting this characterization of the "farmer," the scientists reiterated a popular discourse about the power of consumer markets in shaping rural life. Explanations of the oft-noted growth in wild animal farming are telling in this regard. Whether from NGOs, social scientists or government reports, the increase in wild animal farming is seen as a consequence of intersecting changes in consumer demand and government policy. The consumption of wild animal products, for food or medicine, has grown rapidly across Asia and particularly in China during the past few decades. China's wildlife conservation policies, however, have restricted the poaching of certain species--while other species have simply become increasingly rare. As *Studies on Poyang Lake* and other policy documents frame the issue, the farming of wild ducks was an ideal technique for simultaneously advancing the objectives of wildlife conservation and social development: hunting from the wild would be reduced, while farmers would earn higher incomes than from farming common poultry.<sup>251</sup> The double goal was well captured by a slogan written on the wall of a building at the migratory bird refuge: "Protect Birdkind, Enrich Humankind" (*baohu niaolei, zaofu renlei*), it proclaimed. In the process, however, the farmer is figured as a passive, if rational, actor directly responding to market forces.

In China, the outbreak of SARS brought attention to the "problem" of "wild consumption" as a possible source of dangerous zoonotic diseases.<sup>252</sup> In June of that year, following studies linking the source of the virus to civet cats in southern Chinese markets, a joint statement of government bureaus called for a total ban on the marketing of wild land animal products. But only a few months later, the Forestry Department issued a list of fifty-six species that could be marketed and sold if raised under "mature" conditions of commercial husbandry, including two species of wild ducks (but not wild geese). As Mei Zhan argues, discourses about the danger of consuming wildlife proliferated during SARS, as well as contested understandings of what exactly counted as "wild." She suggests that "the 'wild' was marked with a set of heterogeneous meanings specifically and intimately related to, if not produced by, human consumption--consumption in its visceral and discursive dimensions."<sup>253</sup> I argue the same discourses implicate an inverted, but also normatively ambivalent, subject of human production: "the farmer" as a rational actor at the whim of consumer *demands*, as if bound to an almost obsequious willingness to please that can only be contained and regulated through law and policing.

Marshall Sahlins, drawing on Marx and Engel's *German Ideology*, has argued for taking up production as a "mode of life" that extends beyond material and economic determinations. For Sahlins, instrumental practice is preceded by and conditioned through the symbolic world of signs and distinctions. Ideas about *homo economicus* and rational, self-interested action are themselves one particular ideology of practice. Actual practices, reflecting a diversity of symbolic possibilities, exceed the configuration of economic self-interest.<sup>254</sup> Paul Rabinow adds an essential caveat to Sahlins' attempt to mediate culture and practical reason: cultural symbols are also objects of practical work and transformation, and often reflexively so.<sup>255</sup> The breeding of wild birds is practiced in a historical context in which the worth of "rural" life has become uncertain and contested. The practices associated with breeding wild birds are acts taken within

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<sup>251</sup> *Studies on Poyang Lake, op cit.*

<sup>252</sup> Mei Zhan, "Wild consumptions: relocating responsibilities in the time of SARS" in Li Zhang and Aihwa Ong, eds., *Privatizing China: Socialism from afar* (Ithaca: Cornell University Press, 2008).

<sup>253</sup> *Ibid.*, 157.

<sup>254</sup> Marshall Sahlins, *Culture and practical reason* (Chicago: University of Chicago Press, 1976).

<sup>255</sup> Rabinow, "Artificiality and enlightenment," *op cit.*

this context of uncertain identity. As such, they are efforts to cultivate a sense of self-worth, as much as pursuits of self-interest.

### *Peasant doubts*

On a hot summer day in July 2011, I rode the small fish-boat ferry a short distance across the channel from Wucheng, base of the Migratory Bird Reserve, to the nearby island. Ye Xing, the wild goose breeder, met me on his motorcycle, his bright pink polo shirt glowing in the sunlight. As we rode up the dirt paths into the town, I could see that it was mostly abandoned: like many places in rural China by that time, all of the youth had gone down to Guangzhou or Shenzhen to work. Ye's home was a three story affair of recent construction, and the surrounding fields were dotted with livestock ventures in various states of completion. There were long sheds where the birds were fed and slept, a fairly large pond where they foraged during the day, and a spacious grassland with a small herd of cows. On the far edge the open waters of the lake gently lapped the shore.

When we walked around the pond and looked at the geese, Ye commented that he was one of the few of his generation still living in the village. Young people see the city as a place to make easy money, he said, whereas in the rural areas, earning money is "both exhausting and dirty." But Ye had decided not to seek his fortune in Guangdong or Shanghai. When he had reached adulthood, he had considered his prospects. Lacking an educational certificate, and lacking any skills, what could he do in the city besides the most menial of labor? Others want to *xiangshou*--to enjoy the consumer lifestyle of the cities--but they neglect the benefits and possibilities that remain in the countryside, he reflected.

The most promising of these opportunities, Ye believed, was the farming of wild geese and ducks. Ye's pond is about one hundred meters long, filled with large swan geese (*hongyan*) and small baby mallards (*lutouya*). The pond was once part of the common land collectively held by the village, when it had been used to raise regular domestic ducks. But these village enterprises had long since been abandoned. Since the land around the pond was sandy and not suitable for cultivating crops, Ye was able to reserve (*chengbao*) the rights to work it. He started raising wild swan geese in 2003, inspired by the interest in wild bird conservation that was promoted by the nearby refuge. He believed that if he raised wild swan geese he could help protect them, he said, with what might have been a facetious smile on his face. On the side of his door, I noticed a red New Year's character poster that read: "Protect Poyang Lake Wetland Ecology." But his success in selling the birds had caused him to expand the endeavor, and he now believed that this opportunity could only improve as China continued its economic growth. The growth of wealth is accompanied by an increase in the consumption of 'rare and unusual' (*xiqi guguai*) foods, he postulated.

In many respects, Ye's explanations repeat the arguments made by the influenza researchers and other analysts of wild bird farming: farmers turn to raising wild birds because of increased market demand and new conservation laws or policies. However, Ye situates the forces of market and government within a narrative that raises a much deeper question: how to live in "rural" (*nongcun*) China today? Was it possible to make a life worth living in the village? In turn, this ethical question reflected uncertainty about the identity and status of the breeder of wild geese.

In China, the inhabitants of rural areas have been a central and troubling site for the theories and projects of modernizing elites, and as a close corollary, for the social sciences. As

Ann Anagnost writes, "The unreadiness of peasant bodies for full participation as a modern citizenry has been a recurring figure in the discourse of Chinese nationalism--from the *xinmin* (new citizen) of the early twentieth century modernizer Liang Qichao to the prolonged project of political tutelage under the Guomindang."<sup>256</sup> Moreover, the very term "peasant" (*nongmin*) is wrapped up in these simultaneously conceptual and practical efforts to make China modern. Myron Cohen argues that although the word *nongmin* does appear in some ancient texts, it was rarely used, as more commonly used terms included *nongfu*, *nongjia*, *nongren*, *zhuanghu*, *zhuangjiahu*. The term *nongmin* was introduced in the late 19th century as a "reverse loan word" from Japan, where language modernizers had selected the term as a translation for the English "peasant." Cohen sets this "reverse loan" in the context of two historical changes. First, he argues that China did not experience the stark differences between city and country that characterized Europe. Only with the arrival of colonial economic forces in the "treaty-ports" did such divisions begin to grow, divisions that were soon observed and commented upon as one of China's "problems" by European travelers and missionaries. Second, in English-language writings by both Chinese and Western observers of rural China, the turn of the 20th century marks a shift between use of the term "farmers" toward use of the term "peasants" to describe rural inhabitants. This inverted the European model, in which "peasant" was typically used to describe feudalism and farmer to describe the rural citizen of the modern nation-state.<sup>257</sup> The term "peasant" was one important part of the modernizing project to assimilate China to the linear, progressive model of the modern nation-state.<sup>258</sup>

The Chinese communist revolution is known as a peasant revolution. However, Maoist communism, and Mao himself, produced contradictory figurations of the "peasant." In his famous essay that began to move the focus of communist organizing from the urban workers to the rural peasantry, Mao argued that those who saw the peasant as irrational and their revolts as "going to far" simply revealed their own class prejudices.<sup>259</sup> But Mao himself supported policies designed to extract resources from the countryside in order to provide the material for large-scale industrialization, which was seen as the only route to communism, a systematic extraction that often resulted in severe inequalities between rural and urban populations and sometimes ended in disaster.<sup>260</sup> Cohen notes that a lasting outcome of the communist era was the transformation of "peasant" from a discursive concept into a bureaucratic category, most notably in the *hukou* household registration system.<sup>261</sup> The *hukou* divided the population into 'peasant' (*nongmin*) and 'city resident' (*jumin*) categories based on place of birth (and sometimes residence) with high barriers for moving between peasant and city registration.

Despite periods of severe suffering during the Mao-era, there is no doubt that the "peasant" was ideologically the leader of the revolution, and by implication, at the forefront of revolutionary society, discursively if not materially. The reform-era, by contrast, has inverted

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<sup>256</sup> Ann Anagnost, *National past-times: narrative, representation and power in modern China* (Durham: Duke University Press, 1997).

<sup>257</sup> Myron Cohen, "Cultural and political inventions in modern China: the case of the Chinese 'peasant'" in Weiming Tu, ed., *China in transformation* (Cambridge, Ma.: Harvard University Press, 1994).

<sup>258</sup> Levenson, *op cit*; Duara, *op cit*.

<sup>259</sup> Mao Zedong, "Report on an investigation of the peasant movement in Hunan" March 1927.

<sup>260</sup> On suffering of rural people during the Great Leap Forward, see for example Everett Zhang, Arthur Kleinman, Weiming Tu, *Governance of life in Chinese moral experience: the quest for an adequate life* (New York: Routledge, 2011).

<sup>261</sup> Cohen, *op cit*.

the symbolic status of the peasant in what could well be called a "great reversal."<sup>262</sup> The beginnings of reform are said, to be sure, to have sprung from the rural villages. Breaking apart the collective farms began as an experiment in certain rural counties well before the Household Responsibility System was approved as national policy. The first markets, too, opened in rural areas.<sup>263</sup> However, by the 1990s the energy of reform and development had definitively shifted to the special economic zones, signifying a broader shift toward the cities. The rural and the peasant were left behind or refigured as "problems,"<sup>264</sup> with the temporal correlate of standing for the backward past. Liu Xin's ethnographic account of a small village in Shanxi province during the 1990s points out that in the post-reform period, the villagers lived in the "shadow of the modernizing process"

"They were the unspeaking objects of the official slogan "to realize four modernizations!" (*shixian sihua*): they were passive but necessary, for the very process of development was possible only when such people were waiting to be modernized."<sup>265</sup>

Under this shadow, the only form of "modern" life recognized as worthwhile was now an urban one. It is well known that many rural people have indeed gone to the cities to find work in factories or shops, producing the hybrid class and subject-position of *nongmingong* (lit. peasant-workers). The villages emptied out of young people, with only elderly and young children left behind, ironically represents the narrative that a worthwhile future does not lie in the rural village. This reversal of the ideological ordering of urban and rural in the reform period is captured in the changing slogans of the time. During the 1920s, Mao had outlined a policy to "surround the cities from the rural countryside" (*nongcun baowei chengshi*). Developed during a period in which the communist party was enmeshed in guerrilla warfare, this was a political and military strategy, which later became a cultural or epistemological policy, as when Mao employed the same rhetoric in distinguishing the "barefoot doctor" from the "city medicine" of the hospital. During my fieldwork a very different slogan was prevalent: "Urbanize the rural countryside!" (*nongcun chengshihua*).

The breeding of wild geese as a mode of life is a response to the uncertainty cast by the "shadow" of modernization in the rural village. The breeding of wild geese mixes high and low, active and passive in ways that troubled the category of "peasant" and yet refused to wholeheartedly embrace the model of urban life. Ye Xing, the breeder of wild birds, constantly distinguished his life path from his peers who had left for the cities driven by desires for commodity consumption (*xiaofei*) and "clean" work. But he also distinguished himself from the peasant cultivator, bound to cyclical subsistence on the fruits of the soil. He described the large "risks" (*fengxian*) of poultry and livestock farming and outlined complex strategies for controlling them. Although he can earn two hundred thousand RMB per year, most of his income is invested back into the animals. He had suffered a few years earlier when high water flooded his pond, causing the geese to sicken and die. Now he distributed risk by investing in a wide range of species and breeds, a method which he called "diversification" (*duoyanghua*).

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<sup>262</sup> On the peasant's leadership during the early years of the revolution, see William Hinton, *Fanshen: documentary of a revolution in a Chinese village*. William Hinton, *The great reversal*

<sup>263</sup> 郑有贵 李成贵 主编, 《一号文件与中国农村改革》安徽人民出版社 (2008).

<sup>264</sup> Most famously, in the statement and subsequent report on the "three rural problems" (*San Nong Wenti*).

<sup>265</sup> Xin Liu, *In One's Own Shadow: An Ethnographic Account of the Condition of Post-Reform Rural China* (Berkeley: University of California Press, 2000).

Along with wild geese, he now raises cows, sheep and chickens, with plans to add dogs to the farm soon. If one of the flocks or herds fails, he can make up the losses in his other endeavors.

Much has been written about rural entrepreneurship as a novel social role or status group, and there are doubtless many comparable elements to the mode of life of the breeder of wild geese. But here I propose a different approach that foregrounds the ethical uncertainty of the life and outlook of wild goose farmers such as Ye Xing.<sup>266</sup> The practice of breeding wild geese can also be seen as an activity of self-formation, situated within the uncertain shadows of contemporary rural China. By breeding wild geese Ye linked himself, materially and narratively, with the rapid expansion of "wealth" in contemporary China. After all, it was the "unusual" quality of his product that caught the attention of wealthy elites. At the same time, he rejected the idea of the city as the only form of possible good life.

Sometimes, however, his narrative became overdetermined by the dominant geography of modernization, in which rural is figured as past and city as future. Looking around the at the fallow fields left behind by others who had gone to the cities, as well as newly built multi-story homes constructed by the migrant returnees become village "big shots," Ye recollected a childhood when the village way of life was different. Everyone lived in *pingfang* (one-story homes) and there was no air conditioning. At night in the summers, the whole village went on the roofs to sleep. Since no one could sleep well in the heat, villagers went visiting, running to their neighbors' doors even to the latest hours. On the roofs, a constant low din of conversation kept up throughout the night. These days, Ye lamented, each family goes inside their home in the evening and turns on the air conditioning. With doors and windows closed and the white noise of the fan, there is no way you can hear your neighbor anymore.

But when he considered his future, and even more so, the future of his son, he gave voice to a very different dream. His greatest hope was to earn enough money to establish his son in some business or employment in the city. There is nothing to do here (for a child), he said. Nothing to play. Nothing to buy. A few days later, I ran into Ye Xing and his wife at the bus station in Wucheng. His wife was headed to the county seat to buy toys for their son.

After a failed attempt to organize a "farmer's cooperative", he had given up working with others. He worried about the tendency for disputes and betrayal. *Renxin ge dupi*, Ye told me, "Human hearts are separated by the walls of the stomach." The saying refers to the hermeneutic difficulty to understand the other, as human hearts can never come into physical contact. But Ye also referred to a specific historical situation of isolation and uncertain identity, as the village is transformed by the pursuits of "self-interest". The choice of idiom was appropriate for one who raised the 'rare and unusual' wild geese: stomachs and their ever more refined desires keep hearts apart.

Ye Xing's narrative shows how the term "peasant" is not only objectively inaccurate,<sup>267</sup> but also subjectively refused by the breeder of wild birds, amidst the changing and uncertain norms of life in rural China. If the scientific studies about influenza at Poyang Lake ascribed "farmed wild birds" to the practices of "farmers," they provided the conceptual material for a new kind of discourse about the rural that replaced the peasant with a different, but no less

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<sup>266</sup> On the distinctions of 'characters' and 'social roles' see Alisdair MacIntyre, *After Virtue*, (Notre Dame: Notre Dame Press, 1984); Liu Xin, *The otherness of self: a genealogy of the self in contemporary China* (Ann Arbor: University of Michigan Press, 2000).

<sup>267</sup> The inaccuracy of the term "peasant" as a social category due to changing conditions in contemporary rural China has been noted, for example in Flemming Christiansen, Junzuo Zhang, eds., *Village Inc.: Chinese rural society in the 1990s* (Honolulu: University of Hawaii Press, 1998).

stable, figure of the rural producer--and wholly neglected the ethical uncertainty articulated in Ye Xing's narrative. It was during my fieldwork among elite Chinese veterinarians,<sup>268</sup> that I came to recognize how the conceptual grounds of these influenza studies intersected with, and even encouraged, an important shift in elite Chinese discourses about the nation and its 'rural problems'.

Gao Lili is a veterinary epidemiologist who was employed in the FAO ECTAD office in Beijing during the first year of my fieldwork, before going on to a Ph.D degree in Canada. She told me one day in 2010 that China had greatly improved in recent years. Speaking in a combination of English and Chinese, she told me a story about rural producers--one I soon found to be a commonly retold tale. The story is about neighbors. A villager builds a chicken farm and makes a lot of money. So the neighbor builds the same kind of chicken farm. Then there are too many chickens, the price drops, and everyone loses money! Adding an epidemiological supplement to the oft-told tale, she said that the increased poultry density would result in increased risk of disease transmission and environmental pressure.

Lili told me that this is how things used to be. But now, "farmers" are becoming more "realistic," she said in English, and clarified that by "realistic" she meant the Chinese term *lixing* [common Chinese-English dictionary translation = 'rational'].<sup>269</sup> This realism or rationality had come about because of greater access to information, allowing more accurate identification of prices, risks, and possibilities. She gave an example of how access to information had improved. Back in 2004 she had been teaching at a veterinary school in Henan, and the government hired her to make a poster explaining the avian flu epidemic. But now such posters are hardly needed because everyone is on the internet, she explained.

A study of government compensation schemes for the culling of poultry following outbreaks of avian influenza, a dissertation written by Mei Fuchun, shows how discourses about economic rationality can be deployed to criticize figurations of the "peasant" as morally suspect or backwards.<sup>270</sup> At the center of Mei's text is an analysis of an avian influenza outbreak in Liaoning province, told through the experience of one family. One day, the father discovers some birds sick and dying. He calls in the local veterinarian, who diagnoses Newcastle disease and sells them some medicine for the birds. His son, however, worries that it is avian influenza and suggests to his father selling the remaining birds immediately. The father refuses. More and more birds die and the son, fed up, sells his own birds for 10 Yuan each, while the father still won't sell. Although there is a taboo among villagers forbidding the discussion of disease among the flocks, rumor spreads that many chicken flocks in the village are suffering epidemics.<sup>271</sup>

Soon, the government recognizes the epidemic and declares that all birds in the village will be culled, and culled birds will be compensated at 10 Yuan per bird. However, the declaration states birds that *were already killed or died will not be compensated*. Meanwhile, word gets around that a *ji fanzi* (chicken trader) will purchase dead birds for 1 yuan. Many people do sell birds to this trader. According to the villagers, the chicken trader shipped the birds

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<sup>268</sup> Introduced and discussed at greater length in chapter five.

<sup>269</sup> Six months later, when she told me nearly the same story but included the specific situation of her experience of a recent reemergence of 'backyard' poultry farms in Henan villages, she used the word *lizhi* (reason).

<sup>270</sup> 梅付春, *政府应对禽流感突发事件的捕杀补偿政策研究* [博士论文] (Compensation Policy for Eradicated Birds Against Avian Influenza) Ph.D. Thesis. (2009).

<sup>271</sup> This public awareness largely came from seeing the "chicken trader's" truck leaving the village each day with new loads.

to many locations, including as far away as Beijing. Outbreaks start emerging in other areas of China, which Mei attributes to contagion from the transport of dead birds.

However, Mei rejects the journalist's claim that the trader's "forgetting of moral principles at the sight of profit" (*jianli wangyi*) or the farmer's "failure to consider the good of the whole" are moral failings responsible for the catastrophe. That is, Mei does not want to denounce the farmer's as immoral persons. On the contrary, Mei argues their action "objectively speaking, has its own rationality" (*helixing*). Mei therefore points the finger at the compensation policy itself. He argues that since the chicken trader offered 1 Yuan for dead birds, and this 1 Yuan exceeded the government's offer of zero Yuan, the compensation policy *produced* this unfortunate outcome.

As we can see, Mei's argument is founded on a particular notion of the human subject and its rationality: the *economically* rational actor. According to this notion, rational actors in any situation will make choices calculated to benefit their individual interest. Moreover, they have the *capacity* and *habit* to do so. As Mei argues, the implications of this notion of the human subject for policy are clear: If one doesn't employ policy and law through recognizing and guiding the self-interest of actors, this self-interest will disrupt and ultimately foil the policy. In the story, farmers continued to sell dead chickens to the "chicken trader" even during the period when the village was under 'quarantine' because, according to Mei, they knew the government would not compensate them for these dead chickens.

This new figure of the "farmer" as a *rational* actor oriented towards financial incentive are replacing the figuration of the "peasant" as morally backward and irrational. They also intersect with government projects to increase the "productivity" and quality (*suzhi*) of rural inhabitants, from birth control policies to "civilization" campaigns.<sup>272</sup> However, although these discourses of the rational farmer contribute to self-understandings of wild goose *yangzhihu*, I argue that the fundamental uncertainty about the worth or "quality" of the enterprise remain far more significant. Those who cultivate wild birds do not do so only because they have discovered a market opportunity and, like dogs to a bone, will join pursuit. They cultivate wild birds in the process of crafting a biography with a sense of self-worth amidst the uncertainty of being "rural" in contemporary China.

### *Chairman of the Birds*

On his business cards, Liu Fenglian refers to himself as "Chairman of the Board" (*dongshizhang*). Although the simple farmhouse where he based his operations may seem discordant with the terminology of the corporate firm, these are no private delusions. Liu has been featured for his wild bird farm enterprise on Chinese central television four or five times, as well as in journals such as *Qiyejia* (Entrepreneur) and *Xin Shengyi* (New Business). These journalistic accounts further develop the character of a man who, as one article begins, "had been a peasant" (*dangguo nongmin*) before he began to experiment with various forms of commercial farming. These journalistic accounts frequently refer to his struggles to innovate (*chuangye*, sometimes *chuangxin*) new economic forms; as one puts it, beginning his wild goose farm involved battling to achieve an entrepreneurial career in the face of defeat (*lubai luzhan*).<sup>273</sup>

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<sup>272</sup> Anagnost; Kipnis;

<sup>273</sup> "鄱湖边上养雁人" 江西新闻 (2013-4-15) <http://www.jxnews.com.cn>. [Breeding swan geese on the shores of Poyang Lake" *Jianxi Xinwen* (2013-4-15)].



Ann Anagnost has analyzed the developmental narratives of the post-reform period that situate the peasant as passive object to be turned into an active subject of the nation.<sup>274</sup> The journalistic accounts figure Liu Fenglian as advancing along much the same track. It should be further specified, though, that the metric of advance is not wealth or civilization, but *creativity* (*chuangxin*).

Most of the time I spent at the farm I spent with Liu Fenglian's son, Haohua, who organized much of the day to day management of the farm. Haohua had graduated from Jiangxi Normal University with a degree in athletics a few years earlier. Upon graduation he had attempted to become a prison warden, following a good friend's example, but had not passed the examination. He then took a job as a fourth-grade math teacher, but had left after only one year, frustrated by the obsequious social world among the school leaders and local officials. He returned home to work with his father, hoping, he explained, to construct a life "between city and country" as a rural "creative innovator" (*chuangyezhe*).

When I was in Jiangxi for fieldwork in 2011, Steve Jobs' autobiography had just been published and it was widely sought after and discussed. One man I met at a small jazz bar on Yongshu Road in downtown Nanchang asked me excitedly about the possibility of bringing him a copy of the edition published in Taiwan if I went overseas, as the mainland version had some sections removed. The request was perplexing, as it was hard to imagine what might have been cut out, but reflected the enthusiasm for the text and its association with a foreign ("American") way of life. Liu Haohua was also a fan of Jobs. When I was planning to take a short trip out of the country, he asked me if I could bring back two iPhones, one black and one white, for himself and his girlfriend because the newest models were impossible to buy in China.

But Haohua also viewed his work on the wild bird farm in a language of venture capital, creative industry, and innovation adopted from the world of Jobs and Bill Gates. Farming wild birds was about being in control of one's time, about creating new things, about branding and marketing. The contrast with employment as a teacher was clear and present, not only because of Haohua's brief stint as instructor, but also because his girlfriend was a teacher in Nanchang. She was constantly threatening to break with Haohua because she viewed his work on the farm as unstable and associated with the 'soil' of the peasant life. But Haohua countered her by figuring their different careers in other terms. For him, his life relied on the risk-taking of innovation in contrast to the stifling security of government or office work. The walls of this security were literal and physical: his girlfriend could rarely go out with him, as she lived with her colleagues in a *danwei* building with curfews and rules about visitors. Haohua proudly put his ethic into speech: he chased the flowing changes, he said, and avoided the flatness of stability.

Attributing a creative agency to the practice of breeding wild birds, then, is not due to the fact that everyone should be understood as having such agency. I am not *making* the subaltern speak. Innovation is not everyone's ideal. Rather, breeding birds with the *objective* of creating new forms is a product of a specific historical conjuncture, between discourses in which "productivity" and economic growth are associated with the creative industry, and yet at the same time the ability of rural residents to inhabit these discourses is contested and uncertain. Still, the material outcomes of these practices are no less real: new things are brought into the world and the "natural objects" of scientific research are transformed. Indeed, Liu Fenglian and his son kept making new things. When I returned to Jiangxi in late October 2011 after a few months away, I found that an entirely new venture now attracted their attention.

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<sup>274</sup> Anagnost, *op cit*.

## *Miraculous Duck Eggs*

“Be-beep! Route currently being replanned ! ” is blaring incessantly from the GPS screen on the front dashboard of the brand-new Honda Accord driven by Liu Haohua. Liu is on the phone with his father, listening to his fathers directions and taking erratic turns. We are carrying a couple of wild duck eggs in the car, the product of what Haohua calls a new "experiment" (*shiyān*) at the Poyang Lake farm. The wild ducks are fed, in addition to their normal feed of a mixture designed for egg-layer ducks, a special concoction of Chinese medicines. This medicinal feed, developed by a scientist in Beijing, acquired a national patent in 2010, and won a gold prize at the Second International Agriculture Exposition in 2011. Liu Fenglian's idea is that the medicines in the feed will travel through the blood of the ducks and be retained within the eggs. When humans eat the eggs, they will also receive the benefits of the medical substances, which include improvements to the blood and heart, strengthening of resistance to viruses and bacteria, cures for high blood pressure, as well as for dementia, back pains, and even aging skin. According to early “experiments,” Liu Fenglian told me, two people who had eaten the eggs over six days, had significant blood pressure reductions, one from 168 to 135, the other from 124 to 93. Haohua, with his father's shouting directions into his ear, is trying to find the Quality Inspection Bureau in order to get the eggs approved for market sale.<sup>275</sup>

“Isn't it miraculous?” Liu Haohua asked me, after hanging up the phone. “Won't medical doctors think these duck eggs are miraculous?”

I replied, almost mechanically, with a too lengthy anthropological discourse on the difference between Western and Chinese medicine, suggesting that Western doctors will not be able to account for these "miraculous" effects within their epistemological norms.

“But this should benefit not only China, but the whole world,” Liu protested.

“The problem is that very few Americans have the habit of eating duck eggs.”

“Just bake it into bread then,” Haohua said with satisfaction and a small smile on his face.

Later that month, I traveled with Fenglian and Haohua to the 2011 second quadrennial China International Forestry Expo held in Yiwu, Zhejiang province. The trip revealed how the practice of transforming poultry (re)production into innovative commodities was, at the same time, a technique of the self for transforming rural residents into "innovators."

The State Forestry Administration organized the Expo. In China, the farming of wild animals is regulated by the State Forestry Administration rather than the Ministry of Agriculture. Licenses are issued essentially for the right to take or hold "wild" species (even if these wild species are purchased from human-operated chick incubators). A wild goose farmer explained to

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<sup>275</sup> We never arrived at the bureau. His father, after calling an official there, realized that the eggs do not have to be approved for sale because they are still "raw" and have not been processed (*jiagong*). "Its just the same as the duck eggs sold by farmers at the Xiaolan Poultry Egg Wholesale Market," Haohua reasoned.

me that in China the farming of wild geese is considered a 'specialty husbandry' (*tezhong yangzhi*) distinct from domestic (*jiayang*) livestock farming. As such, he said, farmed wild geese "belong to Forestry, not Agriculture." As a result, veterinary standards and rules applicable to domestic poultry were not enforced on farms raising wild bird species, even when farm conditions (feeds, housing, etc.) were identical to the domestic poultry farms. Scott Newman worriedly noted the absence of standard vaccination for avian influenza in a context in which universal vaccination was the law for all domestic poultry. In a series of visits to wild bird farms in the Poyang Lake Area, he reported at a workshop in Beijing, Newman found different vaccination regimens at each farm, ranging from weekly immunization to once every six months to a single time immediate before market sale.<sup>276</sup>

The city of Yiwu calls itself "the capital of small commodities." As we approached the Expo Center in a hired cab, we dipped into a small tunnel and suddenly the car seemed to go silent. The asphalt road was brand new, and of such a high grade quality that the wheels of the car on the road made no sound. The Expo Center was only recently completed. Massive balloons, blue, red, and yellow, flew above; but rubble and dirt appeared only a few feet beyond the far side. Yiwu, or this neighborhood of it, felt as if it had sprung from nothing. We struggled to find a hotel and settled for a watery noodle soup for dinner. And yet people said more money flowed through Yiwu than some of the more well known special economic zones. Haohua was quite excited. "There are more BMWs in Yiwu than in all of Germany" he declared to a cab driver.

Inside the expo center, we went to a section reserved for exhibitors from Jiangxi Province. Amidst a large display for some kind of wood from the forested mountains of the south of the province, we propped up a small poster for Po Ye Wild Animal Breed, Co. But Liu Fenglian looked cross. We soon walked to another building where a "National" pavilion was located, which took up one whole building of the Expo center. In one corner of the building was a section devoted to "wild" products. What things were wild? In the middle ten mink coats hung on a rack. To one side some kind of medicinal plant, long brown and bulbous was displayed behind glass. On the other side a man hawked actual snake oil. In the far corner, taxidermy birds perched, still as if pausing to watch us, colorful plumage above green AstroTurf, with price tags as high as ten thousand RMB. In one small section of this corner a sign confirmed the existence of a Wild Duck and Goose Zone (*yeya dayan qu*). The small area was already filled by three or four exhibitors and their wares. We ran into the Dean of the China Wild Animal Protection Committee, an organization of which Liu Fenglian is also a member: he is a Vice Dean of the China Goose and Duck Breeding Specialization Committee. Fenglian persuaded him to make a phone call and soon we were setting up egg boxes and promotional materials on a display case next to the other wild bird farms.

Later, back in the hotel, we sat on Fenglian's bed with the disassembled cardboard boxes, egg crates, and boxes of eggs around us. I folded the cardboard into boxes while Haohua carefully placed eggs into the crates. Then we put the crates in the finished boxes. When we got back to the Expo Center with ten or twenty boxes of eggs in our hands, we suddenly found ourselves locked out. None of us had the registration badge that was necessary for admission. Haohua told his father to call the Dean again. Fenglian hesitated, but finally did, and the man

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<sup>276</sup> Newman, *op cit.*

arrived to pass us a badge so we could enter, one at a time. He later admitted to me, more than a little disgruntled, that he had been just about to sit down for dinner when Fenglian called. Pushing aside some of the other items on display, we put the boxes of eggs against the wall. Next Fenglian got out a large poster and attempted to put it up straight in front of the display counter. A man from a Sichuan wild goose company complained, and Fenglian moved it to the side, nearly knocking over one of the taxidermy birds as he fumbled with the poster stand.

An official from the Expo approached the display counter while Fenglian was away. He was carrying white stickers and handing them out to the exhibitors. The representative of a Zhejiang-based wild bird farm received four, and after some pleading with the official, another three. He started writing something on one of the stickers, then peeled it off and stuck it on a gift box (*lipin*) of cured wild duck meat. They were price tags. Liu Fenglian was busy elsewhere, and when he returned, realized he had missed this all important distribution. He began to beg the Zhejiang man for one sticker, which he gave him, begrudgingly, muttering that he didn't even have enough for his own products. Fenglian wrote 160 Yuan on the sticker and placed it on a box containing sixteen eggs.

Liu Fenglian certainly proved his tenaciousness. When the expo opened to visitors the next day, he stood out in front of the counter, asking everyone who walked past whether they had high blood pressure. If they made any hint of response, he offered to give them a box of the miraculous eggs, no cost, in exchange for their contact information. Within fifteen minutes he had handed out two boxes of eggs, many more pamphlets, and had collected ten or so contacts in his small black notebook. As I watched him hawk the eggs, I saw no peasant, no farmer either, but someone struggling to capture a place for himself in the story of China's 'miraculous' economic growth.

### *Conclusion*

In describing the practices through which Liu Fenglian or Ye Xing cultivated their birds and so cultivated themselves, I hoped to show how their modes of living exceed the determinations of the category of "farmer," attributed to them in the NIH group influenza studies. Second, I argued that this discrepancy is not because we must unearth the creative agency of everyone, but rather because of specific contemporary conjunctures in which "creativity" has become a mark of self-worth and an ideal even in some seemingly unlikely places, such as poultry farms in the Poyang Lake region. It is a common sociological claim that distinctions in social status are marked through differences in qualities that are consumed; however, I add to this that the "creative" act of marking qualitative differences in production is also a practice of distinction; the wild bird farmer makes his or her own pleas for distinctive worth, hoping in the process to emerge out from under the shadow of the modernization process.

Finally, third, I suggest that in the attempt to define the practice of breeding wild birds as the "farmer's" pursuit of economic self-interest, the NIH group reveal a significant ethical problem: a gap appears between the practices they devote toward the construction of their scientific object, on the one side, and the (concurrent) practical re-making of that same object by poultry breeders, on the other. As I argued in the first chapter regarding the relations between international scientists and China more broadly, rather than an impossible breakdown between a "scholastic" point of view and the "logic of practice," this gap between knowing and

(re)producing became the object of ethical work aiming to reconstruct the relation. The figuration of the "farmer" as a kind of *homo economicus* was, after all, one attempt to *account* for breeding practices and thereby reconstruct this gap. But it was disastrously poor at doing so, and, in some sense, the NIH group scientists knew it. This was why the scientists held contradictory views of "farmers": they didn't know anything about farmers, except they knew that farmers were economically self-interested. And this was also why the scientists sometimes sought my help to produce a better anthropological understanding of these breeding practices.

I was able to present a paper on one portion of my work during the 3rd International Workshop in November 2011, in which I described a very early version of the final chapter of this dissertation. In the presentation, I suggested that "farmers" make qualitative judgments based on non-economic criteria before they even enter into market economic situations. I am not sure how much impact it had: although some scientists commented that they found it interesting, they were mostly referring to the fact that I read the paper rather than making a PowerPoint presentation. It was not the anthropologist that would help them bridge this gap; for they had found another figure they believed was better suited to the task. This was the Chinese state-employed veterinarian.

**PART TWO**

**VOCATION AND RELATION**

## Chapter Five

### The Office Veterinarian

In the first part of the dissertation, I argued that building global health knowledge and preparedness for avian influenza involved moving onto Chinese grounds, interrogating the particularity of Chinese "working landscapes" and developing working relationships with Chinese of many kinds: government officials, laboratory scientists, provincial veterinarians, drivers, marketers, and poultry breeders. But global bird flu preparedness programs have also attempted to transform Chinese institutions and subjects into participants in global surveillance programs. In addition to problems of political obscurantism (which I discussed in Chapter Two), global health experts and observers claimed China simply lacked the technical ability to detect and control emerging zoonotic diseases such as avian flu. As Joan Kauffman writes in an article on the avian flu H5N1 outbreak, "China may be one of the weak links in global preparedness for 2 main reasons: it lacks transparency in acknowledging outbreaks, and its health care system is not up to the task of putting in place systems to ensure preparedness of the capability to contain the epidemic if it begins in China's rural areas." She further specifies that one "area of concern is China's veterinary surveillance and its ability to control its widespread live-animal markets or to apply restrictions and controls to its backyard poultry- and duck-raising industries."<sup>277</sup>

When I arrived in China for fieldwork, I found that the main project of the FAO ECTAD office in Beijing involved an effort to improve China's veterinary surveillance of livestock diseases through training of government-employed veterinarians in "field epidemiology." Funded by USAID, the FAO office promoted this training program as a bulwark of global health, not only for avian influenza but also for other unknown emerging zoonotic diseases. Some analysts represent extensions of global health such as the ECTAD training program as examples of post-Westphalian global governance system that imposes increasing limitations on national sovereignty.<sup>278</sup> David Heymann, who directed the WHO's response to SARS, writes that "The global solidarity in the detection and validation of, and response to, the SARS outbreak has blurred the concept that states are sovereign and reign supreme over their territories and peoples, and by so doing has established new norms and standards in international public health."<sup>279</sup> Scholars critical of global health broadly share Heymann's framework of analysis, arguing that such programs, including surveillance systems and new norms of health or diseases, are examples of European and North American global hegemony since they are designed to detect and control diseases overseas before they reach ("Western") domestic shores.<sup>280</sup>

In this chapter, I instead provide an account of my participant-observation in the FAO ECTAD's training program for Chinese veterinarians, the China Field Epidemiology Training Program for Veterinarians (China FETPV). Rather than a simple extension of global health techniques and ways of knowing, I focus on how Chinese veterinarians adopted and engaged

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<sup>277</sup> Joan Kaufman, "China's health care system and avian influenza preparedness" *Journal of infectious diseases* Vol. 197, Suppl. 1 (2008): S7-S13.

<sup>278</sup> David Fidler, "SARS: Political pathology of the first post-Westphalian pathogen" *Journal of Law, Medicine and Ethics* Vol. 31 (2003): 485-505.

<sup>279</sup> David L. Heymann, "SARS and emerging infectious diseases: A challenge to place global solidarity above national sovereignty" *Annales of the Academy of Medicine of Singapore*, Vol. 35 (2006): 350-3.

<sup>280</sup> See for example Nicholas B. King, "Security, Disease, Commerce," *op cit*.

with the discourse and practices of global health during the training program. I also show that rather than an effacement of national sovereignty, the adoption of global health forms for livestock disease control resulted in reformulations, critiques and even expansions of China's national veterinary service. Most importantly, the conjuncture refigured the veterinarian as an ethical project of the nation.

### *Reform and the veterinarian*

Elite Chinese veterinarians diagnose the origin of China's livestock pathologies, from food safety to emerging diseases like avian influenza, in the veterinary vocation itself. "To understand China's veterinarians, you need to know political economy," Cai Haifeng, the most senior 'national' veterinarian at the FAO ECTAD office in Beijing, told me. "Changes at the level of ideology (*yishi xingtai*) have not kept pace with changes in relations of production and means of production."<sup>281</sup> What he meant, he explained, was that the growth and structural transformation of China's livestock production during the reform era, a phenomenon some observers have called China's livestock revolution, had not been matched by corresponding improvements to the abilities of veterinarians. This account differs from those that identify expansion of livestock production, particularly in capital intensive forms, as pathological in itself, such as those that blame large-scale broiler operations for the emergence of novel avian influenza viruses.<sup>282</sup> Cai points to the dynamic historical relations between knowledge and practice, between the science of animal disease and the means of livestock production, and locates pathology in their discordancy or lack of correspondence.

Other Chinese veterinarians employed in the ECTAD office shared Cai's perspective. Ning, who was *liason* between ECTAD and China's Ministry of Agriculture, argued that livestock diseases had become a crisis in China because the 'quality' (*suzhi*) of veterinarians was too low. The veterinary 'software' (*ruanjian*) was no longer adequate to China's rapid material growth. He argued that bricks and mortar can be laid very quickly, but too often the buildings are left empty. The image of the notorious "ghost cities" of China's real estate boom illuminated Ning's view that livestock production has grown rapidly without qualified experts to care for the animals and their diseases.

Chinese veterinarians at elite institutions, such as Cai and Ning, situated the FETPV training program within a broader national project of veterinary reform (*shouyi gaige*). China's veterinary reform began as a response to the avian influenza epidemic. In 2004, the MOA created a "Veterinary Bureau" within the Ministry of Agriculture, including the appointment of a "Chief Veterinary Officer" (*sbouxi shouyi guan*) to conduct international veterinary diplomacy. The following year, China's highest governing body, the State Council, issued a directive acknowledging and calling for extension of the national veterinary reform.<sup>283</sup> The directive stipulates that the reform aims to bring China's veterinary administration system into line with 'international common practice.' However, this embrace of the international is widely

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<sup>281</sup> On ideology in China, see Franz Schurmann, *Ideology and organization in communist China* (Berkeley: University of California Press, 2006).

<sup>282</sup> See Mike Davis, *The monster at our door: the global threat of avian flu* (New York: New Press, 2006); Robert Wallace, et al, "Are highly pathogenic avian influenzas a product of China's historical present?" in Tamara Giles-Verenick, et al, eds., *Influenza and public health: learning from past pandemics* (Washington, D.C.: Earthscan, 2010).

<sup>283</sup> 《国务院关于推进兽医管理体制改革的若干意见》国发15号（2005）。



understood as a project of national strengthening. As one Chinese veterinarian indicated in a published commentary, aligning China's veterinary service with the international norm simply indicates the growing *internationality* of the nation: the nation's power is confirmed by its international role and significance.<sup>284</sup>

Although the veterinary reform involved a wide range of initiatives, the reform centers on reconstructing the "official veterinarian" (*guanfang shouyi*), a neologism coined to describe the reformed government-employed veterinarian. As the State Council directive instructs,

“An official veterinarian (*guanfang shouyi*) is a national state (*guojia*) veterinary worker who holds recognized qualifications or license, legal authority or government appointment, and the power to issue health certificates. With reference toward the international common practice, the national state has begun to slowly institute an official veterinary system (*guanfang shouyi zhidu*). Currently existing veterinary workers are required to improve all-around quality (*zonghe suzhi*) and professional level through professional training, and following recognized licensing or government appointment, to slowly enter the official veterinary staff.”<sup>285</sup>

In the wording of the directive, livestock disease as a problem of the nation is embodied in the 'quality' (*suzhi*) of the official veterinarian. Anthropologists have previously identified *suzhi* as a keyword of China's post-reform era discourse. Translated loosely as moral or personal 'quality', the concept of *suzhi* links practices of self-making with metrics of status distinction and projects of national development. According to Andrew Kipnis, the term *suzhi* "marks the hierarchical and moral distinction between the high and the low and its improvement is a mission of national importance."<sup>286</sup> Ann Anagnost traces the term's prevalence and new meanings to China's population and birth planning, which state officials propounded as a necessary manner to improve the quality of the population (*renkou suzhi*) by reducing its quantity.<sup>287</sup> Subsequently, the term became important to the field of education, when some Chinese educators called for methods of "quality education" (*suzhi jiaoyu*) to replace rote learning and political indoctrination. Today, the term is in widespread use as a metric for distinguishing status and etiquette. For example, "low quality" (*suzhi di*) is an insult often applied to migrant workers or others of low standing, and to describe forms of behaviour considered crass from the standpoint of middle-class sensibilities.

However, scholarship has not analyzed the use of the term to describe the practices of self-making and status distinction among government officials or scientists. Nor has the association of *suzhi* with norms of professionalism and "international" standards been examined. In this chapter, by describing how "international practices" are adopted by Chinese veterinarians during a training program, I argue that these global assemblages produce more than just a technical or even epistemological change. In addition, the international standard must be understood in terms of how it is adopted within the *suzhi* mechanism of mobilizing improvements to self and nation through hierarchical distinctions. In this sense, the international

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<sup>284</sup> 张辅, “试论新形势下官方兽医与执业兽医制度的建立” *中国动物保健* Vol. 14, No. 1 (2012): 2-3. As historians have shown, the rise of the nation-state form and nationalist movement in China, as in many parts of the world, was a product of the expansion of global geo-political forms. See especially Prasenjit Duara, *op cit*.

<sup>285</sup> 《国务院关于推进兽医管理体制改革的若干意见》 *op cit*.

<sup>286</sup> Andrew Kipnis, "Suzhi: a keyword approach" *The China Quarterly* Vol. 186, No. 1 (2006): 295-313.

<sup>287</sup> Ann Anagnost, "The corporeal politics of quality (*suzhi*)" *Public Culture* Vol. 16, No. 2 (2004): 189-208.

standard holds value for enhancing *suzhi*; but as a result, it also produces social distinction and distancing. The paradox of veterinary professionalization in contemporary China, I will suggest, is that when the adoption of the international standard becomes attached to practices of *suzhi* distinction, the veterinarian expands the social distance separating her from her object of knowledge: the breeder and the livestock farm.

### *Embracing epidemiology*

The first meetings of the FETPV training program took place on the top floor of the Beijing Animal Hygiene Inspection Service, located in an awkward and dusty corner of Beijing north of the old Western Gate. A highway overpass crossed and divided the nearby streets, casting shadows into the gullies below, and seeming to signify that the area had been passed over but not carried along by China's rapid development. Indeed, Beijing had long since expanded beyond the immediate environs, which probably explained why no subway trains stopped anywhere near to here. The setting was appropriate, perhaps, because it closely resembled the self-descriptions by the veterinarian-trainees of their vocation. "Very few veterinarians get to study epidemiology," one trainee told me as we walked beneath the overpass, "We're still very behind." This sensibility of backwardness, of falling behind, referred not only to the position of China in relation to the developed ("Western") world. Perhaps most of all, it referred to the position of government-employed veterinarians relative to other professions *within* a rapidly developing China. Public health, which after SARS was radically reworked and reinfused with central investments, was the primary contrast. At other times, some trainees complained about money earned by veterinarians working in the private economy, including above all the private physician of pets.

Inside the Animal Inspection Office, in a sleekly global conference room, a new picture of the veterinarian was being drawn. Dirk Pfeiffer, an expert on veterinary epidemiology from the Royal Veterinary College in London, provided the introduction to the field epidemiology training program. His diverse audience included the Chinese veterinarian trainees, the staff of the FAO ECTAD office, the Asia representative of US AID (the primary funder), and myself. In his opening remarks, Pfeiffer claimed that veterinary medicine faced an epochal moment. When we think of the vet, he said, we usually imagine the kind man in rubber boots out at the farm, inspecting a sick cow or horse. The vet is traditionally concerned with the health of the individual animal. But the increase in emerging zoonotic diseases--from mad cow to bird flu--has put this doctor of individual animals at a loss. Today, Pfeiffer suggested, animal disease must be addressed at the population level, the only scale at which emerging outbreaks can be identified, prevented, or eradicated. If today's greatest disease threats require a One Health approach that deals with wildlife, domestic livestock, and humans within a single frame, 'population' is the concept that can constitute this frame.<sup>288</sup>

Pfeiffer's probably overdrawn historical contrast captures an important distinction, both epistemological and institutional, between a medicine of individuals and a medicine of populations. Michel Foucault's well-known work on "biopower" is grounded in a highly refined

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<sup>288</sup> The narrative is developed fully in Pfeiffer's textbook, *Veterinary Epidemiology: an introduction* (Ames, Iowa: Wiley-Blackwell, 2010). Following the first module of the training program, an unpublished Chinese translation of the text was passed among the trainees.

analysis of the "apparatus of security" and its object: the population.<sup>289</sup> Foucault argues that in the late 18th century, the 'population' appeared as an object of government when for the first time human collectivities came to be seen to have "natural" qualities--certain regularities or patterns of production, psychology, comportment, health. Disease, along with scarcity, was one of the primary sites for working through new techniques of governing the population through security. Disease came to be located within a series of probable events--and thus, statistics and probability rose to importance as the tools for governing epidemics.<sup>290</sup> Public health in its modern sense became possible because of techniques that took up "man in the mass"<sup>291</sup> and situated human disease within "living conditions."<sup>292</sup>

In veterinary medicine, the use of the concept of 'population' appeared much later. According to Susan D. Jones, the use of an 'economic' rather than 'medical' approach to animal disease, including a focus on herd health rather than individual animals, dates from the rise of factory farming in the U.S. mid-20th century.<sup>293</sup> Others date the rise of specifically epidemiological approaches to even later. In an account of the control of foot and mouth disease (FMD) in the United Kingdom, Abigail Woods argues that awareness of the scale of the FMD epizootic was a consequence of policies for controlling it. For example, threats of quarantine led farmers to be more attentive to the symptoms of their herds; and reporting requirements created the basis for statistical knowledge. Yet methods for combating the disease, despite the collection of statistics, did not produce a technology of population. Approaches to controlling the epidemic were largely fought out between biomedical specialists (who thought control of the disease must pass through the laboratory) and veterinarians (who advocated slaughter as a feasible control tactic). According to Woods, the introduction of explicitly "epidemiological" approaches only dates to the use of statistical models to predict spread during the 2001 FMD outbreak.<sup>294</sup>

Although overdrawn, then, Pfeiffer's narrative broadly captures epistemological and institutional changes in European and American veterinary medicine over the course of the 20th century. However, Pfeiffer's narrative was highly inaccurate if taken to describe the experience of its primary audience, the Chinese veterinary trainees. Although they shared Pfeiffer's sensibility of urgently needed change, their way of configuring this change was very different. For one thing, not a single one of the trainees treated the diseases of individual animals or worked at the scale of the proverbial barnyard, and many of them never had.

The trainees came from either Province, Direct-controlled Municipality<sup>295</sup> or National-level state veterinary institutions, making them very elite members of China's overall veterinary administration, and the very pinnacle of the proposed "official veterinary" (*guanfang shouyi*)

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<sup>289</sup> The most important texts are: Michel Foucault, *The history of sexuality. Volume 1. An Introduction* (New York: Vintage, 1990); Foucault, *Security, Territory, Population: Lectures at the College de France, 1977-1978* (New York: Palgrave Macmillan, 2007); Foucault, *Society must be defended: lectures at the College de France, 1975-1976* (New York: Penguin, 2004).

<sup>290</sup> On statistics, see also Ian Hacking, *The taming of chance* (New York: Cambridge University Press, 1990); Theodore M. Porter, *The rise of statistical thinking, 1820-1900* (Princeton, N.J.: Princeton University Press, 1986).

<sup>291</sup> William Coleman, *Death is a social disease: public health and political economy in early industrial France* (Madison, Wis.: University of Wisconsin Press, 1982).

<sup>292</sup> Paul Rabinow, *French modern: norms and forms of the social environment* (Cambridge, Ma.: MIT Press, 1989).

<sup>293</sup> Susan D. Jones, *Valuing animals: veterinarians and their patients in modern America* (Baltimore: Johns Hopkins University Press, 2003).

<sup>294</sup> Abigail Woods, *A manufactured plague: the history of foot-and-mouth disease in Britain* (Sterling, Va.: Earthscan, 2004).

<sup>295</sup> In China, four cities (Beijing, Tianjin, Shanghai, and Chongqing) have direct-controlled municipality status, meaning they are administratively at the same level as a province, directly beneath national government.

system. At the National-level, the trainees came from the China ACDC based in Beijing, or from the four bases of the China Animal Health and Epidemiology Center in Qingdao, Harbin, Lanzhou and Shanghai. At Province and Municipality levels, the trainees were employed at one of two kinds of office: Animal Hygiene Inspection Offices (*dongwu weisheng jiandusuo*), such as the Beijing office that hosted the first module; or Centers for Animal Disease Control and Prevention (*dongwu yibing yufang kongzhi zhongxin*). To for a moment gloss over the differences among these institutions, they all came from institutions devoted to the control of disease outbreaks in livestock herds or poultry flocks. What then did it mean that they did not use techniques of epidemiology? I found that Chinese veterinarians drew on a notion of the epidemic both modern and distinctively Chinese<sup>296</sup> that is embodied in Chinese institutions and practices of epidemic control referred to as *fangyi* (plague prevention or anti-epidemic). These *fangyi* practices relate to a different term and concept of the epidemic: *yi* or *yibing*, rather than the *liuxingbing* of epidemiology. To understand the specificity of the institutional and epistemological background of the Chinese veterinarians, we must turn to a pivotal moment in the formation of modern China and modern Chinese public health: the Manchurian pneumonic plague epidemic of 1910-11.

A large historical scholarship has pointed to the Manchurian pneumonic plague epidemic as a transitional moment in the decline of a diverse set of Chinese medical practices, the institutionalization of Western medicine in Chinese government, and the emergence of public health administration in China. But as Sean Hsiang-Lin Lei provocatively asks, the question is how and why all of these elements--state concern with disease, Western medicine, public health, epidemic control--came to China almost simultaneously, and with what epistemological implications.<sup>297</sup> The epidemic must be situated within the crisis of Qing sovereignty in the Manchurian region and the emergence of China as a modern nation-state (*guojia*). In the late 19th and early 20th century, both Russia and Japan had taken control over railways that cut across the Manchurian territory and were using the railroads to promote resource extraction and settler colonization. When the plague broke out, these two "semi-colonial" powers argued that the failure to control the epidemic rendered Qing claims to sovereignty moot or illegitimate. In order to bolster its sovereignty claims, the Qing empire enlisted Malay Chinese, Oxford educated physician Wu Lien-teh to investigate and control the epidemic. In the process, Lei argues, the Qing directly linked the adoption of Western medicine with China's claims to be a sovereign nation state.

Wu's primary ally was the microscope, which allowed him to make disease visible both in the laboratory petri dish and in the lungs of those stricken with plague. In this way, he was able to demonstrate to public officials and scientists from Russia or Japan that this plague had taken a pneumonic form, and was spreading through respiratory routes, thereby displacing the importance of rats and fleas (who had recently been identified as the means of transmitting bubonic plague). Lei argues that Wu's response contributed to China's "constructing, instituting and thereby coping with a new category of disease--*chuanranbing*." The concept of *chuanran*, derived from a broader set of meaning associated with the Chinese character *ran* (to dye, etc.), had long referred to processes of disease transmission understood to take place through contact

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<sup>296</sup> Or perhaps, more broadly East Asian, including Japan and Korea.

<sup>297</sup> Sean Hsiang-lin Lei, "Sovereignty and the microscope : constituting notifiable infectious disease and containing the Manchurian plague (1910-11)" in Qizi Liang and Charlotte Furth, *Health and hygiene in Chinese East Asia: policies and publics in the long twentieth century* (Durham, N.C.: Duke University Press, 2010).

and contamination.<sup>298</sup> However, since the cause of epidemics had previously been linked with the malevolent *qi* of the earth, the dominant response of government administration to earlier plague epidemics had been a set of practices known as *biyi* (fleeing the epidemic), including rites calling for spiritual assistance and procedures for evacuating an area. Unlike medieval and classical Europe, Chinese imperial governors never used force to control movement or segregate bodies: there was no quarantine and no plague town.<sup>299</sup> With his microscope and Western bacteriological training in hand and head, Wu specified plague as a *chuanranbing*--a disease caused by a microbe that transmits contagiously. Lei argues that the concept of epidemic was transformed not only by the epistemology of bacteriology but also by the practices employed in the control of the disease--disease notification, inspection, laboratory verification, and isolation or quarantine (*jianyi*). The concept of *chuanranbing* became closely linked with the several *notifiable* infectious diseases--yellow fever, cholera, plague and so on--which in modern China are grouped as the "eight *chuanranbing*" and which are seen to cause severe and rapidly spreading epidemics.

Lei argues that the Manchurian pneumonic plague outbreak shifted the governmental response to epidemics from *biyi* (fleeing epidemics) to *jianyi* (quarantine). However, a third term--*fangyi*--should be added to Lei's analysis of historical rupture. After the revolutionary overthrow of the Qing empire in 1912, the newly established Republic of China assigned Wu to lead a permanent, standing health force in Manchuria. Contemporary reports in English-language journals translate the name of the institution--*dongbei sanshen fangyi shiwu zongguanli bu* as North Manchuria Plague Prevention Service. But the use of the English term 'plague' obscures a key shift in the Chinese terminology. During the 1910 pneumonic plague outbreak, Wu's work was consistently referred to as *fang shuyi* (plague prevention), where 'plague' refers to the specific disease (*shuyi*, lit. rat plague). But in the 1912 institution, 'plague prevention' (*fangyi*) has become an autonomous binomial, referring to the generic prevention of epidemics. At the same time, as Lei suggests, *yi* has been transformed and narrowed in scope to refer to only those epidemics caused by *chuanranbing*. The techniques of *fangyi* (anti-epidemic, plague prevention) appeared as the institutionalization of practices devoted towards long-term prevention and control of notifiable (contagious) diseases.<sup>300</sup>

Institutionally, a nationwide "anti-epidemic system" (*fangyi xitong*) was established by Article 167 of the Government Administration Council in 1953. The system is said to be based on a Soviet model, because the Soviets had shifted from a plague-specific and outbreak control institution toward a broader, contagious disease prevention program during the 1930s. However, China's extension of anti-epidemic stations to every rural collective far surpassed the USSR

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<sup>298</sup> Angela Ki Che Yeung, "The evolution of the idea of chuanran contagion in Imperial China" in Liang and Furth, eds., *op cit*.

<sup>299</sup> On quarantine see Giulia Calvi, *Histories of a plague year: the social and the imaginary in baroque Florence* (Berkeley: University of California Press, 1989); on the plague town, see Michel Foucault, *Discipline and Punish: The birth of the prison* (New York: Vintage Books, 1995).

<sup>300</sup> Although much more attention has recently been given by historians to the rise of "public hygiene" (*weisheng*) in China, I argue that *fangyi* reflects important distinctions in East Asian modernity that are not well captured by the works on *weisheng*. See Ruth Rogaski, *Hygienic Modernity: Meanings of health and disease in treaty-port China* (Berkeley: University of California Press, 2004). On *fangyi*, see 杨念群, 再造"病人": 中西医冲突下的空间政治, 1832-1985 [Zai zao "bing ren": Zhong xi yi chong tu xia de kong jian zheng zhi, 1832-1985] (Beijing: Zhongguo ren min da xue chu ban she, 2006).

which only had a network of one hundred or so laboratories.<sup>301</sup> In China, the extension of the anti-epidemic system to include animal diseases and veterinary services occurred in conjunction with the organization of collectives (*gongshe*). In 1959, a livestock veterinary station (*xumu shouyi zhan*) was established in each rural collective. The responsibilities of the veterinary station included anti-epidemic work (*fangyi*), quarantine (*jianyi*), epidemic reporting, treatment of sick animals, as well as agricultural extension projects such as the improvement of breeds.<sup>302</sup>

The significance of the different concept of the epidemic associated with *fangyi* institutions became clear to me in conversations with veterinarians involved in the training. I had first been intrigued by comments that I noted above to the effect that veterinarians in China do not study epidemiology. Ning, the *liason* between FAO ECTAD and the MOA, told me he had never studied epidemiology in his undergraduate degree in animal medicine (*dongwu yixue*) or master's program in microbiology. He did, however, attend a required course on contagious diseases (*chuanranbing*). In the course, rather than studying population techniques such as probability or risk calculation, the students were assigned to memorize a list of contagious diseases, their etiologies, the elements that encourage their transmission, vectors, and how to block transmission routes. When I asked Ning what distinguished the two terms for 'epidemic'--*yibing* and *liuxingbing*--he paused. He pointed out that the ancient Chinese term used to describe when a large number of people in a city fell ill was *wenyi*. But he was careful to note that the meaning of the same term in ancient and modern Chinese may have minimal or no connection.

Teacher Zheng, an epidemiologist from the CAHEC in Qingdao, was a mentor at the first training module of the FETPV training program. Zheng responded to my question with more confidence. He argued that *yi* referred to diseases that could transmit contagiously (*chuanran*), whereas *liuxingbing* referred to both contagious and non-contagious diseases. He described a certain kind of goitre, gesturing to a growth on the neck, and said that this kind of disease is a *difangbing*, associated with a particular locality. These kinds of *difangbing* are included within the concept of *liuxingbing* but not in the concept of *yibing*.

Both Ning and Zheng reflected on the reform of veterinary administration when thinking about these conceptual differences. Ning pointed to the recently created Animal Centers for Disease Control (ACDC-*dongwu yibing yufang kongzhi zhongxin*) to help think through what was meant by *yibing* today. He later asked Zheng about why the ACDC used the term *yibing* when the China CDC (in public health) used the term *jibing* was because the veterinary sector was only concerned about contagious diseases. Zheng disagreed. He said that since the *ji* in *jibing* can mean 'fast' it was used to describe the institutions set up in the wake of rapidly spreading diseases such as SARS.

By turning to the veterinary reform, Ning and Zheng point to how epistemological changes are embedded in institutions and practices. Both epistemology and practice are further configured in narratives of understanding the contemporary predicament of the veterinarian, which reveal how the historical epistemology produces a different subjective and institutional engagement with epidemiology. Relaxing in a hotel room after one of the early sessions of the training, the television tuned to a drama depicting the 1940s civil war between the nationalists and the communists, Zheng reflected on the historical crisis of China's veterinarian.

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<sup>301</sup> Sonia Ben Ouagrahm-Gormley, "Growth of the anti-plague system during the Soviet period," *Critical Reviews in Microbiology* Vol. 32 (2006): 33-46.

<sup>302</sup> 郑红娥, 李小云, 汪力斌, 于乐荣, "对乡村社会风险管理体系及存在问题的反思——以禽流感的风险应对为例" 南京农业大学学报(社会科学版) Vol. 10, No. 4 (2010): 113-120).

Zheng depicted years after the founding of the People's Republic (1949) as a brief golden age: the government was powerful, and promoted veterinary science; at the same time, the number of animals was small. As a result, diseases were rare and when they appeared the veterinary service was more than able to control them. It was only after the 1970s, which is to say after the Cultural Revolution and at the beginning of the economic reforms, that problems started to appear. First, he said, the number of animals grew very rapidly. But second and more importantly, their circulation (*liutong*) also increased. Before the economic reforms, livestock husbandry was enclosed (*fengbi*): an animal born in one county was raised in that county and would be slaughtered, sold and consumed in that county. Often animals did not even move out of a single village. There was very little risk of contagion, indeed, there was not even the conception of contagion (*meiyou chuanran de gainian*), Zheng told me with a flourish. Today, by contrast, a pig from Helongjiang, the northernmost province, may be sold to and consumed in Guangzhou, the southernmost province. As a result the potential for disease transmission is much greater.

Zheng's account is broadly shared among other veterinarians, particularly elitely positioned ones. It is striking in the way it positions the Reform era as highly ambiguous, or at least, as complexly multiple. Narratives of the Reform era, both official and popular, both Western and Chinese, often depict a radical rupture between the Cultural Revolution, or more broadly the Mao era, and the post-Reform period. But while complicating these paeans to reform, the veterinarian's accounts do not reflect a nostalgia for the past. Rather, they situate their own vocation as one whose abilities and prominence have become grossly out of balance with the times.

Resembling the account by Cai Haifeng that I began with, Zheng placed greatest emphasis on the decreasing capacity of the veterinary administration during the post-Reform period. He identified the 1980s reform not as a moment when scientific programs in veterinary medicine expanded, but rather as the source of their decline. After the reforms, he told me, the government did not properly care for veterinarians, and in particular those working in the sector he referred to as "epidemic control" (*yibing kongzhi*). The number of graduates specializing in these fields remained level, despite vast increases in animal production and circulation. More troublingly, veterinarians began to leave the government in order to start their own farm companies, or to work as animal health experts for private enterprises. "In this way they could sell their knowledge for money," Zheng said disdainfully. Precisely as the dangers of livestock diseases increased many fold, the capacities and quality of veterinary vocation had gone into decline.

For Zheng, the solution to this predicament lay in the techniques of epidemiology such as those taught by the Field Epidemiology Training Program for Veterinarians. He himself had attended the regional FETPV held in Thailand a few years earlier. When there are outbreaks of diseases that you know little about, where there is no cure, the only method to use is epidemiology, Zheng told me, drawing an implicit contrast with laboratory based approaches. Epidemiology can determine the cause of disease, what encourages its spread, and how to intervene to stop the spread. Training veterinarians in epidemiology is particularly important because of the high percentage of emerging diseases that originate in animals but spread to humans.

"To stop such diseases there are three methods. The first is to stop the diseases by improving animal health. The second is to break [*qieduan*] the transmission from animals

to human. You can do this by preventing contact [*jiechu*] between animals and humans, by instituting isolation, or by slaughtering animals. The third way is to protect all of the human population with vaccines, but this method costs by far the most money."

The possibility for a Chinese veterinary renaissance, Zheng's account suggest, lay in positioning veterinarians as the barrier between zoonotic diseases and a vulnerable human population. What was required to build this barrier was training in epidemiology, a science that could account for the unprecedented dangerous circulation of animals by instituting new *breaks* between humans and animals. In this way, the Chinese elite veterinarians saw the FETPV as an important site for reforming the quality of the "official veterinarian." They saw this not as the way to shift from the scale of the individual animal to the population, as Pfeiffer had argued. Rather, they sought to energize a lethargic and increasingly overlooked veterinary administration by infusing it with a flexible epidemiology capable of responding to circulation. In the following section, I trace the processes through which the concept of epidemiology was exercised and adopted by the Chinese trainees.

### *Field epidemiology as a global form*

In a proposal for the first module of the training program, the FAO ECTAD office explains that the program is in fact modeled on an existing global form:

" The Field Epidemiology Training Program (FETP) is a globally recognized long-term field epidemiology program developed by the public health sector. This two-year course provides training to support outbreak investigations, analysis of surveillance data, field research and provision of practical recommendations for decision makers. Most importantly, the FETP model also promotes the concept of "training by providing services."<sup>303</sup>

The Field Epidemiology Training Program, or FETP, mentioned in this FAO document was first created by the U.S. Centers for Disease Control and Prevention as an international export version of CDC's own Epidemic Intelligence Service (EIS). Uncovering the origins of the EIS allows me to specify the epistemological and technical particularity of the "epidemiology" brought to Chinese veterinarians.

Alexander Langmuir, chief epidemiologist at the U.S. CDC after the Second World War, developed the Epidemic Intelligence Service (EIS) in 1950 to train a cohort of what he called "first responders" for epidemics.<sup>304</sup> The EIS provided two year training courses in field epidemiology for state and municipal health workers. But Langmuir also organized the EIS to construct a cohort of epidemiologists responsible to the CDC, enabling the federal institution to respond to every call for epidemic aid originating from the State governments. In the process, as I have argued elsewhere, the practices and objects of epidemiology were transformed.<sup>305</sup>

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<sup>303</sup> China Field Epidemiology Training Program for Veterinarian (FETPV) Module 1: Introduction to veterinary epidemiology. 29 November - 22 December, 2010. Unpublished document.

<sup>304</sup> The U.S. CDC, originally known as the Communicable Disease Center, was established in 1946 when a wartime organization devoted to control of malaria was converted into a permanent institution. See Elisabeth Etheridge, *Sentinel for Health: A history of the Centers for Disease Control* (Berkeley: University of California Press, 1992).

<sup>305</sup> My account is in Lyle Fearnley, "Epidemic intelligence: Langmuir and the birth of disease surveillance" *Behemoth*



Historian Dorothy Porter points out the the Second World War was a turning point in the transition from the moral program of social medicine toward a technical practice of epidemiological research.<sup>306</sup> Historians generally speak of an "epidemiological transition" in the developed world from infectious towards lifestyle or chronic diseases, such as cancer and heart disease, during the same period. Precisely at this moment, Langmuir advocated renewed attention to the threat of infectious outbreaks. As he put it himself in an oral history interview, the epidemiology that he instituted in the EIS was an epidemiology he had learned during wartime efforts to control outbreaks of disease among the troops, and it was "quite contrary to the study section research grant" form of epidemiology.<sup>307</sup>

Langmuir envisioned and justified the Epidemic Intelligence Service as a component of civil defense against biological warfare, and he argued that epidemics caused by biological sabotage or by "nature" were comparable. Langmuir suggested that public health services would be adequate to any biological attack, by "man" or "nature," provided two components were ensured: an "intelligence system" based on prompt morbidity reporting; and a cohort of "trained epidemiologists alert to all possibilities and available for call at a moment's notice."<sup>308</sup> Keeping the model of biological *sabotage* in mind, it becomes clear that Langmuir reconstructed the "epidemic" as a discrete event whose appearance and subsidence in a population could be tracked and intervened upon. In contrast to the 19th century moral epidemiologists as well as 20th century social epidemiology, Langmuir's 'epidemic intelligence' did not aim to identify the source or cause of disease in the social or environmental conditions of living.<sup>309</sup> Like the saboteur, the epidemic takes advantage of the conditions of life (waterways, food supply-chains, crowded buildings) but is always foreign to these conditions: an intrusion, not an intrinsic pathology. By epidemiology, Langmuir meant the rapid response to outbreaks and the use of diverse forms of knowledge (statistical, laboratory, investigative) to understand pathogens and their modes of transmission during actual outbreaks. Langmuir sometimes called this 'epidemic intelligence', the sleuth work of 'disease detectives', or 'shoe-leather epidemiology.' At its core was an emphasis on the "field investigation of epidemics."<sup>310</sup>

As a training program, the EIS emphasizes bringing epidemiology into the field. Along with classroom lectures, the EIS trainees are assigned for at least one year to actual positions in the CDC, in State or municipal public health offices, or at research universities. In addition, all trainees are on call for "epidemic aid duty" anywhere in the country.

The Epidemic Intelligence Service was a U.S. national program. However, by the 1970s, the CDC began to share its insights with other national governments. In 1975, Canada established a Field Epidemiology Training Program explicitly modeled after the EIS.<sup>311</sup> In 1980, the U.S. CDC first began to directly assist the extension of the Field Epidemiology Training Program (FETP), cooperating with Thailand's Ministry of Health to develop a training program

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<sup>306</sup> Dorothy Porter, "Transformations in social medicine" *The Lancet* Vol. 354 (1999): 57. See also Dorothy Porter and Roy Porter, "What was social medicine? A historiographical essay" *Journal of historical sociology* Vol. 1, No. 1 (1988): 90-106.

<sup>307</sup> The Reminiscences of Alexander Duncan Langmuir (1964), in the Oral History Collection of Columbia University.

<sup>308</sup> J Andrews and AD Langmuir, "Biological warfare defense. 2. The Epidemic Intelligence Service of the Communicable Disease Center" *American Journal of Public Health* Vol. 42: 235-238.

<sup>309</sup> On the 19th century 'social' and the concept of 'living conditions', see Rabinow, *French Modern, op cit.*

<sup>310</sup> Langmuir, "The Epidemic Intelligence Service of the Center for Disease Control" *Public Health Reports* Vol. 95: 470-477.

<sup>311</sup> J Hockin, "The Field Epidemiology Training Program at the LCDC" *Can J Infect Dis.* Vol. 2, No. 3 (1991 Autumn): 129-130.

there. As of 2011, there were 48 field epidemiology training programs in nations around the world at least partly modeled after the EIS, out of which 43 were established with direct U.S. CDC assistance.<sup>312</sup> At first, the U.S. CDC worked with national Ministries of Health to develop curriculum specifically tailored to local needs and "competencies." However, by the early 2000s, the financial and labor demands of customized programs became insupportable. A review article written by U.S. CDC staff describes how they responded to the problem by creating a standardized form of the training program:

Recognizing that, while countries do have different public health needs, there are many fundamental similarities in the skills and knowledge needed by public health workers and a core set of competencies that all field epidemiologists ideally should possess. Furthermore, meeting country responsibilities to strengthen the national surveillance and response capacities outlined in the WHO International Health Regulations not only relies on a sound public health infrastructure, but a standard set of core competencies, supporting training curricula and/or training materials."<sup>313</sup>

The development of the standard curriculum was led by an 'instructional design' team. Instructional design refers to methods for translating principles and theories of how learning takes place into concrete plans for instructional materials or activities. The final curriculum contains sixteen core competencies, such as "respond to outbreaks," "Use computers for specific applications relevant to public health practices" or "Manage a field project." Each competency is linked with instructional goals. National ministries now work from the foundation of this standard curriculum, although there is room for developing specifically tailored programs as well.<sup>314</sup> According to a review article, "CDC's vision for the future is that every low- to middle-income country has access to an FETP to help build its public health workforce and strengthen its critical public health systems [...]."<sup>315</sup>

The inclusion of veterinarians within field epidemiology training took place almost from the inception of the Epidemic Intelligence Service. The first veterinarian was accepted into the EIS in 1953, two years after the program was begun, and fifty years later a total of 195 veterinarians had been trained under the program.<sup>316</sup> However, the first FETP program designed exclusively for veterinarians was established in 2008 as an outgrowth of the highly successful Thai field epidemiology program. The Field Epidemiology Training Program for Veterinarians (FETP-V) involved collaboration between Thai Ministry of Public Health and Department of Livestock Development, and relied on expert assistance from the Regional Food and Agriculture

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<sup>312</sup> Dana Schnieder, Michelle Evering-Watley, Henry Walke, Peter B. Bloland, "Training the global public health workforce through applied epidemiology training programs: CDC's experience, 1951-2011" *Public Health Reviews* Vol. 33, No. 1: 194.

<sup>313</sup> Ibid.: 195-6.

<sup>314</sup> Denise A. Traicoff, Henry T. Walke, Donna S. Jones, Eric K. Gogstad, Rubina Imtiaz, and Mark E. White, "Replicating Success: Developing a standard FETP Curriculum" *Public Health Reports* Vol.123, Suppl 1 (2008): 28-34.

<sup>315</sup> Schneider, et al: 201.

<sup>316</sup> Marguerite Pappalanou, Paul L. Garbe, M. Kathleen Glynn, Stephen B. Thacker, "Veterinarians and public health: The Epidemic Intelligence Service of the United States Centers for Disease Control and Prevention, 1951-2002" *Journal of Veterinary Medical Education* Vol. 30, No. 4: 383-91.

Organization ECTAD office in Bangkok, rather than U.S. CDC. Funding for establishing a training centre and developing the course was provided by USAID.<sup>317</sup>

China had established an FETP module in the Ministry of Health in 2001, and it was widely seen as an important success. In 2008, the FAO began to plan for establishing a parallel FETPV training program for Chinese veterinarians. In a presentation to the Ministry of Agriculture in January 2010, Vincent Martin and Regional FAO epidemiologist David Castellan outlined the "needs, vision and goals" for field epidemiology training in China.<sup>318</sup> The presentation called the Field Epidemiology Training Program "a proven training model" and traced its history from the Epidemic Intelligence Service to the establishment of China's FETP in 2001.<sup>319</sup> In their presentation, they explain the need for an international standard program in China:

"China faces complex geographic and demographic challenges in controlling animal diseases. For this reason capacity building must be based on a proven training model, provide minimum standards and also be flexible and adaptable. China needs a comprehensive FETPV to bridge the gaps in veterinary epidemiology training in the country."<sup>320</sup>

Following this proposal, FAO and China's MOA worked together to adapt the FETPV to China's veterinarians. However, the program remained broadly based on the global model of the FETP. In particular, the use of both classroom seminars and field practicums, and the technique of "training through service" were adopted for the Chinese FETPV. The objectives of the training stated in a proposal for the first module focus on methods and concepts of field epidemiology, disease surveillance, and outbreak investigation "under a wide variety of field conditions:"

- principles and concepts of field epidemiology for disease prevention and control
- design and assessment of disease surveillance and networks using multi-level and multi-disciplinary approaches.
- planing [sic] and conduct of effective outbreak investigations under a variety of field situations<sup>321</sup>

The China FETPV--drawing on the model of "field epidemiology" developed by Langmuir's epidemic intelligence--sets up an opposition between the classroom and the field. The field,

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<sup>317</sup> FAO Regional Office for Asia and the Pacific, *Emergency Center for Transboundary Animal Disease Notes* No. 3 (September/October 2008).

<sup>318</sup> Ibid.

<sup>319</sup> The PowerPoint presentation begins with a definition of epidemiology: "scientific discipline involving... the study of the frequency and distribution... of health and disease... in populations... in order to identify risk factors (determinants)... for the prevention and control of diseases." But veterinary epidemiology, they suggest, is lacking or inadequate in the Asian region. The FETP program can be a solution to this inadequacy, they argue, because of two factors in the program's design. First, they promote the FETP training as a means of improving the scientificity of veterinary epidemiology. They point to "science based recommendations" and "science based policy decisions" as an outcome of good veterinary epidemiology. Second, they emphasize the value of applied, rather than classroom or textbook learning.

<sup>320</sup> Martin and Castellan, "Field epidemiology training."

<sup>321</sup> "China Field Epidemiology Training Program for Veterinarian (FETPV): Module 1: Introduction to Veterinary Epidemiology, 29 November - 22 December," unpublished document, n.d.

unlike the classroom, is a place where one encounters a "variety" of always particular "situations." Yet the FETPV proposes to be a "model training program" with global recognition and extension. Field epidemiology should be the same in China as anywhere else, although the "field" will always attain its circumstantial specificity in place and time. How does one implement a pedagogy of the field, rather than the classroom? The question of the particular actualization of a global (standard) form is here combined with the pedagogical problem of teaching a knowledge oriented toward the particular, rather than the general. Anthropological work on the global has focused largely on the extension of standards, just as science studies has focused on the laboratory: but how is an ethos of the particular and the variable extended into a new setting?

### *Training in statistical uncertainty*

The basic technique for accomplishing the transposition of the "orientation toward the particular" onto a new setting and into new venues is *simulation*. During my participant-observation in the FETPV training program, I found that the instructors deployed progressive syntheses or mixtures of hypothetical examples and actual field practices, the two assembled through techniques of simulation. I describe this as a movement from the virtual to the real, intending to slowly accomplish greater and greater embodiment of concepts or principles not only in the minds of trainees, but in their actual work practice. Beginning with formal classroom lectures that included wholly hypothetical examples, subsequent exercises incorporated participation of the trainees in computer game simulations, field exercises and "training through service" at their office.

The epidemiological field is not simply out there, waiting for the veterinary trainees to go out into it. Rather, the "field" is constituted as an object, or to draw on the language of Bachelard and Hacking, the field is realized or "made real."<sup>322</sup> The variety of situations in outbreaks of disease are not left in endless variability, but are increasingly defined and differentiated in statistical terms, marking the boundaries of the epidemiological "field". Byron and Mary-Jo Delvecchio Good describe the process of medical training as a phenomenological transformation of the student-physician's relationship to the "body." The body that is an object of medicine, which becomes available to the experience of a trained physician, is nothing like the body that is lived or experienced in what the Goods call "common-sense situations." Similarly, the epidemiological field is distinct from the landscapes, farms, and animals that make it up. But the significant lines of difference for the FETPV trainees was, differently from the medical students the Goods studied, not between lay and medical forms of experience. The training and embodiment of the epidemiological field is grounded in the concept of "population" and techniques of security, including above all statistical reasoning. As I argued above, this epistemology of statistical populations did not replace a veterinary medicine of individuals. Rather, the epidemiological field was gradually overlain on a domain of practice shaped by the historical sedimentation of the concepts and institutions of "plague prevention" (*fangyi*) and the correlated position of veterinarians within China's governmental bureaucracy.

The first module was devoted to the basic epidemiological concepts of rate and ratio. These are the modes of marking the difference between normal and pathological in a statistical population. Deriving from concepts developed by moral epidemiologists such as William Farr,

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<sup>322</sup> Bachelard, *The new scientific spirit* (Boston: Beacon Press, 1984); Ian Hacking, *Representing and intervening*, *op cit.*

the basic principle behind rates and ratios is comparison. In epidemiology, the comparative use of rates and ratios aims to quantitatively analyze excess and differences in morbidity and mortality between populations. The lectures continued on to discuss techniques for statistically calculating the *significance* of observed differences. Examples of these techniques include chi-square tests or the assessment of p-values. These techniques enable a distinctive form of truth statement, in which the truth of one's discourse is always encircled by the possibility of error. For example, while explaining one trainee's chi-squared result, the French instructor said:

"There is a significant difference but there is a five percent change I am making a mistake when saying this."

Following the lectures, the trainees exercised these new techniques for identifying pathology and for speaking truth. The first exercise took place in the simulated space of a game. On the second morning, the students pulled out black Lenovo laptops that had been provided as part of the training. A member of the FAO ECTAD office walked around with a USB stick to load software on each of the laptops. The game is called *Ranema*. You play the role of veterinarian. In the "virtual country" of *Ranema*, your boss, the head of the Veterinary Service, has asked you to respond to an outbreak of disease. Each level of the game is matched with the chapters of a French textbook, now translated into English as *Applied Veterinary Epidemiology and the Control of Disease in Populations*. Each level exercises a particular concept or analytic tool. The game aims to play on affect in order to embody the concepts of statistical population in the subjectivity of the game-player. As the developers of the game (among whom was one of the international trainers in Beijing) put it in a powerpoint presentation, "If you want the trainees to take the tool in its full meaning, it must captivate him, exploit his affects, be held as a movie of which he is the hero."<sup>323</sup>

The virtual world of *Ranema* was only the first platform for simulation and exercise deployed by the training program. Later in the first module, the trainees conducted "mock" outbreak investigations on farms in the rural outskirts of Beijing. We traveled by mini-bus far to the north or west of the city, suited up in white lab-coats and covered our shoes with blue plastic booties, to interview the managers of large-scale farms based on a mock outbreak questionnaire. Lastly, the "training through service" component required the trainees to plan and conduct an actual field investigation or surveillance program. The projects ranged from rabies prevalence studies to investigation of egg-drop syndrome in poultry. The "training through service" component of the China FETPV differed in one important respect from the Epidemic Intelligence Service model, because all of the Chinese trainees conducted service in their actual home unit. The training exercise thereby approached even more closely to the actual than in the EIS form. The idea was that through these mechanism of progressive actualization, the epidemiological concepts (such as the population and statistical uncertainty) could be extended to the field, but as well, they could become part of actual practice inside the veterinary administration.

The actualization within the practice of China's veterinary administration came to be seen as the largest challenge for the training program. An outcome of the training was a shared understanding, among instructors, mentors, and trainees alike, that the bureaucratic organization

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<sup>323</sup> F. Goutard, J. Thonnat, B. Toma, B. Dufour, J. Queste, N. Chansiripornchai, F. Roger, "Ranema: A computer-assisted learning tool for basic epidemiology" Presentation at: 12th AITVM International Conference, Montpellier, 21st August 2011.

of China's veterinary system remained resistant to an epistemology of population and statistical uncertainty. An incident involving the preparation of mock "study designs" points us towards the emerging lines of distinction.

Xu, a trainee from the Beijing Animal Center for Disease Control stood at the chalkboard to explain his "study design" to investigate Newcastle Disease. He explained that it was a study of prevalence (number of cases of disease at a given time divided by number of people at risk) and outlined the steps by which he would collect the appropriate data. At this point, a trainee from Chongqing asked him what the hypothesis of the study was. The instructor agreed: The study is perfect, but there is no hypothesis.

"There is no need for an hypothesis," Xu replied calmly.

"Of course you need an hypothesis," the instructor responded. "Because depending on what hypothesis you are trying to prove correct, you will need a different sample size. If you really have no idea what the prevalence is, you still have to have a hypothesis, so you will hypothesize 50%. Moreover, depending on the hypothesized prevalence, the sample size will vary, and therefore the cost of the study will vary. So when you ask the government official to fund your study, you can tell them exactly how large a sample size you need to verify your hypothesis. Otherwise you may be just wasting money."

Xu smiled. "But things don't work that way in China! You don't ask for money, you are simply given a certain amount of money to conduct a study."

Someone else added, "The leaders (*lingdao*) don't understand these scientific concepts. Telling them the "sample size" you need is just a waste of words and paper."

For many European and American scholars, science in modern China has been troubled by its relationship to politics and bureaucracy. According to this view, the promotion of science by the Chinese state, itself a pathological "scientism" rather than science, has been distorted or perverted by programs to maintain the political correctness and mass utility of science.<sup>324</sup> Although Deng's reforms began by defining scientific experts as part of the productive base rather than ideological superstructure, and called for increasing autonomy and "free play" of scientific activity from political oversight or guidance, these scholars continued to see the Chinese scientist in terms of restrictions imposed by state management or control. In doing so, they draw on Max Weber's formulation of the scientific vocation as a pursuit of truth which, under modern conditions, can never make judgments about political value or how to live. Pivotaly, however, they draw on Weber's formulation as it appears *after* its passage through Robert Merton's Parsonian synthesis, which transposed the problem of vocation onto the norms of social structure.

For Weber, the fate of modern science was tragic, because its unprecedented power to produce knowledge of nature can never reach the other bank of political value or ethical meaning. In Weber's ethical formulation, the scientist must embrace the calling or vocation (*Beruf*) much like the Protestant embraces work: the lack of assurance of salvation should lead to a redoubling of effort.<sup>325</sup> But in Merton's reformulation during the 1930s and 1940s, modern science has become vulnerable. In the first sentences of "Science and the social order" (1938), Merton remarks on the difference:

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<sup>324</sup> DWY Kwok, *Scientism in Chinese thought, 1900-1950* (New Haven: Yale University Press, 1965); Yeu-Farn Wang, *China's science and technology policy, 1949-1989* (Brookfield, Vt.: Avebury, 1993).

<sup>325</sup> Max Weber, "Science as a vocation" in Gerth and Mills, ed., *From Max Weber: Essays in sociology, op cit.*

“About the turn of the century, Max Weber observed that 'the belief in the value of scientific truth is not derived from nature but is a product of definite cultures.' We may now add: and this belief is readily transmuted into doubt or disbelief.”<sup>326</sup>

In his essays, Merton's main concern is the rise of totalitarianism, largely exemplified by Nazi Germany. As Steven Shapin has pointed out, however, Merton and his colleagues were equally concerned about the recent appearance of corporate, industrial, government, military and other novel organizational arrangements of scientific activity in the United States. Rhetorically, Merton claims to be amplifying the concerns of scientists themselves. “Incipient and actual attacks upon the integrity of science have led scientists to recognize their dependence on a particular type of social structure” (254). The task he assigns for himself is to provide clarity on precisely which forms of social structure promote belief in the value of scientific truth and, as a corollary, support valid scientific research. But in doing so, he reduces the ethical problem of vocation into four social norms that, in his view, are necessary conditions of legitimate scientific activity. For Merton, these four "institutional imperatives"-- universalism, communism, disinterestedness, and organized skepticism--comprise the "ethos of modern science" and are directly shaped by the form of social structure.

By defining the scientific ethos as a product of social structure, Merton shifts the scale of the problem from the ethicality of the scientific subject (Weber's 'vocation') to the order of society in which the scientist practices. This suggests to him the possibility of categorizing forms of social order in terms of their adequacy or value in supporting modern scientific research. Although Merton acknowledges that science has in fact existed in many forms of society (aristocratic, democratic, and so on), he argues that totalitarian societies constrict science while democratic societies promote science to the greatest measure.

Merton's framing of the problem of science and society was fateful for understandings of science in modern China. From Suttmeier, to Saich, to Wang, to Goldman, to Greenhalgh, the question of science in modern China has been unable to escape a diagnostic pathology of social structure, which examines the forms of politics, social order, and the state for evidence of distortions or obstructions to valid scientific inquiry.<sup>327</sup> As I have throughout this chapter, in the following section I invoke a renewed attention to Weber's problem of science *as a vocation* rather than as a social structure. This allows attention to how the relationship between state and science, between bureaucracy and modes of thinking, are taken up as objects of normative judgment and lived practice. And as a result, a range of disputed positions appear, revealing the Chinese configuration of science and society (as well as its interface with global health) as a problem-space, rather than a singular problem.

I should note that this revision of Merton moves in a different direction from the critiques of Merton that are dominant in the field of science and technology studies. Epitomized in Latour and Woolgar's slights, these critiques develop from the apparent fieldwork observation that norms do not operate in scientific domains, and scientists rarely if ever speak of them. More theoretically, STS scholars have argued that social order cannot be artificially separated from

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<sup>326</sup> Robert K Merton, "Science and the social order" in Robert K Merton, *Social theory and social structure* (New York: Free Press, 1968): 254.

<sup>327</sup> Richard P Suttmeier, *Research and revolution: science policy and societal change in China* (Lexington, Ma.: Lexington Books, 1974); Tony Saich, *China's science policy in the 80s* (Atlantic Highlands, NJ: Humanities Press International, 1989); Denis Fred Simon and Merle Goldman, eds., *Science and technology in post-Mao China* (Cambridge, Ma: Council on East Asian Studies, 1989); Wang, *op cit.*; Susan Greenhalgh, *op cit.*

scientific practice in order to argue that the first conditions, promotes or restricts the second.<sup>328</sup> Instead, they argue, science and society are co-produced. For example, Robert Boyle constituted and justified a certain mode of social intercourse as an intrinsic part of establishing the validity of observations in the air-pump.<sup>329</sup> From the STS perspective of co-production, the problem of the relation between the scientific ethos and political or social order becomes a comparative typology of relations between science and social order, as evidenced in Sheila Jasanoff's notion of "regulatory science."<sup>330</sup> My goal here is different. For one thing, as I mentioned previously in Chapter Two, scientists working on influenza in China constantly spoke of norms and made normative judgments, and so for this reason alone the normative dimension cannot be ignored. But normative complaints about the relation between science and bureaucracy can be phrased in a variety of ways. Rather than reducible to a singular concern about "attacks upon the integrity of science," the variations and conflicts among these judgments map out a disputed terrain of truth, value, and modes of subjectivation.

### *Disputed Vocation*

Often, the elite Chinese veterinarians, either employed by the FAO office or "mentors" recruited from CAHEC, would try to make their own translations of the concepts, or mediate across these claims of difference. Teacher Zheng, for example, on occasion became frustrated with the way the European instructors were explaining a concept, and would suddenly run to the chalkboard, writing and speaking excitedly to the trainees about ratios or p-values. He also held impromptu evening reviews of material, which sometimes included entire lectures in Chinese. In one of these lectures he addressed the problematic relations of statistical uncertainty to bureaucratic governance in China.

We had returned to the conference room after a meal in the hotel dining area. A few trainees had cups of coffee and there was a warm feeling in the room as they worked (or instant messaged friends and family) on their laptops. Zheng began his lecture with a comic story. Back in 2008, he said, China was trying to prove that there was no avian influenza in the country, because they wanted to force the United States to lift a ban on chicken imports. Chinese officials presented a study, a *beautifully looking* study, describing laboratory results that showed no positive results. An American official asked them: what was the sensitivity of the test you used? The Chinese official presenting the study simply stared straight ahead for a moment, silent, then bent over to his colleagues and muttered: what the hell is sensitivity? They explained the concept to him, this way and that way, until finally he straightened up, adjusted his tie, and said to the audience: I guess its about 95%.<sup>331</sup>

Zheng then redirected the focus of ethical concern from the leader to the veterinary trainee. Describing the techniques for conducting outbreak investigations, he asked the trainees: "When does one conduct an outbreak investigation?" He then answered his own question:

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<sup>328</sup> In his first book, Merton acknowledges that religion and science actually have reciprocal influence, but religion can be seen as causally shaping science because in Protestant society religion was an "effective social force" due to its high place within the values of the society.

<sup>329</sup> Stephen Shapin and Michael Schaffer, *Leviathan and the air-pump: Hobbes, Boyle and the experimental life* (Princeton: Princeton University Press, 1986).

<sup>330</sup> Sheila Jasanoff, *op cit*.

<sup>331</sup> Part of the humour of this story is that statistical confidence always seems to be set at 95%. There is probably a very non-statistical sense of significance associated with that number.



"When the leader (*lingdao*) says go, you just go, this is also incorrect. In order to conduct a systematic investigation, a lot of preparation work must be done before heading to the field. This doesn't mean to *oppose* the leader, but to take the leader's order and make it scientific research (*kexue yanjiu*)."

The relationship to the *lingdao*, or 'leader', of the office emerged for the trainees as a practical problem of time and attention. The question was how to fulfill the demands given by the leaders while still attending the training program. Often it meant working overtime. One young trainee from the Beijing Animal Hygiene Inspection Service was particularly harried. Since the first module was held in the office building of his *danwei*, he often attended the training program during the day and worked overtime at night. Sometimes he hardly slept. Precisely because of these challenges, the second training module was located in Qingdao, giving the Beijing based trainees a modicum of independence from the demands of their leaders.

Another incident reveals the challenges the trainees experienced in negotiating obligations to the training and to the *lingdao*. Deng is a pathogens expert at the Key Laboratory for Avian Influenza in Harbin, a city about twenty hours by fast train and several hours by plane north of Beijing. During one part of the training, he struggled to both accomplish the requests of his *lingdao* and fulfill his work in the training program. One week he left early on a Friday morning, returning to Harbin for two days, then returned to Beijing for class on Monday. The following weekend he again flew back to Harbin, before flying to the southern province of Jiangsu to collect samples from poultry, finally returning to Beijing on Tuesday. Anticipating the travel, he complained that he had submitted the proposal for the study months ago, but his *lingdao* only that day replied and demanded that the study go forward. "There's nothing I can do about it, *lingdao* are like this," Deng told an FAO staffmember and myself in a low whisper, while a lecture was in progress. "Next year there are next year's things."

As it turned out, the first module of the FETPV took place at a particularly inconvenient moment in the cycle of Chinese bureaucratic time. Held in December 2010, it happened to be just more than a month before the end of a Five-year plan. All offices throughout the Chinese bureaucracy were busy compiling the paperwork necessary for final reports to be submitted to the higher authorities, accounts of the past five years and proposals for the next. All of these reports were to be finalized and submitted by the onset of the Chinese New Year holiday in February. Research needed to be completed; the more results the better. The veterinary trainees were caught up in this flurry of paper. If they weren't working double time during the course itself, a pile of papers was waiting for them on their desk when they got back to their *danwei* in early January.

For the trainees, this was experienced as a conflict of time and attention, and addressed with resignation: "leaders are like that." There were no deep ethical concerns among them. Mostly they compared the various benefits and opportunities provided by their offices: whether or not they could 'report' expenses for meals or hotels; whether there were many 'office cars' in the parking lots or whether increased anti-corruption oversight had eliminated them; whether English lessons had been paid for. At most, as when I joined one trainee for a number of *tai ji* lessons in a park in Beijing, enthusiasm for other modes of life became the object of leisure time. When I queried this trainee about his apparent interest in ancient China and classical Chinese, he simply lamented that Chinese today cannot achieve the depth of understanding necessary for classical texts because they lack sufficient free time: everyone is too busy working at the office (*shangban*).

But instructors, organizers and elite Chinese veterinarians (such as FAO national staff or mentors) raised concerns about the *ethos* of the veterinary trainees themselves, complaining of their lack of motivation, quality or spirit.

Martin, the Coordinator of the FAO ECTAD office who had worked to bring the FETPV to China, complained about the poor quality of the trainees. We were walking from the Garden Hotel, where the third module was taking place, back to the FAO ECTAD office a few blocks away. During the training that day, we had heard a lecture from Zeng Guang, an important Chinese epidemiologist and Chinese director of the FETP (human public health) training. "Did you see how Zeng Guang asked how many Ph.D.s were in the room and so many hands were raised? Now this would lead you to believe there were a lot of experts in the room. But it is not the case! The quality is much lower than the public health FETP."

Cai, the lead "national" epidemiologist at the ECTAD office, complained on a separate occasion about how the "management system" of the veterinary administration would waste the techniques that had been cultivated by the training. He lamented that veterinarians devote excessive labor and attention to pleasing their superiors. "They don't care about society, they care about the higher-level leaders" (*shangji de lingdao*). They don't care about the common people, they only care about the higher-level," he told me.

One day, as we sat at a common lunch between classes, Cai attempted to personally persuade the veterinarians to change their engagement and attitude towards the training. Cai was one of the first generation to rigorously study veterinary science in the post-Mao period, and after many years working at the top levels of the Ministry of Agriculture, he is now a senior technical advisor to the FAO ECTAD office. Cai spoke quickly and curtly, with disgruntled animation. "Is everyone understanding the lectures?" he asked. "Are there problems with language or accent?" All of the trainees answered that it was better, it was better now, things were getting better. A few half-hearted jokes were made about the difficult accents of the trainers, two of whom spoke English with German or French inflection. Cai looked up from his noodles to meet the eyes of his audience. "This is not like other *peishun* (trainings). It must be taken very seriously," he said firmly. He noted that the FETPV is based on a "training-through-service" methodology, which means that the concepts and techniques they learn should be put to use in their everyday work. In fact, he reminded them, one of the requirements of the training will be to conduct a long-term epidemiological research investigation as part of the work at their home *danwei*.

A heavy-set woman from the national-level ACDC, based in Beijing, spoke up. "How will we be able to do these research projects when we go back to our *danwei*? We are already taking a month off, and won't we have to return to regular work when we get back?"

Cai's face tightened. The FAO already made agreements with each of the *danwei*, he told the trainees. It had all been made very clear: the *danwei* must allow them to conduct these research projects. It was very important that the training not go to waste (*bai peishun*).

Leaning forward in his chair, Cai began to relate a story about his own experience in a training program during the 1990s. At the time, he was a young veterinarian working at the Qingdao National Animal Quarantine Service (*dongwu jianyi suo*, now renamed as China Animal Health and Epidemiology Center, or CAHEC). The Ministry of Health invited veterinarians to a training program to learn techniques of epidemiological research. MOH spent

a lot of money on the training. But when Cai returned to work he was unable to put any of the things he had learned to use. Soon he forgot his new skills.<sup>332</sup>

“Now, just this month-long module of the two year FETPV program alone is costing over one hundred *wan* [RMB, around \$150,000]. Let's not let it go to waste,” Guo concluded.

The problematic ethos of the trainees, their lack of attention or commitment, was also raised in official plans for the program. But from the perspective of higher-level officials in the Veterinary Bureau, this problem should be attributed to the FETPV training program, not the trainees. The meeting minutes from the 5th Steering Committee Meeting for the program, held in January 2012 as the first cohort of trainees was nearing completion of the course, report that Dr. Sun Yan, Head of the Division of Science and Technology and International Cooperation, Veterinary Bureau, commented that the FETPV training needed to “better motivate/push the trainees to be more committed to the training.”<sup>333</sup>

### *Problems and problem-solving*

For the instructors and mentors, the negative effect of bureaucratic habits on the ability of trainees to incorporate the ethos of field epidemiology could be further specified as a problem of formalistic thinking. Field epidemiology, the trainers argued, demands the investigation of an outbreak in its particularity, and they worried that bureaucratic norms of standard practice and obedience to leadership were obstructing the flexibility of the Chinese veterinary trainees. In an effort to develop new habits of thought, wild bird specialist and “One Health” instructor Scott Newman introduced what he called “problem-based exercises” into the Third Module of the FETPV, held in July 2011. These exercises explicitly addressed not only the content of knowledge to be transmitted, but also the mode of knowing.

Newman's lecture explained the “ground rules” of problem-based learning. After dividing us into groups of five or six, he described the various “roles” to be fulfilled by the members of each group: chairperson, note taker, board writer, and computer user. Newman also outlined the dispositions and techniques appropriate for collaboratively engaging problems. As summarized in an accompanying hand-out:

“You may feel that you do not know enough to solve the problem but that is the challenge! You will have to gather information from various sources starting with each other and learn new concepts, principles, and skills as you engage in the problem-solving process. A set of ‘guiding questions’ are provided as an aid but you can answer your own as you prefer” [In English].

Each day we spent a couple of hours in the afternoon working in groups on problem-based exercises. On one of these days, the ‘problem’ provided for our exercise was about wild bird farming, and clearly originated in Newman's own experiences at Poyang Lake. The text provided for the exercise first noted that farming of wild deer is popular in America. It then stated that “some countries in Asia including India, China and Vietnam” have begun farming wild birds. These farmed wild birds have their wings clipped and are ‘imprinted’ to think of farm managers as parents.

Participation in the training and comments afterwards revealed a variety of responses to the “problem-based exercise.” Gao Lili, national veterinary epidemiologist in the ECTAD office,

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332 A similar story was told to me from the perspective of an European “donor” during a meeting of the U.N Working Group on Diseases at the Human-Animal Interface.

333 “China FETPV 5th Steering Committee Meeting: Meeting Minutes” 18 January 2012, Ministry of Agriculture Meeting Room 105.

was impressed by how much the trainees discussed and debated the problem. In the group I participated in, on the other hand, I found that a problem-based exercise could still be exercised formalistically. Rather than a "problem-solving process," this group simply transposed the parameters of the problem in the exercise into the framework of the required presentation. We set our project statement and objectives by merely restating what was already written. The "guiding questions" were copied, word for word, from the document. The solutions section required some discussion. But most of this discussion involved how to word the proposal, rather than the benefits or drawbacks of any proposed initiative. The first suggestion was to *Strengthen isolation (jiaqiang geli)*. Someone then pointed out that *Strengthen biosecurity (jiaqiang shengwu anquan)* sounded better. My group had nearly run out of time by the time we got to the solutions. As a result, Xu from Beijing went to stand behind the woman trainee from Yunnan who was writing the powerpoint. Without any pretense of discussion, he started listing off set phrases that seemed to appear in every exercise: strengthen quarantine, strengthen biosecurity, implement surveillance, he ticked off faster than the woman could type them in.

It seemed then that the "problem" was easily submerged within formalized language and the structure of the presentation. In the incident I describe below, Newman attempted to challenge the trainees on this formalistic approach to the outbreak. But a Chinese expert from CAHEC contested his account, raising questions about the logic of formalism embedded in global health itself.

We had all rejoined the larger group in the hotel ballroom in order to present the results of our group work on the Poyang Lake "problem-based exercise." Scott Newman stood at the front of the room with a microphone, while a second mic was passed around to each of the small groups. A woman from China Animal CDC reported that her group's primary solution to the outbreak was to implement culling in a 3 km "epidemic area" and declare a second 5 km diameter as a "restricted area," with controls on movement of persons and poultry. Newman asked her why she selected 3 km for the epidemic area and 5 km for the restricted area. Some people in the audience chuckled and eventually someone spoke up: There is a guideline for highly pathogenic avian influenza outbreaks from China's MOA. Newman responded with seriousness:

"Good, I am glad to know this comes from somewhere, it is good to have guidelines. I thought that when the guidelines were created they were more like 10 km. But anyway, we've come to realize that these numbers do not matter. Birds certainly travel more than 3 km when they go out during the day, and they are in contact with other birds, and possibly spreading the virus. You must base what you do on the epidemiology of the situation. Every situation might be different. These viruses evolve very fast, you know."

At this point, a woman who is a higher-level veterinarian from the CAHEC headquarters intervened with a question for Newman: "The MOA standard listed above is based on the international standard. So which expert determined this standard?"

Newman said that he forgot who had decided the standard, but he knew that it was based on experience with other diseases. Which disease he did not remember, perhaps Newcastle disease, where exclusion zones were useful.

The CAHEC expert asked: "If this is not appropriate [to institute exclusion zones], what do other countries do for HPAI?"

Newman: "I do not know what the rule is in every country, every country can set its own rules, but its the same situation. Sometimes the rule works, and sometimes it doesn't."

Newman then told a story about an outbreak of HPAI in Turkey that he had investigated. According to OIE standards, he said, after an outbreak, if there are 30 days without detecting a case, you can declare yourself "disease-free". In the Turkey outbreak investigation, we found a positive bird within the zone on the 35th day. Is this a subsequent event? To my mind it is part of the same event. We set up rules with the best knowledge we have. But the disease keeps changing and the epidemiology is specific to each situation.

Later, Cai Haifeng added: "The idea of a pollution area, these numbers, is based on the experience of Japan, which showed that more than 3 km away from a farm, the pollution is much less, more than 5 km it is practically non-existent.

The CAHEC expert insisted, "OIE still has recommendations."

Cai: "The rest of the world changed, only we still do this."

CAHEC expert: We also changed.

Scott Newman's comments place epidemiological thinking in opposition to a mode of practice based on standards, rules or norms. One mode responds to events with *specificity* and *openness to change*; the other mode applies the same rule to every situation. Newman indicates himself as one who sees rules as "guidelines" but can remain flexible to the "epidemiology" of the situation. He is critical of the Chinese trainees for adopting the standard without questioning its applicability to the particular situation.

However, the CAHEC epidemiologist's responses reveal the more complex understanding of the standard from the perspective of the Chinese veterinarian. The standards, after all, came from the international institution (in this case, OIE). The CAHEC epidemiologist was visibly frustrated that Chinese veterinarians had been blamed for implementing standards set by international norm. She suggests that formalism arrives in China with international standards, and China is not a distortion or exception from the "rest of the world." One conclusion of this chapter is that for the Chinese veterinarians, the international standard was adopted within a sensibility of *backwardness*. On the one hand, China was behind the "international" world and the standards or practices employed in the international setting therefore represented both the Other and the future. On the other, the veterinarian--and the veterinary administration as one sector of the government--was *backward* in relation to other sectors, such as human public health. This backwardness was measurable in terms of the level to which they accorded with the international standard. In this way, the international standard or practice (such as "field epidemiology") came to hold a value above and beyond its technical utility: a value for validating the veterinarian within the Chinese contemporary. As a result, global forms could be adopted as mere formalism. The CAHEC epidemiologist's question, then, was whether this formalism might be inevitable when the "global" or "international standard" is attached to a developmental logic. The problem was not Chinese bureaucracy, but rather the overvaluation of the international and the global as a source for truth standards.

I began this chapter with the concerns of Cai Haifeng and I end with his worries expressed as the training was well under way. He had come to fear that the veterinary trainees were not able to grasp the variable, situated nature of the epidemiological field, precisely because of the veterinary reform. Their practice was hindered, not benefited, by the professionalization

and internationalization of the "official veterinarian." This was a threat to the viability of veterinary science that he knew well, as he found it in the biography of his own life.

Cai came from a poor background in Shandong province. His college degree was paid for by the government on condition that he would work in government service after graduation. His first job in the late 1980s was at the animal quarantine service in Qingdao, Shandong province, the main national institution devoted to control of epidemic disease in animals. Within less than a decade, however, Cai found himself working amidst an unprecedented livestock revolution and its pathologies. According to Guo, it was not until the late 1990s, "very late", that the large-scale rouji (broiler) enterprises took off, with many farms established in Shandong.<sup>334</sup>

In the past, Cai told me, Chinese farmers had only ever raised chickens in a "backyard" manner: small flocks raised in family courtyards and considered a part of the household unit. All of a sudden, there were farms raising one hundred thousand, five hundred thousand, even a million birds. The rapid increase in poultry numbers and density caused many disease outbreaks: Newcastle disease, parasites, and "diseases you don't have in Europe and America." Some of the farms experienced death rates from disease as high as ten percent.

But in Cai's opinion, the increase in livestock, in living beings and their ecology of pathogens, was not in itself productive of diseases. Rather, diseases appeared because of a failure of management (guanli) and biological security (shengwu anquan). The model of large-scale farms was imported from Europe and America, but Chinese farmers had no experience in husbandry at this scale. "They didn't know how to raise chickens on a large-scale, nobody did, I didn't know how."

Despite not knowing, Cai was often called on to control disease outbreaks on the large-scale poultry farms. The moment remains inside him today, indeed the visceral experience of fowl bodies and their diseases produced a longstanding repulsion. To this day, Cai will not eat chicken. But despite this, his reflections on the moment are surprisingly fond, because it was in that era that Cai's knowledge could take shape in close adjacency to the practices of production. Cai was able to conduct full investigations on his own, from the survey of farm conditions to the laboratory diagnosis of disease, sharing experience with farmers and even developing recommendations for control interventions and how to prevent future outbreaks.

Each investigation was distinctive. Cai did not have a standard protocol but rather cultivated a nose for the unexpected, searching out the trail of contagion. One day, he told me, he had struck upon a perplexing outbreak of a contagious disease. When he arrived to inspect the farm, everything looked clean, bright and orderly. Appropriate segregation and isolation measures had been instituted throughout the farm. Farmers lived in quarters separated by a good distance from the birds; the chicken sheds were carefully walled off from each other and access restricted; and yet, still disease kept arriving at the farm. As Cai was strolling around the farm, looking for any sign of unreported mixture or contact, a truck arrived at the gate to the farm. A few workers jumped out, entered one of the chicken sheds, and as the truck backed close to the shed, they reemerged carrying shovel fulls of manure, tossing the contents onto the back of the

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<sup>334</sup> A report by economists from the China Center of Rural Economy and published by FAO presents agricultural census data showing an enormous shift in overall poultry production from small to large-scale farms occurring between 1996 and 2005. Although the first intensive operations were established in the 1980s, in 1996 only 0.3 percent of poultry operations raised 1000 or more birds. Those truly large-scale farms, raising 10,000 or more birds, already accounted for 25 per cent of total poultry production. By 2005, this share had nearly doubled to 49 per cent. Ke Bingsheng.

truck. With a few questions, Cai discovered that the truck traveled from farm to farm throughout the region collecting manure. It could easily carry pathogens along with the rich cargo from one farm to the next, as if a mechanized God of Plague.

During this period, Cai skirted the lines between positions as an employee of the quarantine bureau and as a private individual providing veterinary services to the farmers. He told me that the large rouji companies, in particular, were suspicious of the government, fearful of quarantines or other disruptions to their commerce, so they “never told the government anything.” I found surprising that they spoke to Cai openly, although he was a government worker. Cai explained to me that he had made clear to the industrial farms that he would not report what he saw to the quarantine authority. At first, the farmers did not believe him, but over time they came to trust his word. They paid him for his work, and he earned much more from these investigations than from his regular salary. When Cai spoke with me, he emphasized that this situation was far from ideal. And yet, the benefits he saw in this work were not only personal monetary compensation. There were also benefits for China, and perhaps, even for truth itself. By developing trust with the owners and employees of the company farms, by breaking his own duty as an officer of the quarantine bureau by refusing to report the disease outbreaks, he was able to produce knowledge that otherwise would not have existed.

Immersion in the practices of poultry production, nearness to the farm with its shit and feathers, Cai believed was essential to the search for scientific truth. When in later years, just after the turn of the millennium, Cai advanced to more formally prestigious positions within the Ministry of Agriculture, he was brought farther and farther from the farm, and he sorely felt the pathos of this advance. His life experience, narrated as an increasing separation of knowledge from its object, embodied Cai's diagnosis of the problems afflicting contemporary China and veterinary practice. He noted skeptically the recent increase of “professors”, and lamented that students today favored laboratory and specialized topics such as molecular biology. Only in such specialized sciences could you “write a good thesis,” he told me, and yet these specialized knowledges were of no use in the practice of livestock disease control. He argued that China's official veterinarians could be divided into two kinds: those trained in specialized sciences which have limited practical applicability; and those who lack knowledge altogether.

The expansion of veterinary knowledge through international-led trainings threatened to have a paradoxical effect: by constructing subjects of increasingly specialized and elite forms of scientific knowledge, the possibility of sharing a social world or way of life with poultry breeders became more difficult. As communication with the international scientific community intensified, through journal publications and outbreak reporting, the lines of communication developed by Cai between government science and poultry production were rendered morally problematic: even, corrupt. Cai reframes the problem of the contemporary Chinese veterinarian in its most provocative form: is the office veterinarian, the subject of international scientific knowledge and the moral bulwark against corruption, incapable of being the subject of truth about livestock diseases in China?

## Chapter Six

### The Duck Doctor

In the veterinary circles of Beijing and Qingdao, and among the elite veterinarians such as those participating in the FETPV training, talk frequently returns to the topic of veterinary reform (*shouyi gaige*). Whether hopeful (like Teacher Zheng) or pessimistic (like Cai Haifeng) about the future of China's veterinarian, agreement is widespread about its direction: towards the international standard, highly specialized, expert scientific, normatively professional, "official veterinarian." In this way, the veterinarian would not only "join the international" (*yu guoji xiangtong*), as one text on the reform puts it, but would also join the more advanced sectors of China, fulfilling the idealized transition from planned to market economy, and towards scientific modernity. Ning once told me that his former university had even recently eliminated the veterinary (*shouyixue*) degree, replacing it with the more modern sounding "animal medicine" (*dongwu yixue*) in order to keep up with the changing times.<sup>335</sup>

Out in the Poyang Lake region, however, I found that the government-employed "official veterinarian" is a minority among the duck doctors. The Xiaolan Wholesale Poultry Egg Market, just south of Jiangxi's provincial capital, Nanchang, in the heart of the Poyang Lake duck breeding region, is one of the largest wholesale markets specializing in poultry eggs in the entire country. The market deals exclusively in duck and quail eggs, the two kinds of eggs that are preserved as *pidan* (translated in English as "century egg", they are black from a process of preservation). On my first visit to the Xiaolan market, I was surprised to find a massive red banner hung on its outer wall, a single character emblazoned in stark white: 药 (*yao*, medicine). On closer inspection, I found that the banner was put up as an advertisement for a small shop below that sold livestock medicines to the poultry breeders who brought eggs to market. Inside the covered marketplace, the outer walls were lined with ten or so small livestock medicine shops. More than twenty cluster on nearby roadways. The shelves inside are filled with brightly colored boxes and packets of medicine, produced by factories in Shanghai, Chengdu or Guangzhou. These shopkeepers are much more than pillbox salesmen. They earn their living diagnosing diseases, prescribing medicines, providing advice to breeders--by curing ducks.

Just outside a small shop on the outer wall of the Xiaolan market stands a small concrete block, rising to just below the waist of an average man. On that first visit, I stopped to watch as a breeder walked up the two concrete steps to this duck doctor's shop, a ten-gallon black plastic bag slung over one shoulder. Greeting the doctor, she dumped her bag, five or six dead ducks, grey and damp, feathers matted, falling to the ground. The doctor sprang to work. Moving quickly and with certainty, displaying a focused confidence in the movement of his limbs, he flung a duck carcass on the concrete block. He reached for a sharp blade and made a long

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<sup>335</sup> The Chinese term for veterinarian (*shouyi*) is a compound of *shou* (beast) and *yi* (medicine, doctor). Ning's rationale for the change of name was that *shou* holds a number of bad connotations in contemporary Chinese where it can colloquially be used to refer to someone as "beastly". Indeed, some newspaper articles I have read pun on '*shouyi*' to discuss things like medical malpractice, i.e. "beastly doctor". It is also worth noting, though, that *shou* and *shouyi* are old Chinese terms, while *dongwu* (lit. 'thing that moves', i.e. *animal*) is a European loan-word adopted in the modern period along with the craze for Darwinian evolution. *Shou* (beast) is conceptually opposed to *ren* (human), whereas *dongwu* is technically inclusive of *renlei* (humankind, human species).



incision from mouth to anus, then pulled open the body cavity. He thrust in his bare hands and sought the entrails. A string of coiled intestines in his grasp, he again drew the knife, slowly this time, incising along the length of the digestive tract. He peered intently at the contents exposed, rubbed them in his hands, and looked again. After a minute of inspection, the doctor raised his head and tossed the duck off the pillar to the ground below. He grabbed a second duck and went through the same steps, but more quickly, verifying his initial apprehension. After a third duck was tossed to the growing pile on the far side of the block, the doctor straightened up, rinsed his hands with a pail drawn from a bucket of water, and gestured for the breeder to join him in his shop. Once inside, he decanted a dark brown-red fluid from a large glass container into a small plastic bottle. With a few words of explanation, he handed the bottle to the breeder, who gave him a few paper bills in exchange.

Many months later, I asked ECTAD national technical director Cai Haifeng about these 'duck doctors'.<sup>336</sup> Doesn't the duck doctor represent the antithesis to the "official veterinarian"? I asked him, describing to him diagnostic scenes like the one above. Might the duck doctor suggest an alternative to the problem of overprofessionalization and social distance from the farm that Cai diagnoses in today's "official veterinarians"? Cai grinned and shook his head. These vets who "sell medicines and feeds," he replied, are nothing more than cheats (*pianzi*), deceitful quacks and mountebanks. "They will say anything in order to futher their own interests."

In practice, the veterinary reform extended only to government-employed veterinarians, and training programs such as the FETPV were even more restricted, only permitting a few elite province and national-level vets to participate. But the discourse of veterinary reform extended much farther. The veterinary reform is needed, many elite and official veterinarians told me, in order to weed out charlatans from the upstanding through professional standards and licensing. These licensing programs had not been rolled out yet, and as far as I could tell, the veterinary reform had no practical impact on the duck doctors. But much like rural residents more broadly, the duck doctor could be said to exist in the "shadow" of modernization programs like the veterinary reform.<sup>337</sup> This was particularly true when the discursive language turned toward the metric of 'quality' (*suzhi*). While the veterinary reform called for the improvement of "all-around quality" (*zonghe suzhi*), the "grassroots" vet stood as a sign of the poor quality (*suzhi cha*) that marked the veterinary profession as backward. The cultivation of moral quality and scientific ability in the elite "official veterinarians" is paired with the denunciation of the "grassroots" veterinarian, who covers over lack of ability with pleasing words in the pursuit of self-interest.

During my time accompanying duck doctors at the Xiaolan market and in the Poyang Lake region, I was soon forced to confront the question of fraud on my own terms. After a few months outside of Jiangxi, I returned to the Xiaolan market in order to visit Hao Weidong, a duck doctor I had met in the summer. Hao's shop was little more than a hastily constructed wooden shed squeezed between two of the large cages where egg buyers received and stored their wares. Shelves packed full with brightly colored packets of medicine, gold and orange and

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<sup>336</sup> There was no consistent appellation used in referring to these shopkeeper/veterinarians. Most farmers referred to them simply as 'boss' (*laoban*), the typical term of address for any shopkeeper; some used the term 'master' (as in master-tradesman, *shifu*), the typical term of addressed for skilled tradesman (taxi drivers, carpenters, etc.). Some Chinese referred to these kind of vets as "grassroots veterinarian" (*caogen shouyi*). Of course, these shopkeeper/veterinarians treat birds other than ducks (geese, quails, chickens) and very, very occasionally other animals. But I wanted a term that captured the specificity of the veterinary work in this region, which is dominated by duck farming. I did once read a newspaper article describing a *ya yisheng* (duck doctor).

<sup>337</sup> Xin Liu, *In one's own shadow*, *op cit*.

blue, lined the three inner walls. Hao proudly pointed out to me the plentiful medicines filling the shop. A strong metal gate blocked off the front of the shop when Hao was not there. A simple sign above the gate read: "Duck and quail medicines."

When I entered his shop, Hao was carefully looking over the medicine packets on the shelves. Seeing me at the front of the shop, his face brightened. He pulled out a long, low wooden bench and gestured for me to sit. After exchanging a few brief greetings, he told me he had recently had a bit of success. He had developed a cure for *qinliugan* (avian influenza). "With my medicine, *qinliugan* isn't really a disease (*bing*) anymore," he told me. "All the ducks immediately get better." He explained that his prescription involved a combination of *xiyao* ("Western" medicines--antibiotics, antivirals, and the like made into pills and bottled in factories) and *zhongcaoyao* (Chinese herbal medicines). He showed me a bag filled with dried leaves that gave off a pungent odor, sweet and bitter. He soaks these leaves in water, letting the medicine seep out from the leaves, and gives this solution to the birds to drink. It is very effective: they return to health in three days or less, Hao claimed.

I was surprised to hear Hao's claim to cure avian flu. As far as I knew, the only medicines that could be said to "cure" avian influenza are antivirals such as oseltamavir (Tamiflu), branded drugs which are tightly controlled and licensed only for human use. I could feel my heart filling with doubts and questions. Was Hao a fraud? Should I trust his statements?

My doubts were actually twofold: on the one hand, I had doubts regarding the truth-practice of Hao the duck doctor; on the other, I had doubts regarding my own truth-practice as anthropologist. The spectre of the deceitful informant haunts anthropology and the fieldwork method, a ghost of doubt well captured by E.E. Evans-Pritchard's duel with the Azande witchdoctors. Evans-Pritchard notes that the esoteric nature of the witch-doctors posed difficulties for the ethnographer because the secrets of their trade hold value and power precisely in so far as they are secret. Evans-Pritchard's solution to the dilemma is to match the witch-doctor's tricks with his own: first, by having his personal Zande servant Kamanga train as a novice witch-doctor; and second, by exploiting competition among witch-doctors to extract secrets, reveal contradictions, and expose lies. The climax of Evans-Pritchard's tricks comes when a servant in his own household falls sick, and he calls on one of the witch-doctors to assist Kamanga in conducting a surgical cure, the removal of the "object of witchcraft" from the body of the sick. I quote the key passage at length:

I told Bogwozu that if his pupil were able to perform the operation successfully I would gladly give him his ten spears and let him return home on the following morning. Bogwozu prepared a poultice of *kpyo* bark, and while Kamanga was making an incision on the sick boy's abdomen, he inserted a small piece of charcoal into it. I was sitting between Bogwozu and Kamanga. When the teacher handed over the poultice to his pupil I took it from him to pass it to Kamanga, but in doing so I felt for the object which it contained and removed it between my finger and thumb while pretending to make a casual examination of the kind of stuff a poultice consisted of and commenting on the material. I am not certain whether Bogwozu saw what I had done, but I think that he suspected my motive in handling the poultice, for he certainly looked suspicious. It was a disagreeable surprise for Kamanga when, after massaging his patient's abdomen through the poultice, and then removing the poultice, he could not find any object of witchcraft in it.<sup>338</sup>

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<sup>338</sup> E.E. Evans-Pritchard, *Witchcraft, Oracles and Magic among the Azande*. (Oxford: Clarendon Books, 1976).

The wily anthropologist eventually brings Bogwozu and Kamanga to another room, confronts them, and gets Bogwozu to "confess" the artifice.

Evans-Pritchard thought that by exposing the trick of the witch-doctors he had "triumphantly" established the real truth. Although the exposure of the witch-doctor's fraud makes for dramatic prose, and, according to Evans-Pritchard, its effect on the novice Kamanga's "sublime faith" is "devastating", the chapter continues just a few pages later to acknowledge that skepticism of witch-doctors is actually widespread among the Azande.

"It is important to note that skepticism about witch-doctors is not socially repressed. Absence of formal and coercive doctrines permit Azande to state that many, even most, witchdoctors are frauds. No opposition being offered to such statements they leave the main belief in the prophetic and therapeutic powers of the witch doctor unimpaired. Indeed, skepticism is included in the pattern of belief in witch-doctors. Faith and skepticism are alike traditional. Skepticism explains failures of witch-doctors, and being directed towards particular witch-doctors even tends to support faith in others."<sup>339</sup>

Evans-Pritchard's ultimate solution to the problem of deceit is a dynamic notion of tradition, in which a tradition's vitality lies precisely in the doubts that it encourages. At the same time, this solution relies on placing belief within a functional whole of culture, language and social order. "Azande do not consider what their world would be like without witch-doctors any more than we consider what it would be like without physicians," he writes, and adds, "All their beliefs hang together" (109).<sup>340</sup>

Another approach to the potential of the deceitful informant, that proposed by Jeanne Favret-Saada, is to challenge the idea of the "informant"--transmitter of "information"--as a legitimate fieldwork relation. Words do not simply carry information or representations--knowledge and truth are not only relationships of communication. Words are exchanged, disputed, combated: words *do* and *contest* things.<sup>341</sup> I am not equating the diagnostic encounters I observed with witchcraft practices in the French Bocage, where "words wage war." However, discourse of all kinds, not only a violent discourse like witchcraft, produces social and material effects. Discourse is efficacious not only in the everyday performative sense, but also in the particular pathways through which words shape social orders, categories and ways of being.<sup>342</sup> Of particular importance to the question at hand, critiques, accusations and denunciations are more than simple descriptions of the social world; they actively intervene and refigure the moral orders and hierarchical positions of the social world.<sup>343</sup>

Favret-Saada argues that both public and most anthropological discourse about witchcraft beliefs presents the rural peasantry as credulous, blinded by superstitions and fooled by mountebanks. The peasant, surely, lies deepest in the shadow of modernization. Backwardness or ignorance approaches a moral category: it is only a short step from being vulnerable to fraud

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<sup>339</sup> Ibid.: 107.

<sup>340</sup> Evans-Pritchard's tricks should also be compared with

<sup>341</sup> Jeanne Favret-Saada, *Deadly words: witchcraft in the Bocage* (New York: Cambridge University Press, 1980).

<sup>342</sup> Ian Hacking, "Making up people" in Thomas C Heller, ed., *Reconstructing individualism: autonomy, individuality and the self in Western thought*; Hacking, *The social construction of what?* (Cambridge, Ma.: Harvard University Press, 1999); Michel Foucault, *Fearless Speech* (Los Angeles, Ca: Semiotext(e), 2001).

<sup>343</sup> Luc Boltanski and Laurent Thevenot, *On justification: economies of worth* (Princeton: Princeton University Press, 2006); Chateauraynaud and Tornay, *Les Sombres precurseurs: une sociologie pragmatique de l'alerte et du risque* (Paris : Éd. de l'École des hautes études en sciences sociales, 1999).

to being responsible for its persistence. The comparison with the duck doctor is clear. Figuring the duck doctor as charlatan also relies on a presumption that rural Chinese residents are naive and vulnerable to tricks. However, after living with poultry breeders and observing them at the Xiaolan market, I found that they were anything but simple fools. Over the course of the many diagnostic encounters I observed at the Xiaolan market, at Hao's or other shops, I found they often disputed the diagnosis of their birds or the efficacy of a prescription. They even themselves accused the duck doctor of being a cheat (*pianzi*).

However dynamic Evans-Pritchard's notion of skeptical tradition may be, I argue that the truth-practice of the duck doctor's diagnosis cannot be explained through recourse to beliefs that "hang together". All agreed that it was the *novelty* of the situation that was significant, rather than its traditional character. Intensive and large-scale duck farming; many of the diseases afflicting the birds; and even the duck doctors themselves are all phenomena that poultry breeders and duck doctors alike claimed were at most a few decades old. For poultry breeders, the recent appearance of the diseases was precisely what made them seek out specialists, since their own experience (*jingyan*) was not adequate to cure them.<sup>344</sup> Rather than the norms of belief among farmers, in this chapter I turn to the disputed diagnosis itself. I ask how disputes over pharmaceutical exchanges are contested and resolved. I examine how critiques and denunciations (as well as trust and affirmation) operate in actual diagnostic encounters, rather than in broad discursive formations. I then conclude by suggesting how this anthropology of diagnostic dispute reframes our understanding of the broader critical discourse on veterinary medicine in contemporary China, and in particular its attention to the problematic of the fraud. The fraud, I argue, makes the external mold of the true understood as ethical rather than epistemological.

### *Disputed Diagnosis*

"Six of my quails died last night. You're stupid prescription was not effective!" the old man yelled over the phone at Hao Weidong, the duck doctor I met in the Xiaolan market. I was sitting on the low wooden bench as Hao paced back and forth in his small shop.

"What do you mean not effective?" Hao shouted back, grimacing into his cellphone. "I explained this to you that day I went to your house to look at the quails. Some of your quails were already weak, they weren't eating or drinking water. I told you that the Chinese herbs (*zhongcaoyao*) will cause birds that were going to die anyway to die more quickly. You say only six birds died last night?" Hao concluded. "Well then! That means the outbreak is already stabilized!"

With these few words, a dispute was broached. The two narratives were equal and opposed. One or the other must give way. By examining how the dispute was played out and resolved, we gain insight into the practices of justification that ground the duck doctor's diagnostic truth. What is in question is not the empirical fact that six birds died, but the judgment about *responsibility* for the deaths. Did Hao's prescription work or not? Who or what was responsible for the deaths?

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<sup>344</sup> On the contested nature of traditional ideals amidst changing social worlds, and therefore on how anthropology should approach a "culture," see Paul Rabinow, *Symbolic Domination: cultural form and historical change in Morocco* (Chicago: University of Chicago Press, 1975); and Rabinow, *Reflections on fieldwork in Morocco* (Berkeley: University of California Press, 1977). See also Joseph Levenson, *Confucian China and its modern fate*, 3 vols. (New York: Routledge and Kegan Paul, 1958-1965).

In order to understand the dispute, it is necessary to return to the first moments of the diagnostic encounter that I observed a week earlier.

When the Xiaolan market opens around six in the morning, the buyers of duck and quail eggs who lease shop-space begin to set up their scales. Soon after, farmers pull into the market place in large trucks. In most cases, four or five farmers join together to engage a driver and a truck to bring their eggs to market. The farmers sit with the driver up front in the cab, with wooden boxes of eggs piled high on the bed of the truck. The trucks slowly lurch to a stop, unloading their passengers near the center of the market. At this central square, farmers and buyers meet in small agonistic clusters to haggle the daily price of eggs. Beneath a high plastic and metal roof, open to the air at the far end, the egg exchange is lit with a raking horizontal light. The most powerful buyers are the owners or representatives of the preserved egg factories. They arrive late in the morning in foreign luxury cars, shiny black against the dust of the market floor. These buyers agree on prices with farmers and send them with their trucks directly to the factories.

In the morning, Hao opens the shop by pulling up the metal grate, then moving a wooden desk to just out front. The desk, decorated with a few boxes of medicine, and lined on both sides with boxes of product, forms a simple counter. Hao stands on one side and greets farmers as they pass the shop, offering cigarettes and asking about their birds. He hopes to invite them into the shop, asking them to sit on one of the long wooden benches, to talk about ducks and quails and medicines.

On any given day, the two wooden benches in the shop hold a revolving set of bodies: Hao's assistant, a woman whose family has connections to the management of the marketplace; farmers passing by or old customers, as well as their companions; representatives from the pharmaceutical factory; and now, myself. That morning, Hao gave me a bottle of water and told me to make myself comfortable while he stepped out to run a brief errand. I watched the trucks pull in and out of the market, and watched the duck breeders unloading boxes of eggs.

The local people of the Poyang Lake region have been raising layer ducks for centuries as a 'sideline enterprise' (*fuye*). Following the Reforms of the late 1970s, however, the scale of duck husbandry significantly increased. The most important factor in this increased scale is not the rise of market economies, but more specifically the growth of the livestock feed industry in the late 1980s. Previously, ducks had been primarily farmed in a circuit involving the cultivation of rice: the ducks work as a kind 'biological pest control' among the rice plants by feeding on weeds and pests, and they glean rice grains left behind after harvest. With the introduction of commercial feeds, the scale of husbandry could increase beyond the limitations of "the rice economies." As a result, for some breeders duck husbandry became a primary, rather than sideline, enterprise.

In the Poyang Lake region, a highly particular economic structure has consolidated over time around the breeding of 'layer-ducks' (*danya*) for their eggs. The Xiaolan market plays an important role in organizing this economy. Originally a rural goods market that had gradually become specialized in poultry eggs, in 1998 the Nanchang government established an official "poultry egg wholesale market" to link layer-duck farmers with the owners of the 'preserved egg' (*pidanchang*) factories. Duck and quail farmers lease trucks to bring their eggs to the market every two or three days, where they negotiate prices with owners or representatives of the *pidanchang*. The poultry breeders are paid almost entirely in credit, which can be exchanged for large bags of feed grain in another section of the market. Some farmers and observers suggested that the system was highly exploitative due to the "monopoly" of the *pidan chang*, and pointed to

the large number of farmers who went bankrupt. A published news article acknowledges problems of 'market domination' in the early years of its establishment, but claims they have since been resolved when the market was restructured as a "dragon-head corporation" (*longtou qiye*).<sup>345</sup> Whatever the case, farmers make very little money in the exchange of eggs for feed, at best breaking even. At the end of one or two years, when laying production has begun to seriously decline, the ducks are sold for meat. But they are not sold as ordinary duck meat. Instead, they are sold as "old hens" (*laomuya*--sometimes translated into English as "spent hens"), fetching prices double or more than regular meat ducks. The sale of old hens is thus a windfall. Farmers viewed layer-duck farming as a kind of investment--the best way to create a large sum of *cash*, difficult to accumulate from standard crop cultivation, yet necessary for life-events like house construction, weddings, funerals, or the like. In turn, this windfall was only possible because of the peculiar economy of duck eggs constituted by the Xiaolan market, despite its exploitative potential.

Only a few trucks remained in the market by the time Hao returned. He carried a large black plastic bag with him and put it in the back end of the shop. At the back of the shop was an open doorway leading to a small back storage area. Hao ducked into the darkness and returned with a kilogram scale. Untying the large plastic bag, he pulled out six small pink plastic bags which exhaled a bitter-sweet smell. Roots and grasses and herbs--Chinese medicines, or *zhongcaoyao*. Looking at a prescription scrawled on a small piece of paper, he asked for his calculator and made a few calculations. He began to untie one small bag after another, pouring a percentage of their contents--dark gnarled roots, dried and twisted--on to the kg scale. He was preparing medicine, he told me. Satisfied with the weights, he then formed two identical piles on simple cardboard sheets. He carefully poured these pharmaceutical mixtures in two new plastic bags. Finally, he pulled some white bottles of factory-made medicine off the shelves and, I noted with surprise, placed them in only one of the plastic bags, but not the other.

"A farmer had eight quails die in only two days," he explained. Just then, one of his mobile phones rang. I heard a woman's voice on the other end. "Hello, little sister," Hao calls her familiarly. "Yes, I am preparing some *zhongcaoyao*, along with some anti-virals (*kang bingdu*) and *wei C* (vitamin C)." I expected this woman was the farmer with the eight quails, but soon found that the relationship of diagnosis and exchange was more complex.

Hao and I set out with the two plastic bags of prepared medicines, his prescription given substance and weight. At a busy thoroughway Hao hailed the public bus. We rode for some time, passing through the urban center of the county seat, then out into open country. The bus came to a stop, the terminal station, in an old town that straddled a river, the buildings rebuilt in past years with concrete and white tile. Hao waved to a young woman on a motorbike across the street.

Leaving one of the black plastic bags with me, he walked toward the woman, and they greeted each other with bright smiles. After a short conversation, Hao handed her the bag of medicine. When she tried to hand him some cash, however, he vigorously refused.

"Why didn't you accept her money?" I asked when Hao returned to where I was standing.

"She introduced me to her uncle!" Hao said with a laugh, "I earn his money this time."

We walked across a bridge over the river to a small restaurant where the uncle, Old Wu, was waiting. Wu wore a lightweight blue work apron common to many of the rural peasants in Jiangxi. Inside the restaurant, Hao unpacked the medicines in the bag, explaining how to feed

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<sup>345</sup> 张绪鸿, 全国禽蛋加工企业南昌三分天下全国禽蛋市场价格小蓝有话语权; 小蓝: 小禽蛋“孕育”大市场”江西新闻网 2010-08-25.

them to the birds. There are three packs of *wei C*, use over two days, or you could stretch it and use one per day. Suddenly, Wu pointed to the *zhongcaoyao*.

“These should all be very cheap.” he stated bluntly, suggesting that Hao's prices were too high. He explained that until retiring in recent years he had worked at the local Chinese medicine clinic, and knew all of the herbal medicines and their value.

Hao, surprised, excused his prices. “Its the Western medicine that's expensive,” he said, pointing to the white bottles.

After some deliberations, the two men decided that Hao would go to Wu's home to *kan anchun*, of "look at the quails," despite the fact that Hao had already prepared a prescription based on the symptoms reported to him over the phone. We rode in a three-wheel motor-powered cart out into the open countryside, which was dotted here and there with duck sheds and fish ponds, crossed by train tracks and power lines over head. In the river, a number of barges were mining sand, large cranes carving out the the riverbed and building tall white mountains on the shore. Arriving in a densely built village, where a second-story had been added to nearly every home in recent years, Uncle Wu led us into his courtyard. We walked into the house, then went up a small flight of stairs. Hao commented on how clean Wu kept his quail cages, which usually produce a very strong smell.

Inside the upstairs room, the walls were lined with tall wooden cages and each cage filled with the small squawking birds. Hao looked around, scanning the cages, and declared that the birds seem to lack spirit (*jingshen*). Wondering aloud about the temperature, he checked a thermometer on the wall. It read 28 degrees. Too hot. Perhaps this was the cause of their illness. After a few more words of advice, Hao protested that we needed to return to the shop, and in a few moments we were back in the three-wheel and headed to Xiaolan.

### *Closing the dispute*

A few days later, Uncle Wu called Hao to dispute the efficacy of the prescription. As I described above, he complained to Hao that after giving the medicine to his birds, six more quails had died, proving the prescription was ineffective. Hao countered that the dead birds must have already been casualties of disease before treatment had begun, and if anything, his prescription had prevented the number of deaths from becoming far greater. By examining the modes of judgment and justification employed to resolve the dispute, I argue we gain insight into the mode of truth-telling that grounds the duck doctor's diagnosis.<sup>346</sup>

Observation of the birds was the first key metric of diagnostic truth. Uncle Wu's observation that six birds had died was enough to support his initial accusation that the prescription was not effective. Poultry breeders in the region closely observe the appearance and behaviors of their birds in order to identify health and disease. When birds do not eat, for example, it is seen as a sign of disease and cause for seeking out the doctor or treatments. Drawing on an important term in Maoist Chinese medical and scientific practice, they told me that the ability to identify disease in birds lay in 'experience' (*jingyan*).

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<sup>346</sup> On modes of truth-telling or "veridiction", see Michel Foucault, *Fearless speech, op cit*; Foucault, *The courage of truth (The government of self and others II): lectures at the College de France, 1983-1984* (New York: Palgrave Macmillan, 2011); Paul Rabinow and Gaymon Bennet, *Designing human practices: an experiment with synthetic biology*; Steven Shapin, *A social history of truth: civility and science in seventeenth-century England* (Chicago: University of Chicago Press, 1994).

Judith Farquhar begins her book on "traditional Chinese medicine" practitioners with a puzzlement. When she found what appeared to her as factual contradictions in textbooks, she asked Chinese physicians about them. "How do the doctors know which explanation is correct?" she asked. They answered either with *jingyan* (experience) or *shijian* (practice). "We take experience to be our guide." Farquhar recounts how her initial dissatisfaction with this response broadened into understanding. She came to realize that they were not simply being evasive or secretive, but rather articulating a philosophy of how medicine should be practiced. Experience (*jingyan*) was not reducible to philosophical "empiricism" (in Chinese, *jingyan zhuyi*). Rather, it articulated a distinctive approach to knowing as a "personal and historical experience formed in scholarly, didactic, and therapeutic practice." Rather than grounding knowledge in the accurate representation of the world, or the proper accordance of mind and nature, Chinese medical practitioners drew on *jingyan* to develop situated and individualized diagnoses in conjunction with the patient.<sup>347</sup> Farmers spoke of *jingyan* as a mode of knowledge based in long-term engagement in the practice of breeding. However, they distinguished between their ability to identify illness and their inability to treat unknown or newly appearing diseases. For treatment and access to pharmaceutical cures, they sought out specialists (including both government vets based at agricultural science stations (*nongke yuan*) and duck doctors). The transaction with the duck doctor, therefore, involved a judgment of trust in the relationship above and beyond observation of the birds.

Uncle Wu's observation of bird death was rejected by Hao, the duck doctor. Hao deployed his own observations of the birds immediately prior to the provision of the medicine to support a counter-claim that the medicine had worked, and the prescription was effective. The even balance of claim and counter-claim made clear to me that experiential observations were not the only factor involved in authenticating a duck doctor's diagnosis. The truth of the duck doctor's diagnosis was relative to whether the duck doctor's word could be bound to experiential observations of the bird's health, disease and death. The material efficacy of the prescription is judged by how well words can be linked to observations. But how are these links made and strengthened or weakened?

The disciplines of both medicine and economics--and sometimes, the accompanying anthropological studies of medical and economic practice--treat exchange as a binary relationship between two actors.<sup>348</sup> But Hao and Uncle Wu were unable to resolve this dispute on their own. Instead, a third person, Wu's niece, played the determining role in judging the efficacy of the prescription. It was the niece who had arranged the diagnostic encounter in the first place, and who Hao and I had met on the way to see Uncle Wu. She involved herself in the dispute over efficacy as well. When the niece heard that Uncle Wu had doubted the prescription, she immediately questioned his judgment. "What do you mean it wasn't effective?" she asked him, according to an account I was told later. "Our medicines were all effective!" Referring to her many diagnostic encounters with Hao Weidong (as well as, pivotally, the medicines gifted that same day), the niece asserted the *general* efficacy of Hao's medicines. In the end, this led

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<sup>347</sup> Judith Farquhar, *Knowing practice: the clinical encounter of Chinese medicine* (Boulder: Westview Press, 1994). Cf. Richard Rorty, *Philosophy and the mirror of nature* (Princeton: Princeton University Press, 1979), for a powerful critique of Western philosophical tradition's *epistemological* mistake of attempting to justify the relation between mind and nature.

<sup>348</sup> For a critique of models of binary exchange in economic and economic sociology, see Callon, Meadal, Rabiharesoa, "The economy of qualities" *Economy and Society* (2002). For a critique of models of binary exchange in medicine and medical anthropology, see Michael Taussig, "Reification and the consciousness of the patient" *Social Science and Medicine* Vol 14B, No. 1 (1980): 3-13.



the uncle to retract his accusation. In the diagnostic encounter between the duck doctor Hao and the quail breeder Wu, it was a third person that produced a final determination of truth and efficacy in the diagnosis.

She did so by attributing a general efficacy to Hao's medicines, thereby increasing the strength of the link binding Hao's word to the observations of the birds. Once her claim was made, Hao's word counted for more and this was enough to overrule the observations of the uncle. But her reference to the *generality* of Hao's medical efficacy reveals the importance of her *relationship* to Hao in the authentication of the true diagnosis. In modern biomedicine, the "clinical encounter" is the site where diagnostic truth is made. The clinical encounter is a particular formulation of the doctor-patient relationship, whose truth is grounded by locating the patient's body in a series produced by the hospital and statistics. Medical anthropologists have suggested that the clinical encounter actually involves "transactions" that move beyond the doctor's examination of the patient. As Arthur Kleinman puts it, "the target of treatment, then, will be seen as involving considerably more than the patient's body." Kleinman claims that in addition to judgments about the body, the clinical encounter involves transactions of beliefs between the physician and the patient, or the patient's family. Kleinman called these beliefs "explanatory models." "Health care relationships," he wrote, "(e.g. patient-family or patient-practitioner relationships) can be studied and compared as transactions between different EMs [explanatory models] and the cognitive systems and social positions to which they are attached.

The duck doctor is, obviously, not a physician of human patients; the diagnostic encounter is fundamentally a relationship between doctor and poultry breeder, not physician and patient. A second significant difference is that the duck doctor's diagnosis does not take place in a clinic, but rather in spaces adjacent to the marketplace. As a result, the relationships that ground the diagnostic encounter contain a very different set of material and moral transactions than those described by Kleinman and other medical anthropologists. The niece based her judgment on a lengthy series of diagnostic and pharmaceutical exchanges with Hao, successful enough to say that "all" of them had worked. But the niece was not simply placing one set of experiential-observations against another; the life of her quails against the death of her uncle's quails. Hao described his relationship to the niece as a *friendship*, and I observed that Hao often went out of his way to offer her assistance, such as helping her to unload quail eggs when she arrived at the Xiaolan market. Hao's gift of medicines to her on the way to meet Uncle Wu both affirmed her important role in the exchange and cemented their relationship.

The development of personal relationships that combine both moral obligation and instrumental interest is known in contemporary China as *guanxi* (lit. 'relation'). Anthropologists have shown that the extension of *guanxi* practices beyond kin or locality appeared in response to socialist modernization and expanded during the Reform era. Building *guanxi* is not a fixed cultural code, but rather a practice through which relationships are made and re-made in particular acts of gift-giving, honorific speech, or other exchanges of feeling and obligation.

The extensive literature on *guanxi* focuses on the relationships in terms of their economic impact, as a distinctive Chinese form of sociality, their association with corruption, or their potential for politics, but little or no work has focused on the role of *guanxi* in making claims of truth or knowledge.<sup>349</sup> The duck doctor's diagnosis, however, is situated firmly within a series of

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<sup>349</sup> An early text to identify the distinctive form of Chinese sociality as based in concentric "webs" of relationships around the individual, rather than structured "organizational" groups, is Fei Xiao-tong's *From the soil, the foundations of Chinese society: a translation of Fei Xiaotong's Xiangtu Zhongguo* (Berkeley: University of California Press, 1992) [Original *Xiangtu Zhongguo* published in 1948]. In this work, Fei does remark that rural

'friend-making' transactions. If we compare this to Kleinman's analysis of 'transactions' of beliefs between doctor and patient, we see an important difference. Here, in addition to beliefs or explanatory models, money, medicines, gifts, meals, favors, honorary speech, or words of politesse may all be exchanged among the counterparts. Second, these extended transactions are not external to, or carefully separated from, the diagnostic encounter. Rather the exchange of gifts is considered in terms of a series of transactions that *includes* the diagnostic encounter, and vice versa.

For the duck doctor, the diagnosis must fuse a discourse (the diagnostic truth-claim) to observations of material efficacy (the prescriptions effects on duck bodies). The glue that helps to accomplish this fusion comes, above all, from the ethical conduct of *guanxi* relationships. Rather than a wood-and-metal toolkit, Hao uses this ethical equipment to build his diagnostic truth. The conditions for judging the diagnostic encounter are influenced by judgments about the moral character of the relationships, and, vice versa, the character of the relationship is influenced by the material outcome of the diagnostic encounter and prescriptions.

The importance of Hao's *guanxi* with the niece was verified for me when, a few weeks later, he invited me to travel along with him on a medicine delivery to the niece's house. Hao doesn't own a car, since much to his chagrin he is "not a *laoban* (boss)", and we set out for the bus stop as the sun fell low across the horizon, bags of medicine in hand. It was a Saturday evening, and bus after bus was filled to the brim with young men and women returning from Nanchang city to the rural villages. The buses were overflowing, sometimes they would not even stop, and, when they did, we couldn't climb aboard. Hao finally called a friend with a mini-van who drove us to another bus station across town. But it was the same story there, and the friend could drive us no further in his van.

Increasingly frustrated, Hao finally grumbled that the only reason that he was delivering the medicine was that the niece was an "old customer" who had stood up for the efficacy of his medicines when her uncle doubted them. We finally got onto a long-distance, coach-style bus, and the driver let us off at a crossroads about fifteen minutes later. By now night had fallen, blindingly black.

*Laodi*, Hao said to me as we waited in this lonely place, cold dropping with the evening, 'really I shouldn't be delivering this medicine. They should come to the shop to pick it up. But from the first I must *zuoren*, then they will introduce me to their friends, so that in a few years I will be able to earn good money.'

When we finally arrived at the niece's house, the supper plates were already on the table, steaming, including a bottle of *baijiu* and quail eggs made three ways: preserved, fried in an omelet, and steamed.

### *Zuoren: the practice of becoming human*

Hao Weidong arrived in Jiangxi province a few years before I met him, leaving behind failures in the construction industry up north to start anew. All he brought with him, he told me, was a brochure from the Puqiang livestock pharmaceutical factory. He traveled the fields and wetlands of Nanchang county, looking for the low grey roofs of the duck sheds, and delivering

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villagers engage with the natural world in the same logic as with social relationships: *shuxi* (熟悉—familiarity, with the literal meaning of 'ripening'). Also, the anthropology and history of science has shown the importance of relationships of communication and trust in the "authentication" of scientific claims. See Mazzotti, ed., *Knowledge as social order*, *op cit*. Shapin and Schaffer, *op cit*; Shapin, *op cit*.

lectures on poultry disease. If there were sick birds, he would cut them open and make a stab at a diagnosis. He was constantly learning: borrowing experience from farmers, examining birds, reading books at night. But at the same time, he also cultivated relationships with farmers. Showing the brochure to them, he placed a large 'X' next to any drug they expressed interest in. In most cases, Hao would purchase the selected medicine from the factory on credit and deliver it to the farmer at no cost, hoping to earn money in the future. Through these visits, Hao aimed to establish himself as a person with both ability and moral uprightness: in his words, to make farmers his friends.

Hao frequently discoursed on the importance of friendship and making friends (*jiao pengyou*). On one of my earliest visits to his shop, he invited me to eat lunch at a beef specialties restaurant attached to the Xiaolan marketplace. Sharing a bottle of *jinjiu*, a kind of spiced, herbal liquor, he declared that we were now friends. Friendship implied obligations of reciprocity, but Hao made it clear that there were a range of such "friendships" that varied in the way that they combined economic interests with moral obligations. The vulgar and casual collegiality of the banquet or gambling buddies (*jiurou pengyou*, *dubo pengyou*) implied obligations very distinct from the kinship-like bonds of the "dear friend" (*qinai pengyou*). Most farmers were referred to as business friends (*shengyi pengyou*), and even within this category there were fine and fluid gradations of norms and relationships.

As scholars of *guanxi* such as Yang, Kipnis, and Yan argue, the practices of friend-making should not be seen as strictly instrumental. The variability of *kinds* of friendship confirms this point. But beyond rejecting the instrumental actor, little attention has been given to the *subject* of *guanxi* exchanges. As Hao pointed out to me in the passage quoted above, 'making friends' was a practice, but it was not a practice that was only oriented toward others. Rather, 'making friends' depended on techniques of self-cultivation, or *zuoren*, that made one capable of or successful in friendship. Hao stressed that this was not a momentary decision but a life-long process. "You can't say one day, "Tomorrow I will start to *zuoren*" he told me one day, before turning to me and explaining that in essence my research was also a process of self-formation (*zuoren de guocheng*): the practice of social investigation (*shehui diaocha*) was self-transformative, but it was also the formed self, the self cultivated in a particular form, that enabled me to develop relationships with others and make 'social' knowledge.

In order to humanize himself (*zuoren*), Hao relied on what I described above as an "ethical equipment." In his later lectures, Michel Foucault studied the ancient Greek and Roman *parakeue*, or ethical equipments, which he defines as "discourses with a material reality."<sup>350</sup> For the Stoics, to take one of Foucault's favored examples, true discourses were investigated, spoken, listened to, memorized, written and otherwise exercised in order to transform the *ethos* of the subject. The value of these true discourses lay not in their position within an abstract body of knowledge, but for the transformative effects they could have on the subject. For example, Marcus Aurelius could write of how the world looked from miles above, not for the purpose of adding to what is known of the world, but in order to demonstrate the insignificance of the human subject within this massive world.

Hao Weidong, the duck doctor, had himself collected a set of true discourses which shaped how he interacted with others, making friends and cultivating *guanxi* relationships. Resembling his medical approach that employed both "Chinese" and "Western" medicines, Hao's ethical equipment was eclectic. Some truths came from a Buddhist tradition, such as his refusal to eat snails or beef. Others came from a broadly Confucian tradition. Still others came from the

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<sup>350</sup> Michel Foucault, *Hermeneutics of the Subject*, *op cit*.

business ethics of Amway, the U.S. "direct-marketing" firm, which had recently made a big push into Nanchang.

Years earlier, Hao told me over lunch one day, he had heard a speech given to a select audience at a hotel in Beijing (he indicated that he was not the intended audience, without making clear how he had managed to get inside). The speaker lectured on how to cultivate oneself as a human being (*zuoren*). The speaker divided the human (*ren*) into four kinds, each with its own permutation of two elemental qualities: 'talent' (*cai*) and virtue (*de*):

The gentleman (*junzi*) has both talent and virtue; the inferior man (*xiaoren*) has talent but lacks virtue; the mediocre man (*yongren*) lacks talent, but has virtue; the idiot (*shazi*) lacks both talent and virtue.

Hao's application of this maxim to himself was to say that "it is difficult to be a gentleman (*junzi*)." The maxim tells him that it is better to be a mediocre person than to be morally inferior; better to act virtuously without talent than to seek talent by abandoning virtue. He often admitted to poor or unsystematic knowledge of poultry diseases and medicines, and explained that he sought to enhance his practice largely through ethical (rather than technical) improvement.<sup>351</sup>

Hao also drew upon a truth discourse that could be described as 'sociological' in order to understand his position and how to act in and through relationships. I stopped by Hao's shop one afternoon after visiting some other veterinary shops in the center of Nanchang county seat. After I arrived he received a phone call, and, after listening for a minute, used the excuse that he was with a friend to get off the phone. He then turned his phone off. "I have been receiving phone calls all day!" he complained to me. He explained they all came from the drug factories, who sell the packaged "Western" medicines to retailers like himself. Hao was affiliated with two or three livestock pharmaceutical companies. These companies were based in major Chinese cities like Shanghai or Guangzhou. The precarity of Hao's business was made clear to me in the way he dealt with the deliveries of medicine from the drug factories. They shipped medicines at a regular schedule to a nearby shipping warehouse. The warehouse held the medicines until Hao came and picked them up. Since he had to pay for them when he picked them up, however, he tried to leave them at the warehouse for as long as possible. After two weeks or so, they would be sent back to the drug factory. The factory representatives had contacted him because they believed some of the medicines intended for him had been shipped back, which they were very angry about. Hao denied they were his.

"They are always trying to make me buy more things, its really irritating." Anger and irritation stirred up inside him, he pointed out how 'doing business' (*zuo shengyi*) differed radically from 'going to work' (*shangban*). People who *shangban* receive a regular salary, but in business you always need to be preparing for changes in the environment. Pointing to the now completely empty interior of the market, he reminded me that in just the past year, the number of trucks bearing farmers and eggs to the market had significantly decreased. There had been trucks backed up out of the marketplace, waiting to deliver eggs. "Boy, if it was like that now, I would be making some money!" These days, business in the market is tepid, he lamented.

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<sup>351</sup> The apparent 'modesty' of Hao's maxim is somewhat complicated by the way it permutes a more common version of the maxim. In the common version, holding both virtue and talent is ascribed to the sage (*shengren*), while lacking talent but having virtue is ascribed to the gentleman (*junzi*). Hao, therefore, while claiming to abdicate himself from the possibility of being a gentleman, actually claims for himself the structural position typically ascribed to the gentleman.

After the new year, Hao hopes to buy a car, which he could then use to easily drive to Wuyang, or all of the other places where he has relationships with farmers, and not have to rely on people coming by the marketplace. I asked if a lot of the other veterinarians have their own cars. He said, some do, the ones who have been doing this for a long time--I only started a year ago, after all. Some have motorcycles, some have cars, but the majority don't.

Thinking about the car that he hoped for, he suddenly remarked that he “wasn't a boss” (*laoban*). A boss, he said, should own his own company (*gongsi*), registered with the government, and should have hired “at least ten employees,” so that he should not have to work himself. A boss, if he wanted a car, would just say, “Mr. Fang,<sup>352</sup> let's go buy a car.” I am not a boss, but “one who cares for himself” (*ziguzhe*). I run the shop on my own, all the responsibilities are my own. Not like a *qiyejia* ('entrepreneur) or *zebenjia* ('capitalist') who have money and hire employees to run the company for them.

I asked him whether he thought that *qiyejia* and *zebenjia* were the same. I had recently read an article on “entrepreneurship” in China which had chosen *qiyejia* to translate 'entrepreneur', but noted that in China opinion differed about whether this should be referred to as *zebenjia*. In response, Hao pulled out a piece of paper and drew a X/Y axis (see image). The category *furen* (富人 wealthy), he said, included both *zibenjia* and *qiyejia*. He argued that *ziguzhe* (自顾者) was not as badly off as the poor person (*qiongren* 穷人), but yet still was on the same side of the equation, essentially opposed to the wealthy, and always striving to move towards the right hand side of the chart. The point is not to say that Hao's words are an accurate sociological description of his status, but rather that his knowledge of the social is one part of his ethical equipment, helping him to understand his goals and relations to others.

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<sup>352</sup> My Chinese name is Fang Li'an.

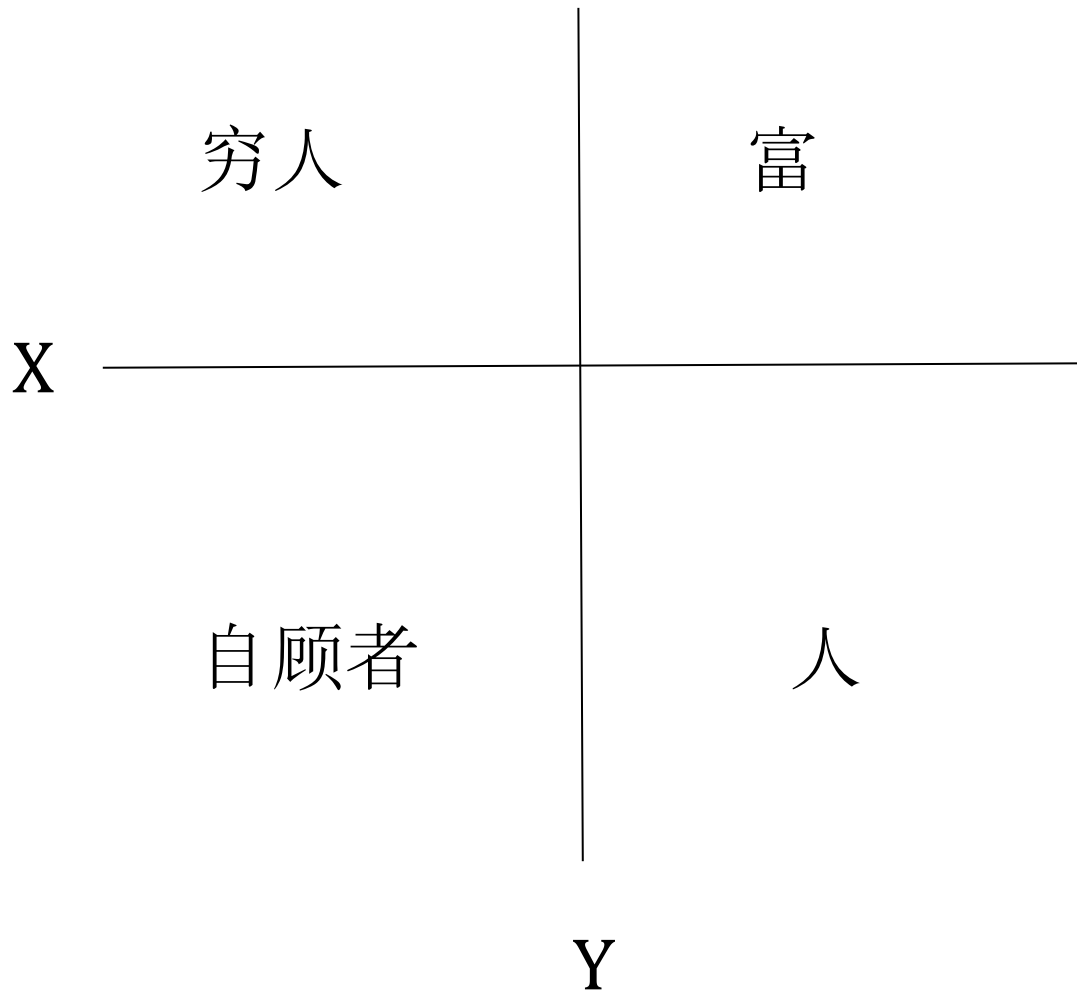


FIGURE 3: Hao Weidong's sociology.

## *The diagnostic spectrum*

Hao aims to cultivate relationships with farmers not only as individuals, but in order to gain access *through them* to relationships with others. Any individual farmer also always represents a position within a web of kinships or friendships with other farmers. In the case recounted above, Hao cultivated a friendship with the niece not only in order to treat the birds of her own household, but also in order to access other households, such as Uncle Wu's. As I argued, the relationship to the niece continued to shape and inform the relationship to Uncle Wu long after the initial introduction.

Hao made the importance of these webs of relations clear to me one day when he talked about the trucks that farmers drive to market. He works particularly hard to cultivate friendships with the drivers of trucks, giving small gifts or favors, or inviting them to eat in a "beef specialities" restaurant that is attached to the Xiaolan marketplace. Hao told me that farmers will often follow the recommendations of their driver when purchasing medicines. If the driver is a friend of Hao's, they will visit his shop instead of another.

This interest in trucks and their drivers is built on an understanding that the truck is metonym for web of relationships. The farmers who band together to lease a truck do so based on ties of familiarity, either from common membership in a village or literal kinship. Hao's strategy reflects the fact that a friendship with one member of a particular truck almost inevitably brings with it a relationship to the other members of the truck. The cultivation of this friendship with one member quickly flows through and extends into these other relations. A friendship with the truck driver, in particular, can lead to the development of friendships with the other members of the truck.

Depending on the affect passing through the web in a particular moment, the density of ties can impact a particular diagnostic encounter in either a positive or negative direction. The strengthening or weakening of a bond to one friendship can quickly flow to impact the bonds of other friendships. The duck doctor's diagnosis is both an exchange that cultivates these relationships, and is impacted by the changes in these relationships. The efficacy of the diagnosis and prescription is not judged at a different register or in a different domain from judgment about the duck doctor's character. This is not to say that the cultivation of a good friendship is enough to overdetermine all observations about the success or failure of a prescription, as accusations of charlatanry often contend. Rather, it is to say that judgments of efficacy and personal character are tried in the same court.

In some cases, a farmer observes a medicine's apparent failure, but trust in the word of the duck doctor modulates his judgment. Perhaps there were other circumstances, or the medicine was not fed to the birds properly. In other cases, the farmer refuses to accept the word of the duck doctor, and insists that the medicine failed. In this case, the judgment of character can also be inverted: the friend becomes a cheat (*pianzi*).

Whether a prescription leads to a strengthening or weakening of the bonds of friendship depends partly on the other relationships within the local web of relations. If these relationships are particularly strong, it may help to overrule the judgment of the index farmer with the sick birds. This appears to have been the situation in the case of Uncle Wu's quails. On the other hand, if the index farmer persists in judging the duck doctor a cheat, he is likely to spread his bad feelings to his relations. Hao told me that after conducting a diagnosis and prescription, he often worries that the farmer will judge it ineffective and then spread negative feelings to others. In one case, he told me, a farmer had talked badly of Hao to the other farmers in his truck, telling

them that Hao's medicine was not effective (*bu guanyong*). None of the other farmers ever returned to Hao's shop.

By coincidence, one day as Hao and I were traveling by bus to another farm, this "plaintiff" farmer called and asked if Hao could visit his farm and diagnose a disease afflicting his ducks. On the telephone, Hao avoided committing himself, telling the farmer that he wouldn't be able to make it this afternoon, as we were on the bus to another farm already, and the day was getting short. But when he had hung up, he told me he was determined he would never go to such a 'negative' farmer's household. Repeating to me the story of how this man had spread bad feelings to the others in his truck, Hao told me he would not diagnose his ducks, neither in person nor based on the farmer's reports. "I told that man he has to come to my shop with the names of the medicines he wants written on a piece of paper, then I will sell them to him," he said simply and defiantly.

Hao's statement makes clear that a moral valence adheres to the range of diagnostic encounters. These can be arranged along a spectrum between two poles. At one pole is a visit to a breeder's household to diagnose the birds in the field. Going to a breeder's household is referred to as *kan yazi* (looking at the ducks) or *kan anchun* (looking at the quails). When Hao went to visit Uncle Wu, for example, he spoke of the trip as 'going to look at the quails'. Judith Farquhar has written about the importance of the Chinese way of speaking of a visit to the doctor as *kan bing*, or "looking at the illness". Both doctor and patient *kan bing*, they "work together on a better embodiment." Farquhar argues that *kanbing* involves a very different conception of the relationship between doctor, patient and disease than biomedical diagnosis. In biomedicine, the diagnostic gold standard is the corpse, and in many diagnostic situations--such as CT scans--the patient plays the corpse to the doctor's clinical gaze.<sup>353</sup> But in *kan bing*, both sufferer and doctor "look at the disease" together.

For the duck doctor, *kan anchun* or *kan yazi* is not important solely for the examination of the bird that it entails. In the case of Hao's visit to Wu's household to *kan anchun*, I was at first quite perplexed, because Hao had already made a diagnosis and prescription and there was no indication that he might alter it. Rather, I found, *kan anchun* has much more to do with the duck doctor's relationship to the quail breeder. Since these farmers almost exclusively raise birds through the economic unit of the household,<sup>354</sup> and raise birds in or near the home, going to see the sick birds involves a visit to the farmer's house and family, with the moral obligations and significance that this entails.<sup>355</sup> Often these visits will end with drinking cups of tea inside the farmer's home, or even dinner. As a result, the diagnosis is situated within a strengthened relation of trust.

At the other pole is a refusal of diagnosis. In practice, what stands in the place of this pole is the transformation of the diagnostic encounter into a pure market sale. In a pure market

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<sup>353</sup> Cf. Barry F. Saunders, *CT Suite: The Work of Diagnosis in the Age of Noninvasive Cutting*. Durham, N.C.: Duke University Press, 2008.

<sup>354</sup> A consequence of the "household responsibility system" land reform policies that broke up collective land in the late 1970s and early 1980s, defining each household an economic unit, and distributing land-use rights by household. Although various forms of increasing scale (*guimo*) are being experimented with since the 1990s, most of these--particularly in the poultry sector--involve various forms of organizing household-scale farms, from cooperatives (*hezuhui*) to dragon-head integrated corporations (*longtou qiye*). Cf. Qian Forrest Zhang and John Donaldson. 2008. "The rise of agrarian capitalism with Chinese characteristics: Agricultural modernization, agribusiness, and collective land rights," *The China Journal* 60.

<sup>355</sup> Cf. Kipnis, *Producing guanxi*, *op cit*.



sale, the duck doctor does not commit his diagnostic word to the medicine--there is no *prescription*. In this extreme case, it is as if the duck doctor was merely the anonymous medium of a market transaction. Hao's insistence that the 'negative' poultry breeder write down the medicines he wants on a piece of paper before arriving seems an effort to reduce the diagnostic encounter to an absolute minimum. Hao will say no words.

In between these two poles are a range of diagnostic encounters and pharmaceutical exchanges: from the examination of sick or dead birds at the duck doctor's shop; to the identification of a diagnosis and appropriate prescription based on the breeder's report of observed symptoms; to a recommendation for treatment given to a breeder passing through the market; and so on. The reciprocal relationship between judgments of prescriptive efficacy, and judgments of moral character are configured within this range of diagnostic modalities. The friendship Hao had developed with Wu's niece, including visits to her farm, affirmed and authenticated his diagnosis of Uncle Wu's quails. On the other hand, when the 'negative' breeder accused Hao of being a cheat, Hao responded by reducing their relationship to a market transaction.

This suggests that accusing the duck doctor of being a cheat or charlatan (*pianzi*) is simply one within a series of possible moves ranging between these poles, and structured by the patterned configurations of moral obligation and material observation. In the duck doctor's diagnosis, truth does not exist solely on the side of moral trust nor exclusively on the side of observed materialities. The true diagnosis can be formed through variable mixtures and syntheses: a friendship overrides observations in one case; or observations of the ducks and the medicines is enough to legitimate the prescription despite lack of close friendship in another case. To make an accusation of fraud is, on the other hand, a move that holds to an extreme position within this field of moral/material polarity. It is a move in which material observations completely trump friendship. One can no longer believe the prescription in the light of what one observes with one's own eyes. To make an accusation of fraud is to cut ties of friendship, to break off all exchanges, or reduce these exchanges to the filling of a written order.

But in doing so, the denunciation of the duck doctor as quack or fraud remains *internal* to the range of possibilities provided by the duck doctor's diagnostic spectrum, even as it tries to *externalize* itself from these possibilities by denouncing their 'corruption'. The extreme in which material observations completely override relationships based in trust is one pole within the patterned configurations of the diagnostic spectrum. This insight allows us to return with a new perspective to the discourse of the "office veterinarian," the veterinary reform and its metric of 'quality' (*suzhi*) that I started the chapter with. In attempting to ground "high-quality" veterinary medicine on a version of scientific truth grounded in the autonomy of the scientific observer, the "scientific" reform of the veterinarian takes to the extreme position in which material observation is the sole criterion of the true. As a result, it is no surprise that the cultivation of quality in the "office veterinarian" brings with it the denunciation of the "grassroots veterinarian" as "low quality" and ethically suspect.

As the statement by Cai Haifeng that I started with makes clear, the critique of "those vets who sell medicines and feeds" because they "will say anything in order to further their own interests" defines the fraud as one who allows speech and self-interest to obscure material reality. For Cai, the observation of matter should trump discourse, the object should trump the interested relation. The ethics of scientific truth, for him, requires the separation of truth from ethical judgments. But this space in which truth appears as the direct product of material reality has a judgment of value at its very origin. The duck-doctor, whose practice builds a circuit in which

judgments of moral personhood are configured with judgments about empirical evidence, must be named and excluded as a condition of possibility for veterinary reform.

The rise of a positivist "international" science in the post-Reform period is, then, not a simple substitution of factual or practical truth ("seek truth from facts", "make practice the criterion of truth") for the Maoist overdetermination of reality through idealized ("ideological"<sup>356</sup>) values. Rather, China's post-reform scientific development follows on a primary judgment of what kinds of life and social relation are valued. Moreover, this value judgment, understood as an extreme move in the circuit of configuring matter and morality, involves itself in a symbolic contest over moral values--and only becomes more involved the more it claims to stand on the grounds of material alone, "outside" of the space of moral relationships.<sup>357</sup>

Ann Anagnost has argued that discourses of *suzhi* deploy an absolute fissure between high and low, between those who have and those who lack quality. She suggests the differential should be understood as an extraction of *suzhi* value from one set of bodies to another. In the veterinary reform discourse, the deployment of *suzhi* tracks between the cultivation of 'quality' in official veterinarians and denouncing the lack of quality among duck doctors. Although this may be extractive, in Anagnost's terms, it also marks a moral distinction between two modes of veterinary practice. The duck doctor's reciprocal friendships are denounced as corrupt and fraudulent, whereas the "office veterinarian" must break social relations to poultry breeders as a precondition for speaking official truth. To speak only on the basis of nature is itself a moral claim of *disinterest* in the relation to the poultry breeder. In this way, the refusal of an ethical relation can be specified as a polarizing move within the diagnostic spectrum, but also only one of many possible moves. The duck doctor's practice suggests the possibility of a form of veterinary knowledge that can acknowledge and even cultivate social relations with poultry breeders.

My own anthropological truth and writing is also implicated in the dynamics of this discourse. Luc Boltanski, in a critique of critical sociology, has argued that the social sciences rely on a "*thought experiment* that consists in positioning oneself outside" society or social life.<sup>358</sup> In this chapter, I have attempted a different thought experiment, which must now be

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<sup>356</sup> See Schurmacher, *Ideology and Organization* for a Parsonian analysis of how Maoist Chinese used "ideology" to substitute for the lack of an appropriate "culture/ethos" following the radical shift in social organization that accompanied communist revolution.

<sup>357</sup> Cf. Clifford Geertz, "Ideology as a cultural system" in Geertz, *The interpretation of cultures* (New York: Basic Books, 1973) for a symbolic anthropological critique and reformulation of the notion of ideology, which argues that ideology is a kind of symbolic practice (a "model of" and a "model for"), but one which specifically attempts to provide "maps of problematic social reality and matrices for the creation of collective conscience." In this sense, ideology is something like the work of Weber's politician. Geertz concludes by contrasting ideological work on the level of "ideals" and "values" with scientific work through efforts of clarification and disinterestedness. "The social function of science vis a vis ideologies is first to understand them--what they are, how they work, what gives rise to them--and second to criticize them, to force them to come to terms with (but not necessarily surrender to) reality." Yet, Geertz recalling Weber reminds us, we cannot forget that science itself requires an "evaluative" *commitment* to its "vision."

<sup>358</sup> Luc Boltanski. *On critique: a sociology of emancipation*. Malden, MA: Polity, 2011.

carried to its conclusion. How do those who judge the duck doctor's truth, whether the veterinary reformers or myself, appear if we are included within the duck doctor's spectrum of diagnostic encounters? How are those positioned outside bought inside the field that is their object?

I have already suggested that the critique of the charlatan, while claiming to stand outside, merely represents one polar position within the duck doctor's diagnostic spectrum. And if I include myself within the same spectrum? I have my doubts. But I do not denounce Hao as a fraud. This is not because I want to bracket all judgment about truth, nor is it to say that *for others* there is a particular truth, or function, which is different from the truth *for myself*. With the molasses-black rice wine Hao gave me on my shelf above me as I write these words, I cannot but feel an obligation to our friendship. Critiques that presume to expose 'the fraud' often seize on gifts as a proof of corruption, of interested relations overdetermining material evidence. Such critiques assume that the gift makes everything simple. But the true gift--a transaction implying a delay of reciprocation--is never clear about the obligation that it entails. The warmth in my belly from the wine may not be enough to make me *believe* the duck doctor: but it is enough to keep me from denouncing him, and to conclude by raising the question of how the conduct of friendship might bear on the practice of making knowledge, including anthropological knowledge.<sup>359</sup>

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<sup>359</sup> On the anthropology of friendship and its problems in the fieldwork encounter, see Paul Rabinow, *Reflections on fieldwork in Morocco, op cit.* to which this chapter in particular is deeply indebted.

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