

ETHNOGRAPHIC  
PLAGUE

*Configuring Disease on the  
Chinese-Russian Frontier*



CHRISTOS LYNTERIS



# Ethnographic Plague



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Configuring Disease on the Chinese-Russian  
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*To the memory of David J. Riches (1947–2011),  
anthropologist, teacher, and friend.*



## NOTE ON TRANSLATION AND TRANSLITERATION

Romanisation of Russian follows the British Standard (BS 2979:1958), with the exception of endings in surnames, where *-ŭ* is rendered *-y* (hence *Zabolotny* and not *Zabolotnĭ*). In the case of Cherkassov and Beliaevsky, the names as appear here have been retained in accordance to established bibliographical reference in English (instead of Cherkasov and Belyavsky). I would like to thank Anna Afanasyeva for her generous help in proofreading these transliterations. Romanisation from the Chinese follows the pinyin system, with the exception of direct quotes and bibliographical notes, where the original Wade-Giles transliteration is retained. Throughout the book the Siberian marmot is spelled *tarbagan*, with the exception of direct quotes where the original text or direct transliteration is retained. Translations from the German are by Emily Stavridis with editorial help by Lukas Engelmann. Translations from the French are the author's, with due thanks to Frédéric Keck for suggestions and corrections. Translations from the Russian are by Nikolai Ssorin-Chaikov unless otherwise stated. The author retains all responsibility for mistakes in translation and transliteration.





## PREFACE

My interest in plague and its relation to marmots began in the course of conducting anthropological fieldwork in a local Centre for Disease Control and Prevention in China in 2008. Examining medical journals and archives in search of epidemic events that shaped the modern Chinese state, I quickly came to realise what a series of medical historians had already detected: the birth of Chinese epidemiology, even of the biopolitical paradigm in China as such, could be traced to the 1910–11 Manchurian pneumonic plague epidemic. Reading through published and archival material, I became fascinated by the role attributed to marmots as the origin of the outbreak that left 60,000 dead and radically reshaped the relation between state and population in China.

Faced by the devastating epidemic, Chinese and Russian epidemiologists seemed to agree that the source of plague amongst humans was not the usual suspect, the rat, but an altogether different rodent species: marmots. Noting that the animal had become a lucrative source of income due to the unprecedented demand for its fur by the European markets after 1908, Chinese medical authorities blamed marmot hunting for the spread of the disease from the wild rodents to humans. Yet, to my fascination as an anthropologist, I soon realised that the hunters blamed univocally for the disaster were not native Mongols or Buryats, who were relying on marmots for meat, fat, and fur, but Chinese migrant workers flocking to the region for quick profit.

This outbreak narrative lauded natives as knowing everything about plague, including how to prevent it, and blamed Chinese migrant workers, who were employed in the mass-hunt of the particular animal, as an

exemplar of ineptness and ignorance. While natives were said to be able to recognise ill animals from their walking and signalling behaviour, so-called migrant coolies completely failed to relate to this alleged natural abundance of warnings. They supposedly captured the animals greedily and in ways breaking with tradition, thus contracting the disease, which they proceeded to spread in dirty underground hovels and opium dens. Plague thrived amongst their ranks, spreading like fire in the prairie, so that when the so-called coolies decided to return home for the Chinese New Year, they disseminated it across the great Manchurian cities all the way south to Beijing and their Shandong homeland.

As an anthropologist interested in biopolitical aspects of epidemiology, my attention was immediately captivated by this narrative. Still, I took little time to think about the native-knowledge allegations and their epidemiological as well as ethnographic implications. After all, I too, like most people in the West, considered plague to be a forlorn disease, a sign of our premodern past. Then, almost a year following my return from China, in the summer of 2009, pneumonic plague struck Xinghai County in the Hainan Tibetan Autonomous Prefecture of Qinghai Province. The first signs came in mid-July 2009, when a thirty-two-year-old Tibetan herder fell ill, after burying his dog, which had supposedly eaten an ill Himalayan marmot (Wang et al. 2011). The man died on July 30; a day later twelve more relatives were reported ill and isolated Xinghai County Tibetan hospital. On August 1, the People's Republic of China officially announced that it had a pneumonic plague outbreak on its hands. A seventeen-mile radius quarantine covering 1400 square miles and involving twenty-three quarantine stations was imposed on the nearby town of Ziketian. Though two more contacts succumbed to the disease, raising the death toll to three out of twelve infected individuals, containment measures proved effective and the epidemic was successfully stayed. The death toll hardly matched the epic proportions of 1910–11, or even that of the other big Manchurian plague epidemic, in 1920–21. The epidemic would not have retained my attention for much longer if it was not for one detail: having traced the disease back to marmots, the Chinese authorities openly blamed indigenous hunters for the epidemic.

The 2009 outbreak and its connection to marmots created a flash of interest in the international epidemiological community with glimpses of the news also appearing in the daily press. This small medical panic was once again rehearsed after the 7.1-magnitude earthquake that hit Yushu County, again in Qinghai Province, in April 2010. A plague scare, which

never materialised in an actual outbreak, swept China, catching the attention of international media. In the worst-case scenario rehearsed by columnists, plague was at any moment ready to spread from marmots to the earthquake victims in the region (Anon. 2010). Whether that was to occur via the latter eating the marmots, or in some other way, was left to the audience's imagination. A quick research of dates and facts made evident that such crises were not that rare, with one or more cases of plague being reported in the region on an annual basis. Whilst global attention is usually drawn to these events as a result of the draconian quarantine and isolation measures imposed on stricken communities—the July 2014 outbreak in Yumen being the most recent of such biopolitical outrages (Kaiman 2014)—what is overlooked is a major shift in the epidemiological reasoning surrounding these outbreaks. Native subjects, who had been lauded for their knowledge of plague and its prevention 100 years ago, now stand accused of spreading it due to their ignorance. Of course, the natives this time are (in all cases so far) Tibetan rather than Mongol or Buryat, whilst the marmot species is *Marmota himalayana* rather than *Marmota sibirica*. Still, this may be seen as a minor detail by comparison to the landslide shift of epidemiological narrative as regards the overall ethnographic profile of plague epidemics in the Chinese periphery.

The entire edifice of plague epidemiology in China after 1911 had been built on the premise that plague in what is tentatively known as Inner Asia is as old as the hills, yet as long as marmot hunting was left to natives no harm could be begotten, for their ancestral ways and native wisdom protected them, and China in turn, from the fearsome disease. Now, all of a sudden, the world was told that natives were irresponsible, naïve, and dangerous for global health.

Could this simply be because the new, twenty-first-century natives in focus are Tibetans? Did this shift from trust to blame reflect Beijing's increasing animosity towards the Qinghai Tibetan population, following the March 2008 uprising? Or did it actually indicate a challenge to long-held epidemiological theories regarding the zoonotic transmission of plague in the region? Contemplating these preliminary questions, I soon came to realise that no one appeared to be noticing the contradiction made evident by recent events. Books and articles, both lay and scholarly, cited the old Chinese native-coolie divide as a fact, adopting it in their narrative regarding the great 1910–11 epidemic. I thus came to realise that, rather than being of historical interest alone, the native-coolie binary exegesis of historical plague outbreaks in the wider region held an important place in the epidemiological imagination of plague today.

My interest in this topic led me to pursue postdoctoral research at the Centre for research in the Arts, Social Sciences, and Humanities (CRASSH) of the University of Cambridge (2011–13). The project, titled ‘Transregional Flows: The Social Ecology of Pneumonic Plague in Inner Asia’, was based on extensive research examining what I call the ‘native knowledge hypothesis’. Rather than pursuing to support or debunk the latter, my project progressed by focusing on the epistemological premises of knowing plague by means of ethnographic research, a practice much pronounced in the region I examined but also prolific across the globe in the context of the third plague pandemic (1894–1959). This book is an anthropological reflection on the medical history of plague research during the peak of the most recent pandemic of the disease. As such it owes much to a historical anthropological tradition that has since led me to pursue the topic of the configuration of plague from a new, visual perspective.

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## Introduction

### Plague Beyond the Laboratory

Between 1894 and 1959, a wave of plague outbreaks spread across the globe, striking major cities and harbours as well as rural areas in all inhabited continents.<sup>1</sup> The disease killed approximately twelve million people and established long-lasting endemic foci of the disease in Asia, Africa, and the Americas. What came to be known as the third plague pandemic led to one of the most extensive studies on any infectious disease at the turn of the century. This undertaking was disproportionate to the total number of deaths, when compared to those incurred in the afflicted areas by other diseases, such as malaria. Rather than sheer numbers, what fuelled research in the particular disease was its exceptionally high fatality case rate: approximately 60% for bubonic and 100% for pneumonic plague. Equally important were the economic burden and the civil disorder resulting from the outbreaks.

On a symbolic level, a crucial role must also be attributed to the notion that the pandemic soon came to be understood as caused by the same pathogen as the great mortality of the Middle Ages, known since the early nineteenth century as the Black Death.<sup>2</sup> Fears that the new pandemic signalled the return of the Black Death clad it in a legendary and terrifying aura. This ‘imaginary of menace’ also informed and amplified medical and governmental approaches to individual outbreaks, large or small.<sup>3</sup> Plague was thus rendered an object of knowledge under the bane of its perceived ability to wipe out humanity. This approach was medical as well as lay, with plague panic across cities and villages necessitating the study of the disease as a means to public health as well

as public order.<sup>4</sup> Plague research, one of the most prolific epidemiological operations of the half-century, was hence intricately linked to the mythic image of plague as the prototype of all human pandemics. This mythic, governmental, and scientific entanglement created the conditions for problematising the third plague pandemic and its regional and local manifestations in unprecedented ways.

For the first time in history, scientists had the technical tools (microscopes, cameras), the political means (post-Berlin Conference colonialism), and the conceptual framework (germ theory) that allowed them to conduct extensive and multifaceted research on an unfolding global pandemic. This public health crisis and the governmental and medical responses it elicited have not escaped the attention of historians. In the past three decades, scholars have studied the history of modern plague in Hong Kong, India, Manchuria, San Francisco, Senegal, and other important foci of plague at the time, contributing to an important corpus on the pandemic. Some of the principal themes of this research have been the colonial encounter dynamics of the pandemic, its impact on the global economy and international trade, and its implications for immigration and race relations.<sup>5</sup> And yet, in our case, it would not be altogether accurate to claim that ‘the historiography of colonial science has tended to be more concerned with its political nature rather than the activities of scientists and the history of scientific experiment and invention’.<sup>6</sup>

Parallel to, and in dialogue with, studies of the geopolitical and social history of the pandemic, another scholarly strand has focused on the study of plague epistemology. This has been an outcrop of what Graeme Gooday has called the fascination with ‘the laboratory in the history of science’; a critical approach flourishing since the late 1970s, which has left a definitive mark in studies of modern plague.<sup>7</sup> Whilst stimulating research on the production of plague-related knowledge, this focus on the laboratory has also created a distortion in our understanding of the development of plague epistemology. This book is an attempt to redress this bias by providing an alternative perspective on how knowledge about plague was produced in the context of the third pandemic.

In particular, this book aims to demonstrate that well after the laboratory identification of the bacterial agent of plague, medical researchers engaged in ways of knowing the disease that not only were field rather than laboratory based, but also drew on a methodological and epistemological tradition that has largely escaped the attention of modern plague historians: ethnography. Examining plague research on the Chinese-Russian

frontier between the eruption of the first recorded outbreaks of the disease in the region and the great Manchurian plague epidemics of 1910–11 and 1920–21, this book explores the role of ethnographic research and ethnographic imagination in the configuration of plague.

### PLAGUE, A BIOLOGICAL SKETCH

When not synonymous, as it often is, with just any sort of plight or calamity, plague is commonly imagined today as an infectious disease that disappeared with the dawn of the industrial age. This narrative ignores the fact that the third plague pandemic, the first of the particular disease to assume truly global proportions, took place in the last half of the nineteenth and the first half of the twentieth century, roughly between 1894 and 1959. It also overlooks the fact that outbreaks of bubonic and pneumonic plague continue to occur across the world, affecting up to two thousand individuals per annum.<sup>8</sup> To give but a snippet of this epidemiological picture, in the second half of 2014 plague outbreaks were reported in China, the USA, Bolivia, Peru, the Democratic Republic of Congo, and Madagascar.

And yet, after the World Health Organization (WHO) officially declared the end of the third plague pandemic in 1959 little research was conducted on the disease outside the Soviet Union and American-occupied Vietnam. Only following the 2001 anthrax mail-attacks in the USA did plague research witness an unexpected renaissance. Classified as a Grade-A biological-threat agent, in scope of its potential employment as a bioweapon by terrorists or enemy states, plague has since become the object of intense scientific study.<sup>9</sup> The intensification of plague research has shed new light on the molecular biology as well as the ecology of the disease, including the field of retrospective diagnosis of human remains through the application of ancient DNA methods. In recent years this has led to important historical breakthroughs as well as to new historical approaches of plague, advocating a closer cooperation between the humanities and the life sciences.<sup>10</sup> In terms of introducing plague to what I assume is largely a non-life sciences audience, it should be noted that this synopsis reflects the current understanding of the disease, which is likely to be significantly revised and advanced soon after the publication of this book.<sup>11</sup>

Plague is a zoonotic disease caused by a Gram-negative rod-shaped coccobacillus, known today as *Yersinia pestis* and in the past as *Pasteurella pestis*. The anaerobic bacillus is carried by a wide range of wild and domestic mammals and birds (of which more than 203 rodent species) as well as by a

number of insects; not only or principally, as often portrayed, by the black rat (*Rattus rattus*) and its flea (*Xenopsylla cheopis*). Whilst it is usually thought that plague maintains itself in so-called enzootic cycles which erupt into epizootics, or mass die-off events, at irregular intervals, research in the last fifteen years has suggested that ‘the evidence for separate enzootic and epizootic cycles is often unconvincing, and epizootics might simply represent periods of greatly increased transmission among the same host and fleas that support *Y. pestis* during interepizootic periods’.<sup>12</sup> Nonetheless, considerable efforts continue to be made to understand the cause of what is traditionally seen as epizootics of plague amongst different host populations, as this is the mechanism through which, more often than not, plague spreads between species, including humans. Whilst host abundance is typically considered to be a key driver, climatic factors (including climate change), host diversity, and host resistance, as well as anthropogenic factors such as land use change, are increasingly considered as affecting enzootic/epizootic processes.<sup>13</sup> At the same time, hitherto considered an arcane field of Soviet plague science, the study of the landscape ecology of the disease is also coming into the mainstream of plague research today.<sup>14</sup>

An important question regarding the transmission and maintenance of plague relates to the role of and interrelation between different animal and flea species. In the course of the third plague pandemic it was the black rat and its flea that played the central role in the transmission of the disease to humans. As a result, research focused mainly on how plague was maintained within commensal rat populations, and on how it thereof spread to humans.<sup>15</sup> By contrast, contemporary research is more interested in the role played by wild animals and their vectors. Studies of sylvatic plague (that is, plague amongst non-commensal animals) hence focus on the ‘long-term maintenance of natural transmission cycles’ in natural foci of the disease.<sup>16</sup> Whereas recent studies have rekindled scientific interest in questions of the survival of plague in the soil and in soil protozoa, as well as in the possibility of plague being efficiently carried by lice, the consensus remains that fleas (including, potentially, the human flea—*Pulex irritans*) are the only significant vectors of plague within and between different host species.<sup>17</sup> Amongst the latter, the most important as regards the maintenance and transmission of plague in the vast majority of the natural foci of the disease are non-commensal rodents such as marmots, gerbils, and prairie dogs. At the same time, the role of predators such as coyotes and birds of prey, as well as domesticated animals, such as dogs, cats, and camels, in carrying plague bacteria across long distances and in spreading the disease to humans has also come under increasing scientific scrutiny.<sup>18</sup>



Humans may contract plague in three ways: through ‘infectious flea bites, handling infected animals or inhaling infectious materials’, with the disease manifesting itself principally in three distinct clinical forms: bubonic, septicaemic, and pneumonic.<sup>19</sup> Bubonic plague, the most common form of the disease, refers to a plague infection of the body’s lymph nodes, primarily in the loin, armpits, or neck. Usually the infected node is the closest to the entry point of *Yersinia pestis* in the human body.<sup>20</sup> When, as is often the case, lymphadenopathy ensues, the characteristic regional lymphatic oedemas or swellings (commonly called buboes, from the Greek for groin) may develop, acquiring their name from the locality of the symptoms. Groin, armpit, and neck regional lymphatic swellings caused by *Yersinia pestis* are thus referred to as inguinal, axillary, and cervical buboes respectively. The patient suffers from fever, headache, and intense pain in the infected lymph nodes. Death amongst untreated cases has been established to occur in 40–60% of cases.

Although, if diagnosed early, bubonic plague can be treated with antibiotics (reducing fatality cases to approximately 10% in the developed world), in some cases spreading bacteria may lead to another form of the disease: septicaemic plague. In this case, endotoxic shock and disseminated intravascular coagulation usually result in the rapid death of the patient.<sup>21</sup> Rarely, in the absence of an intervening infection of the lymph nodes, primary septicaemic plague occurs through direct contact with an infected source. Symptoms include high fever, chills, headache, vomiting, and diarrhoea as well as bleeding of the skin and gangrene of the limbs and nose. Often the misdiagnosis of primary septicaemic plague leads to mistreatment and rapid death. Death from septicaemic plague occurs in 100% of untreated cases.

Under certain circumstances, plague assumes a pneumonic form. So-called primary pneumonic plague results from the inhalation of infected material, usually from other infected humans, but also from non-human animals, such as cats.<sup>22</sup> So-called secondary pneumonic plague results in cases when, either as a result of bubonic or septicaemic infection, plague bacteria carried by blood cells infect tissues of the lungs, where they multiply.<sup>23</sup> In both cases, the patient becomes a potential source of pneumonic plague infection to human contacts, by sneezing or coughing.<sup>24</sup> As a result, plague infects the lower respiratory tract of the human body immediately, with the lungs being the original and primary site of infection. The incubation of pneumonic plague in humans can be as short as twenty-four hours and as long as four days. The non-specificity of initial symptoms often veils the nature of the disease: fever, chills, headache,

dizziness, body pain, and chest discomfort can easily be seen as signs of flu or the common cold. These last between twenty-four and thirty-six hours, before the second stage of the disease sets in. By the second day in the latest, the infection progresses across the alveola, producing a profusion of sputum.<sup>25</sup> The bloody particles in this sputum have a great concentration of plague bacteria, rendering the patient in this stage highly infectious. By contrast to secondary pneumonic plague, the sputum of primary pneumonic patients is rarely mucopurulent, and as a result spreads profusely in aerosol manner.<sup>26</sup> Meanwhile, the patient (both primary and secondary) suffers a proliferation of bacterial spread throughout his or her lungs, leading to liquefactive necrosis or cavitation, producing characteristic lesions in the lungs.<sup>27</sup> With bacteria spreading through the bloodstream to other organs, the disease reaches its terminal stage and causes cyanosis, with the patient sometimes falling in delirium or coma.<sup>28</sup> Cardiac failure leads to death. Pneumonic plague is treatable by antibiotics if these are administered within a few hours from the first symptoms. Death occurs in approximately 100% of untreated cases as early as twenty-four hours after the initial symptoms.<sup>29</sup>

Although this clinical picture is today universally accepted, there remain areas that are not so clear. Categories such as cutaneous plague and pestis minor, whilst still appearing in textbooks, are rarely employed today.<sup>30</sup> Other clinical categories, such as tonsillar plague, are being currently rediscovered with research promising to clarify their clinical picture.<sup>31</sup> For the historical cases examined in this book the relevant clinical categories are mainly bubonic and pneumonic plague. It must, however, be kept in mind that the specificity and interrelation between the two, as defined by medical science today, were not universally shared at the time of the outbreaks I will be examining here.

### LABORATORY TRANSFORMATIONS OF PLAGUE

Historical studies of the development of plague research and epistemology usually take as their starting point Bruno Latour's highly influential discussion of the discovery of the plague bacillus by Pasteur Institute's Alexandre Yersin in Hong Kong (1894). In a short tract from his book *The Pasteurization of France* Latour analyses the *Annales de l'Institut Pasteur* paper 'The Bubonic Plague at Hong Kong'—the report through which Yersin proclaimed to have isolated the true plague bacillus. Latour defines Yersin's work in Hong Kong, as underlined by three 'displacements':

(a) that ‘he did not treat directly the sick’; (b) that ‘although he was inside the hospital, he was in his laboratory’; (c) that ‘he brought with him his laboratory’.<sup>32</sup> Here, Latour claims, ‘we recognize the true Pasteurian. In the midst of the worst horrors, it was the laboratory that was given first priority.’<sup>33</sup> Reviewing his paper, Latour underlines how Yersin went through clinical symptoms, urban conditions, ethnographic data, class aspects of the epidemic, and comparisons between animal susceptibility to the disease only so as to arrive at what he considered the key to unlocking the mystery of the epidemic: the pathogen. Of course Yersin’s nemesis, the Japanese student of Robert Koch, Kitasato Shibasaburō, had already proclaimed the discovery of the plague bacillus three days in advance of Yersin’s arrival in the Crown Colony.<sup>34</sup> Yet once Yersin was in Hong Kong there was little point in simply admitting he had been overtaken. So, the story goes, braving all obstructions set on his path by the British, he set to rediscover the pathogen. This he did by examining samples from the buboes of corpses, where he discovered not what Kitasato had seen under his microscope, but an altogether different bacillus. But Latour is not so much interested in this well-known story. His focus is with Yersin outside any frame of political, scientific, or international antagonism. He thus provides us with an evocative portrait of the doctor at work: ‘Yersin is interested in the patients who lie there, but in order to capture the microbe he returns immediately to his laboratory. The tumour is no longer a symptom of clinical medicine. It is what must contain the microorganism.’<sup>35</sup> Following Latour, Yersin wanted to ‘reenact plague’ in his lab. As a result, he was not interested in ‘looking for symptoms in the patients in the hospital around him but in the guinea-pigs he deliberately makes sick’.<sup>36</sup> And that is because for Yersin, Latour argues, plague was no longer located between towns, or between the fever and the tumour, but ‘between aniline, Gram’s method and the microscope’.<sup>37</sup> This is why the photographic plates accompanying Yersin’s article ‘show neither the Chinese nor the sores nor the dead nor the rats but the colonies under a microscope’.<sup>38</sup>

In an article published a year before his book on the Pasteurians, Latour had already produced a programmatic reading of the laboratory. Although *Give Me a Laboratory and I Will Raise the World* did not deal with plague, it provided a condensed argument about the importance of the laboratory in the generation of knowledge about infectious diseases. In drawing a dynamic model of the relation between the laboratory and what lies outside it, Latour underlined that what made the transference of anthrax into the laboratory efficacious was the fact that Pasteur brought back only

‘the micro-organism, and not the whole farm, the smell, the cows, the willows along the pond or the farmer’s pretty daughter’.<sup>39</sup> By isolating the microbe, the laboratory allowed Pasteur to perform a radical change of scales from the outside in, reversing the balance of power between humans and the microbe.<sup>40</sup> Latour is careful to note that this was not a simple operation of extending the laboratory, nor was it a mere transformation of the field ‘into the guise of a laboratory’. For, he claims, this ‘metaphorical drift’ rendered the ‘outside/inside notion irrelevant’ as ‘the laboratory positions itself precisely so as to reproduce inside its walls an event that seems to be happening only outside’.<sup>41</sup>

Latour’s ‘new internalism, an internalism in which nothing is left outside’ was later adopted by Andrew Cunningham in his analysis of the importance of the laboratory in ‘transforming plague’.<sup>42</sup> Cunningham thus produced one of the most powerful dictums in the contemporary history of medicine: ‘following the advent of laboratory medicine, infectious diseases are now necessarily and exclusively defined by the laboratory and thus receive their identity from the laboratory’.<sup>43</sup> Making a bold and lasting contribution to historical debates regarding retrospective diagnosis by arguing that pre- and post-1894 identities of plague are incommensurable, Cunningham asserted that plague ‘is defined by—that is, its identity derives from—the question we ask and the activities we undertake in making the identification’.<sup>44</sup> This conclusion came to support Latour’s argument that since the Hong Kong 1894 outbreak the question at hand was no longer clinical but bacteriological. Following this line of thinking, whilst the clinical diagnosis of plague was an act which ‘all it can do is suspect’ the presence of the pathogen as the cause of the disease, the laboratory was what could confirm or establish the latter.<sup>45</sup> Asserting the ‘absolutely crucial’ role of the laboratory, Cunningham claimed that the latter assumed ‘total authority of the authentication of plague’ as ‘the final arbiter of the accuracy of the diagnoses’.<sup>46</sup>

Cunningham’s picture of medical research during the third plague pandemic is hence dominated by the laboratory. It asserts that as of July 1894 the question asked in the process of identifying plague shifted from a concern regarding its identity as a disease or illness to one about the identity of plague as a pathogen—two ontologically and epistemologically distinct categories. Cunningham argued that the new, transformed identity of plague ‘involved, and depended totally on, a new way of thinking and seeing, the laboratory way of thinking and seeing’.<sup>47</sup> Here Cunningham again followed Latour in placing emphasis on the visual aspect of this

identification, as first produced by Kitasato and Yersin in their respective papers in *The Lancet* and the *Annales de l'Institut Pasteur*: 'what was portrayed in these pictures was not the *symptoms*—the patients suffering the disease—but the *microbe*, a thing which could only be seen down the microscope.'<sup>48</sup> Hence, according to this influential reading, the ontological and epistemological shift brought about by the transformation of plague entailed a techno-sensorial rupture that set the space of the laboratory above the space of the clinic or the field, the microscopic lens above the human eye, and a pathogenic binary (yes/no plague) above a hitherto complex diagnostic reasoning regarding the disease.

### THE LIMITATIONS OF LABORATORY-CENTRISM

Though this laboratory-centred approach has had a significant impact on the medical humanities and beyond, it has also been extensively criticised for setting the microbiological laboratory apart, as a privileged sphere or apparatus of knowledge. It is not the aim of this Introduction to summarise the otherwise admirable literature on the dialectic between the laboratory and the field.<sup>49</sup> Nor am I going to outline the debate around the notion that the power of laboratories is based 'solely on the knowledge that [they] project into the world'—that is to say, Robert Kohler's critique that the lab-centred approach is 'a reductive view of a complex social reality' that ignores the laboratory's situation in and usefulness to specific classes and apparatuses of power.<sup>50</sup> Keeping these well-known points in mind, I would like to pose a range of questions specific to plague. Does historical evidence from the third plague pandemic support the Latour-Cunningham thesis that the laboratory identification of plague assumed a diagnostic monopoly? Did the laboratory, to follow Latour's phrase, indeed produce certainty vis-à-vis plague?<sup>51</sup> What was the importance of the laboratory beyond diagnosis, for the broad spectrum of plague research conducted during the course of the pandemic? And did, in fact, a transformation of the key question regarding plague occur, entailing a shift from a concern about it as a disease or illness to a concern about it as a pathogen?

The extent to which the microscope was important in the diagnosis of plague has been recently examined by Sean Hsiang-lin Lei in his work on the Manchurian plague epidemic of 1910–11. Lei draws on Cunningham's mention of the use of the microscope in the course of the particular epidemic in relation to his assertion that the 'laboratory thereafter was and is always crucial'.<sup>52</sup> Lei's analysis of the role of the

microscope in this context marks a major development. It goes beyond Cunningham's reading of Chinese resistance to the laboratory transformation of plague as simply a time-lag, which after the usual reluctance of the less open-minded eventually led to acceptance. Lei underlies that the overcoming of sceptical attitudes towards the microscope was conditioned upon political rather than epistemological premises. It is true, Lei claims, that Wu Liande brought with him a Beck microscope to Harbin, where he had been appointed by the Qing court as head of anti-plague operations in the course of the Manchurian outbreak. It is also true that Wu quickly put his microscope to work, and he thus managed to identify the bacillus exclusively in the lungs of a corpse he was examining. Yet his claim that the epidemic was a contagious form of pneumonic plague was ignored even after local authorities were invited to look down the microscope and see 'the true cause of plague'.<sup>53</sup> This should come as no surprise. Depending on available technologies and skills, a microscope may be an adequate means of visualising a microorganism. It can thus create a visual model of plague against which other samples may be compared. And it can also identify what tissues of the organism this microorganism infects—but it can in no way by itself show how a pathogen is transmitted, and hence how a disease emerges and persists in a given environment or human population. As Jennifer Tucker's work on science and visualisation techniques has shown, it was widely understood by the turn of the century that the microscope and photomicrography could not show 'bacteria in action', and hence could not demonstrate or prove how bacteria caused the disease whose agent they were accepted to be.<sup>54</sup> This inherent technological limitation of microscopy, and not some sort of cultural obstinacy, explains why both Chinese authorities and Western doctors, such as Gerald Mesny, remained unconvinced by Wu's radical theory that the disease was airborne and did not require fleas or rats. Plainly speaking, Wu's method of deduction was not scientifically valid, although its deduced fact was true. It is here that the politics of evidence and scientific proof assumed their true proportion. For, as Lei notes, what the microscopic image could not prove, the death of Dr Mesny did—as it was construed by Wu as a result of the former's refusal to wear a gauze mask (as designed and propagated by Wu) when attending plague patients. At this moment, Lei argues, the Chinese authorities 'did not need the microscope to be convinced that it was a pneumonic plague'.<sup>55</sup>



Illustration 1 Wu Lien-teh in a Harbin Lab. 1911

Did this tragic coincidence then finally establish the microscope as a truth-teller, an indispensable identifier of plague? Lei seems not convinced that such laboratory transformation of plague actually took place on the ground. He notes that whilst microscopes were prescribed in diagnosing the disease during the rest of the outbreak, 'it is hard to believe that the microscope was involved in most of the diagnostic procedures'.<sup>56</sup> Lack of adequate and adequately trained staff, the massive scale of the epidemic, and the state of the corpses found frozen or half-eaten by dogs on the streets and fields are some factors that rendered the 'microscopic test [...] far from the most cost-effective method of identifying plague patients'.<sup>57</sup> In fact, as Lei further notes, even ten years later, during the 1920–21 pneumonic plague outbreak in Manchuria, 'the diagnosis procedure did not require microscopes in every suspected case' but remained based on clinical diagnosis.<sup>58</sup>

The reason for this was not only pragmatic but also epistemological. Doubts about the ability of the microscope to ascertain the cause of disease haunted plague research in the course of the pandemic and, as we will see in the fifth chapter of this book, led Wu Liande himself to controversial conclusions. Perhaps no other work produced during that period demonstrates this troubled relation with the laboratory more clearly than the five volumes of information and reports produced by the Indian Commission on Plague (1898–99).<sup>59</sup> What seemed to underline concerns regarding the laboratory's ability to diagnose plague were the ever-shifting methods and theories regarding the process of bacteriological identification. An indicative example of this is given by Dr James Wallace, who was asked by the Commission if, on condition of being provided with positive laboratory results by a competent bacteriologist, he would be satisfied to accept the diagnosis of a case as suffering from plague. This was his illuminating reply: 'I should be inclined to say that I would accept the bacteriological results with a certain amount of doubt, because bacteriological experiments, and experiences, are altering every day, and while some light would probably come from such an examination, it should be received with caution.'<sup>60</sup> Such scepticism was evinced even by as high a plague authority as the eminent bacteriologist Ernest Hanbury Hankin, who in his interview with the Commission expressed his concern over the validity of microscopic diagnosis. During one of the numerous Indian outbreaks, Hankin had been brought a dead monkey. After performing a post mortem examination he concluded that the animal had died of plague. And yet, noted the perplexed scientist, 'on microscopic observation no trace of



the plague microbe was to be found in any organ of its body'.<sup>61</sup> In its final Report, the Indian Commission on Plague would issue a careful statement regarding the value of microbiological diagnosis. This asserted that the detection of a large number bacteria, 'possessing the morphological characters of the plague bacillus', should be taken as 'conclusive evidence that the patient is suffering from plague'.<sup>62</sup> However, the Commission reserved its judgment in cases of 'a few isolated bacteria arranged together as diplo-coccal forms'.<sup>63</sup> Its concluding remarks also made clear that the absence of plague bacteria should in no way lead to a diagnosis that the patient in hand is not suffering from the disease: 'the non-discovery of such bacteria will not in any way rebut a diagnosis of plague based on clinical symptoms.'<sup>64</sup>

In light of this, it is difficult to sustain Cunningham's aphorism regarding clinical and bacteriological diagnosis, as this applied to the apex of the third plague pandemic: 'all it [clinical diagnosis] can do is suspect. The only way a suspicion of plague can be confirmed or established is "by bacteriological methods"; in other words by a laboratory.'<sup>65</sup> Far from being 'the final arbiter of the accuracy of the diagnoses the physician offers', the laboratory appears to have held a less determining or in fact necessary role.<sup>66</sup> For, as the Indian Plague Commission's verdict makes clear, on the ground of the unfolding pandemic, if a case was clinically diagnosed as suffering from plague no laboratory test was essential. At best the latter could confirm but not contradict the clinical diagnosis of plague. Even Yersin, in his consecutive career as a plague researcher in Indochina, did not always take to the laboratory in order to identify the cause of the disease. In print Yersin did of course maintain that bacteriology was the only valid method of identifying a victim as suffering from plague.<sup>67</sup> And yet, in practice, he adopted a more tacit approach. Soon after his celebrated discovery, in the course of the Nha Trang outbreak (1898), Yersin seemed content to base his diagnosis not on laboratory tests but on the examination of symptoms: we thus see him declaring that a young girl had not been a plague victim, with his diagnosis based not on bacteriological analysis (he did not bother to perform one) but on clinical examination.<sup>68</sup> In light of this attitude, which we see persisting amongst doctors across the globe into the first decades of the twentieth century, it is not at all surprising to find Dr J.M. Swan writing in 1913 from Canton: 'in questionable diagnoses of bubonic plague I have a number of times relied on the facial expression as a deciding point, and it has not failed me.'<sup>69</sup> Rather than being merely the whimsical stance of a sole physician, this reflected a much wider distrust towards laboratory

diagnosis which led doctors and plague researchers to insist on the need for clinical diagnosis in order to verify the cause of illness or death as plague.<sup>70</sup> It would, furthermore, be a mistake to assume that the non-determining power of bacteriological tests was simply the result of inadequate technology or training, soon to be overcome through the general advancement of science. One hundred years after the peak of the third pandemic, in the course of the 1994 pneumonic plague outbreak in Surat, India, the laboratory identification of plague remained a hotly contested apodictic field with laboratory results being challenged by leading scientists.<sup>71</sup> It would take six years for the scientific community to reach a consensus that the Surat outbreak was in fact caused by *Yersinia pestis*.<sup>72</sup> More recently, after several months of media fever over the PCR-based ‘discovery’ of plague bacilli amongst rats in New York’s subway, the scientists behind the study had to admit that their results had been misinterpreted, and that it could not be claimed that Black Death lurked in the microbiome of Manhattan’s extensive underground system.<sup>73</sup> Whereas medical literature often presents such phenomena as related to the ‘remoteness’ of outbreaks (hence reproducing a colonial discourse on the interrelation of place, proximity, and knowledge), the New York City fiasco aptly demonstrates that laboratory failure comprises a constitutive part of plague diagnosis.<sup>74</sup>

The emphasis put on the role of the laboratory in identifying or certifying plague hence tends to ignore the actual diagnostic reality on the ground during the third pandemic and also today. But, more importantly, it also creates a skewed image of plague as an epistemic object contrived solely in the laboratory or with reference to its techniques. Here we should return to Latour and his representation of Alexandre Yersin as someone posited, on the one hand, between the hospital and its patients and, on the other hand, his lab and its guinea-pigs: ‘back in his laboratory Yersin is interested in the patients that lie there, but in order to capture the microbe he returns immediately to his laboratory.’<sup>75</sup> Evocative as it may be, this is an image that lacks historical grounding. It is well known that Yersin never had the opportunity to see patients, or even work in the Kennedy Town Hospital, as he was banished from it by its superintendent, James Alfred Lowson, with access not only to living but also dead victims of plague barred to him.<sup>76</sup> In actual fact, there was never a constitutive Pasteurian choice between the hospital and the laboratory, as Latour imagines it. There was simply a banishment out of the hospital, which left Yersin with no option other than closing himself in his iconic matshed so as to study smuggled corpses.

Equally problematic is Latour's reading of the plates featuring in Yersin's 1894 article, a reading followed closely by Cunningham. As mentioned above, Latour interprets as significant the fact that these were all plates showing 'the colonies under the microscope', rather than 'the Chinese [or] the sores [or] the dead [or] the rats'.<sup>77</sup> It must be, however, noted that at the time the *Annales* did not publish in their pages photographs of patients, let alone pictures of urban scenes during epidemics. Furthermore, the rat had not been established as a host of plague at the time and could not have possibly been the subject of such medical visualisation.<sup>78</sup> More importantly, however, medical and administrative archives contain no photographs of human corpses (or rats) from the Hong Kong outbreak, and only two photographs of patients (in the Glassworks Factory) are known to exist from the outbreak.<sup>79</sup> Contrary to his image as lab-bound, Yersin himself appeared very eager to capture with his camera and include in his diary of the discovery of the bacillus photographs of precisely what Latour considers excluded from his sphere of interest: a Chinese cemetery, the streets of Taipingshan where plague raged in 1894, and some commemorative views of his matshed for his mother.<sup>80</sup> Once again here Latour is mystifying Yersin's agency and decision, ignoring the actual practical limitations on the ground and the politics of representation in place in the course of the outbreak, as well as Yersin's own representational proclivities and aesthetics. In the end, Yersin had little choice but to publish these plates in response to Kitasato having published images of his own bacillus before him in *The Lancet*.<sup>81</sup> Rather than being a breakthrough or a novelty, Yersin's photomicrographic representation was entrenched in a struggle for scientific prestige that was as much personal as institutional, but always already confined in modes of representation that had little to do with Yersin's agency or choice: attitudes more fully expressed instead in the photographic appendix of his diaries.

A work that sheds further light on the limitations of Latour's and Cunningham's approach of the discovery of the plague pathogen is Robert Peckham's recent article on Yersin's work in Hong Kong. In his *Matshed Laboratory: Colonies, Cultures and Bacteriology*, Peckham returns to Yersin so as 'to offer a counterpoint to the growing body of literature that proclaims a laboratory "revolution" and tends to overlook the variety of laboratory spaces, focusing instead on a fundamental epistemic shift'.<sup>82</sup> Peckham examines in detail the construction, use, and discourse around Yersin's makeshift laboratory in its specific, matshed form, within the context of colonial problematisations of native forms

of dwelling. He hence points out that what constituted Yersin's laboratory as a site of knowledge was its complex position in the midst of ever-shifting colonial discourses and policies about 'colonial space, race, and social order'.<sup>83</sup> Crucial to this configuration was Yersin's own attitude towards discovery. Here Peckham draws on Andrew Mendelsohn's examination of young Yersin.<sup>84</sup> According to Mendelsohn, whilst still in Paris, Yersin was powerfully drawn to what he describes as the *flânerie*-exploration-leisure model of knowledge.<sup>85</sup> Influenced by literary works such as Victor Hugo's *Les Misérables*, as well as by new trends in plein-air painting, Yersin sought to know disease in the French capital through urban field excursions: what another eminent Pasteurian, Émile Roux, called '*bacteriologie en plein air*'.<sup>86</sup> There lay the truth of disease, and for the researcher to reach it, he or she had to breach the walls of the laboratory, the study, and the library. Peckham follows Mendelsohn's note that this provided a model of knowing that was not panoptical, in Michel Foucault's sense of the term, but rather ethnographic. He hence underlines what laboratory-centred approaches tend to ignore: Yersin's prolific career as an explorer of the great outdoors and his spring 1894 ethnographic explorations of the Annamese highlands, a few weeks before venturing to Hong Kong.<sup>87</sup>

In light of this reading of Yersin's work and epidemiological perspective, it is important to re-examine Latour's claim that 'microbiology laboratories are one of the few places where the very composition of the social context has been metamorphosed'.<sup>88</sup> Seeing the laboratory as a source of 'new agents', this syllogism raises the former to the status of an evental site: 'fresh sources of power for modifying society and cannot be explained by the state of the society at the time.'<sup>89</sup> Hence Latour is led to articulate his Archimedean 'parody', and to assert that 'in this *moment* the laboratory gains strength to modify the state of affairs of all other actors'.<sup>90</sup> What makes this an evental moment is the reversal of force realised within the walls of the lab.<sup>91</sup> Yet, as we have already seen, nothing could be less true about the way in which the discoverer of the plague bacillus operated and perceived his research; in other words, in terms of what Latour would call Yersin's 'scientific practice'.<sup>92</sup> Mendelsohn and Peckham's examinations of Yersin's work do not simply provide a critique of the idea of the laboratory's fictional 'placelessness', as already developed by Kohler.<sup>93</sup> They also give us a new way of thinking about the laboratory without viewing it as a hegemonic apparatus. Moving beyond the phantasmagoria of the laboratory as a pure evental site, this analysis underlines the 'constantly shifting

relation between laboratory and field, indeed the fundamental “mobility” of the laboratory as it was (re-)configured within different but overlapping dynamic contexts’.<sup>94</sup>

Following this critique, the central premise of this book is that, rather than being ‘*the* interpretation’ of the disease, microscopically identified pathogens (and their photomicrographic image) were simply one aspect of plague epistemology at the turn of the century.<sup>95</sup> As regards what Latour calls ‘scientific practice’, then, bacteriology was important insofar as it identified the pathogen of plague. And yet, the laboratory did not hold some magic key to the macro-problems of plague. It did not decide the nature of plague as a disease any more than it constructed it single-handedly.

The purpose of this book is not to deny tout court the importance of the laboratory in the configuration of plague. There is no doubt that laboratory research played an important role in configuring plague as a distinct medical and epidemiological category, by grace of identifying its pathogen. Yet the epidemiological category of plague as a disease is best conceived as having been generated by relations of relative autonomy between different epistemological spheres. What we must keep from the Latourian thesis is that, in this interchange and struggle between techniques of power and knowledge, the microbe was a means of ‘locomotion for moving through the networks’, inter-constituting the larger scheme of plague epistemology.<sup>96</sup> In order for this complex set of epistemological relations to be understood we need an accurate, balanced, and critical history of the uses and limitations of laboratory research in the study of plague. But we also need to take seriously methods of researching and knowing plague that were not lab-based. It is to this neglected field of plague epistemology that I want to turn my attention in this book, with particular reference to ethnography.

### PLAGUE ETHNOGRAPHIES

Rather than relying exclusively on the laboratory or on bacteriological identification, in the course of the third pandemic plague research was based on complex regimes of knowledge, whose often contradictory evidence was far from reconciled by simply looking down the microscope. These ways of knowing plague, or what, following Sokhieng Au, we may call modes of epidemiological reasoning, involved a wide array of methods, disciplines, and approaches: historical, statistical, demographic, geographic, cartographic, climatological, photographic, and ethnographic.<sup>97</sup>

These methods and techniques of researching and reasoning about plague developed in agonistic dialogue to each other, and not solely or primarily in relation or by reference to the laboratory. The epistemological and strategic interplay between these methods configured and reconfigured plague in the course of the third pandemic. This it did within diverse social, political, and economic contexts. However, the vast majority of areas affected by plague were regions ruled by European empires, predominantly Britain, Russia, and France but also Portugal, Germany, and the Netherlands, where the great bulk of plague research took place.

As historians like Myron Echenberg, David Arnold, Maynard Swanson, and Guenter Risse have demonstrated, the development of plague science during the third pandemic was intertwined with the complex politics and biopolitics of anti-plague measures.<sup>98</sup> And yet, if anti-epidemic policy could be drawn by means of a sovereign decision by imperial powers (albeit within international rules regarding, amongst other things, quarantine), these seldom held such a territorial monopoly when it came to plague research.<sup>99</sup> As early as 1878, that is, before the first major outbreak of the third pandemic, the great European powers had conceded that the eruption of plague in their territory legitimated the presence and operation of extra-territorial medical committees in the area.<sup>100</sup> Hence when plague erupted in India in 1896, the British accepted the presence not only of individual alien doctors, such as Yersin, but entire foreign plague commissions that conducted their own research and drew their own conclusions on British colonial soil.<sup>101</sup> Hence, as the pandemic progressed, an ever more complex network of epistemological approaches to plague came into being, as each great power had its own traditions and proclivities as regards investigative methods (e.g. cartography and medical geography) as well as distinct toolkits of epidemiological reasoning.

And yet, between these diverse imperial scientific trajectories a common baseline research language, with all the trappings of translation, rapidly arose. This it did through the development of common tropes and methods, but also by establishing certain areas of interest that were believed to hold important answers with regard to plague. One of these areas was the social life, habits, and beliefs of populations affected or threatened by the disease. The study of these was thought to hold the key to understanding what conditions allowed the transmission of plague in a particular place, but also what permitted the disease to acquire a seasonal presence or persistence in a given locality. This was a concern that had previously preoccupied the old philosophers of pestilence, underscoring the long,

early nineteenth-century debates about the nature of epidemic disease.<sup>102</sup> Yet as regards plague at the turn of the century, the study of the social and cultural conditions surrounding the disease came to rely primarily not on a bricolage of information gathered by diplomats, travellers, and priests (though, as we will see, this did not cease altogether) but on direct and systematic observation in the field: in other words, through a crude and yet pioneering medical ethnographic approach.<sup>103</sup> Through this study plague researchers aimed in different times and places at diverse objectives. Some of these included the following: to collect data on possible modes of transmission; to reason about or against already alleged infection routes; to identify plague-fostering practices or customs; to inquire about native modes of prophylaxis; and to pin down certain classes, races, or ‘types’ as prone to or resistant to plague.

In many cases the constitution of what we may call, for brevity, ‘plague ethnography’ had an explicit administrative scope: the minimisation of public disorder in the wake of public health intervention. This public order aspect of the collection of ethnographic data regarding plague was especially relevant to places and where the initial, autocratic management of the epidemic had sparked a wave of often violent protests. To give but one well-known example, in the case of India this led to a more collaborationist policy which put in place the implementation of measures in ways that ‘conformed to customary Indian beliefs and practices’.<sup>104</sup> Hence investigating and evaluating attitudes towards anti-plague measures such as corpse inspections or removing the roofs of infected buildings became a priority for the Indian Plague Commission.<sup>105</sup> Equally important was the study of how rumours about plague were generated and spread amongst the colonised, the reasons for people to conceal plague cases, responses to segregation, and medical treatments.<sup>106</sup> In some cases, this strategy of pacification led colonial authorities to allow practitioners of non-Western medical systems to operate as supervisors in plague hospitals and camps, though in some occasions, such as Hong Kong, this was to lead to new aspects and levels of strife.<sup>107</sup>

And yet, while it is true that, in general, ‘governmentality [...] assigned to science a pastoral influence in the regulation of colonial affairs’, it must be kept in mind that the ways in which socio-cultural practices and approaches of disease were studied in the context of each different outbreak, as well as the conclusions drawn from these studies, often reflected diversified colonial strategies.<sup>108</sup> Indeed such studies often provided the platform for the expression and development of political and personal antagonisms within

and between medical experts and colonial administrators. It was hence not uncommon for the interpretation and evaluation of an alleged native practice vis-à-vis plague to become the point of heated colonial debate, with different interpretations of these practices and beliefs employed in ever-shifting fashions according to the political and social situation.<sup>109</sup>

Given that this was an operation unfolding for the most part under colonial auspices, one is not surprised to find that the majority of plague ethnography represented colonised societies stricken with the disease ‘as ignorant, and mired in negative cultural practices’, hence reinforcing the configuration of the notion of the ‘native’ as one linked to ignorance, superstition, and resistance to change.<sup>110</sup> This is what we can call, following Melissa Leach and Barry Hewlett’s work on hemorrhagic fever in Africa, the negative representational strategy of plague ethnography.<sup>111</sup> On the heels of a well-trodden path of representations of others in European medical writing, it led to a large body of work on supposedly insanitary living conditions and habits of colonised populations. Within the framework of colonial control, and following developments regarding the problematisation of other infectious diseases, it was a narrative that attributed responsibility for spreading disease to beliefs and practices singled out for abolition or reform, leading to such interventions as medical policing, medical education campaigns, and infrastructural change.<sup>112</sup>

During the third plague pandemic, this strategy witnessed wide application, especially as regards areas like Hong Kong and India, which came under direct colonial rule.<sup>113</sup> In the ‘Index of Evidence’ of the five-volume work by the Indian Plague Commission we find a list of over thirty such ‘habits of the people affecting liability to plague’.<sup>114</sup> These allegedly harmful practices included mundane behaviours such as walking barefoot (since the Hong Kong outbreak in 1894, plague, believed to be soil-borne, was thought to infect walkers through cuts on their soles) and sleeping on the floor, but also ritual aspects of social life.<sup>115</sup> Prominently amongst the latter figured death ceremonies and rituals. From Bombay, where the Muslim practice of placing a coin on the mouth during funerals was blamed for spreading plague amongst women, to Senegal, where Sereer funerary rites became the *bête noire* of French anti-plague efforts, to Madagascar, where the suspicion arose amongst Pasteurians that reinterment practices (*famadihana*) spread plague, native funerary rites unnerved colonial authorities.<sup>116</sup>

Often this negative mode of representation was associated with efforts to generate or reproduce specific anthropological typologies, which in turn



served the symbolic and political economy of colonialism at large.<sup>117</sup> An indicative example of this technique may be glimpsed in the work of the famous French colonial army doctor, Vincent Rouffiandis, and his interest in Chinese ideas and practices regarding plague. Rouffiandis's outbreak narrative is underlined by a story related 'in all seriousness' to him by 'a well educated, and very intelligent Chinese man' in Fuzhou.<sup>118</sup> This tells of a man who was returning home at midnight when he realised he was being followed by two dark figures. After seeing a paper falling out of the pocket of one of his pursuers, the man picked it up only so as to read written therein the names of all the people who would be stricken with plague in the next three days. Seeing his own name included, he tore the part of the paper where the latter was inscribed. Soon after, the two figures returned to retrieve the paper, which the man handed back to them. During the next three days all the listed individuals died one after another from plague, and 'only he escaped the disease'.<sup>119</sup> Rouffiandis commented he was happy to relate the story without a comment, as its moral appeared self-evident; a clear demonstration of the Chinese 'character'. Such was also the aim of relating another story mentioned to him by the secretary of the viceroy of Fujian. From the reverie of night-time Chinese streets in the first narrative, this second tale transports us to the surface of the operating table, where an odd discovery was made: the open bubo on a woman suffering from plague was observed to contain a gutless living rat.<sup>120</sup> Assuming a reassuring tone, Rouffiandis is quick to explain that, in fact, this must have been no more than a cyst containing odd cutaneous forms, nails, etc., which just so happened to look like a rat, 'with the help of imagination'. 'For the latter', he concludes, underlining once again the issue of so-called native character, 'is fertile in strange concepts amongst the Chinese'.<sup>121</sup> What we have here by way of a self-confessed ethnographic interest in Chinese ideas about plague is no less than an attempt to establish the basic truth about the 'native character' in question. It comes as no surprise, given the wider post-Opium Wars colonial discursive context, that in the case of the Chinese this anthropological type was contrived as a fatalist character, lacking the spirit of inquiry and unable to be precise or exacting.<sup>122</sup> In other words, a character lost in the mists of its opium pipe, ancestor veneration, fatalism, and demonic superstition: traits which, in the eyes of European colonialists, had led the once-glorious Chinese Empire to dismal decline.

The dialectic between colonial modernity and native tradition (seen as little more than stagnation) was an important axis for the constitution

of this negative mode of representation. What was crucial to this project was conjuring evidence in support of the alleged fact that the colonised failed to grasp what Ruth Rogaski has called hygienic modernity.<sup>123</sup> In other words, evidence in support of the narrative that, in spite of all the proper knowledge and means at their disposal (made available through colonialism), they insisted to confront plague in premodern ways. This discourse reached its quintessential form in medical narratives depicting native subjects as abusing modern anti-plague inventions. An example of this is evident in the way in which during the 1910–11 plague outbreak in Manchuria the Cambridge-educated Wu Liande decried the need to stamp plague masks with temple seals so that the working-class inhabitants of Harbin would be convinced to wear them: not for their anti-bacterial properties but as magic amulets against plague.<sup>124</sup> Mixing ritual with science, or using the finest fruits of the latter for occult purposes, was a constant source of colonial anxiety at the turn of the century. For whilst European and American fancies of capturing ghosts with the photographic lens could, to some extent, be tolerated, even entertained, when it came to native subjects employing equally creative *retournements* of scientific media, this was seen as a profanation of reason itself.<sup>125</sup> What could be more obscene, in colonial eyes, than the inhabitants of Fuzhou using carbolic acid in the course of anti-plague idol processions? This is how the scene was related by Dr Kinnear for *The China Medical Missionary Journal*: ‘the man carrying the rear end of the pole used one hand to dip a bunch of twigs into the solution, then, giving them a swing, sent a teaspoonful of this diluted antiseptic in the direction of the front of each house.’<sup>126</sup> The extreme pole of this negative mode of representation consisted in the identification of specific ethnic groups or social classes as essential elements in the spread of plague. This strategy of configuring ethnic and class vectors of plague infamously coined the latter a Chinese disease in Honolulu and San Francisco or, as we will see in the fifth chapter of this book, a disease of Chinese ‘coolies’ in Manchuria.<sup>127</sup>

This mode of plague ethnography, focusing on notions of tradition and culture so as to explain the spread and persistence of the disease in a specific locus as a result of native backwardness, was not, however, a colonial monologue. In many cases, colonised subjects responded to this representational strategy, contesting its interpretive framework and the overall epidemiological reasoning behind it. A historically neglected yet significant example of this important dialectic is evident in the case of Hong Kong’s long epidemic years following the initial 1894 plague outbreak. With

plague returning to the Crown Colony year after year, and the rat coming into focus as a host of the disease, colonial authorities began taking draconian measures in order to halt the annual outbreaks. One of these measures involved the evacuation and disinfection of houses adjacent to premises where plague-infected rats had been discovered. Implemented for the first time in 1903, this measure led to an explosion of an already in situ practice amongst Chinese residents of Kowloon and Victoria: the dumping of human corpses, dead by plague, on the streets and harbour of Hong Kong.<sup>128</sup> The practice, seen as both morally vile and as a source of further infection, came under colonial scrutiny, with the administration blaming no less than the ‘Chinese character’ for the phenomenon, which soon accounted for up to one in three plague deaths in the colony.<sup>129</sup> Yet this opinion did not go uncontested. Chinese elites, and the leaders of charitable societies such as the Tung Wah hospital, openly debated the validity of colonial readings of body dumping, arguing for a radically different interpretation.<sup>130</sup> In a series of letters to the Sanitary Board the illustrious Chinese merchant and politician Lau Chu-pak (himself a member of the Board) argued that body dumping was a violation of Chinese custom and mores. In order to understand why Chinese families abandoned all norms of filial piety and abandoned their kin on the streets of the city, Lau argued, one had to see colonial anti-plague measures as what they really were: extremely violent, unreasonably harsh, and ultimately socially counter-productive policies. In his correspondence Lau turned the tables on the colonisers, by providing vivid ethnographic images of disinfecting operations that, he argued, struck such terror in Chinese residents that they resorted to what was a profoundly un-Chinese practice. Hence where colonial officers had employed the phenomenon of body dumping as a salient example of Chinese hygienic backwardness, Lau used it as a platform for contesting what we could call colonial hygienic terror and its anomic impact on Chinese society.

Yet things were not as simple as a dialectic between colonial accusatory practices and native resistance. For at the antipodes of, yet in synergy with, the negative representational strategy described above, stood a minoritarian but no less important narrative that valorised native beliefs and practices. Karen Brown’s work has been on the forefront of efforts to approach native knowledge as a partner in shaping scientific ideas in colonial contexts. The Oxford historian has demonstrated how, with regards to plant toxicology and livestock management, ‘scientists analysed and recorded both popular knowledge and their own interpretation of

disease'.<sup>131</sup> This was a mode of representation with a long history in medical writing. However, by the end of the nineteenth century, disease exotica, like the observations made by John Jackson on the supposed anti-plague properties of Tunisian olive oil, would give way to a systematic study of potentially beneficial native knowledge or practices.<sup>132</sup> As Brown argues in her examination of livestock trypanosomosis in Zululand, native ideas about the disease 'w[ere] to have an important influence on early scientific inquiries into the disease, illustrating how western science evolved from a range of sources, not just developments in the metropole'.<sup>133</sup> Such a systematic collection of data about beneficial aspects of 'native tradition' as regards plague is equally evident in the work of the Indian Commission on Plague. Rather than being an idle repetition of reported ideas, the Commission's affirmative strategy involved the active valorisation of given aspects of cultural and social life of the colonised. An example that makes the stakes of this narrative clear is the question of fakirs. This, in colonial terms, was a 'type' of ritual practitioners that in many ways condensed what the British found more fascinating and loathsome about India.<sup>134</sup> As regards plague, long before the importation of the disease to Bombay from Hong Kong in 1896, Garhwali fakirs were suspected of spreading the disease across Northern India during their twelve-yearly Nassik pilgrimage.<sup>135</sup> And yet the Commission was ready to dismiss this long-standing theory (and administrative anti-fakir bias) and endorse the exonerating testimonies of Lt Colonel Hay and Major Hyde-Cates, which stated that due to their open air, peripatetic lifestyle, fakirs were in fact immune to plague. As a result of the supposed fact that their resistance to the disease had to do with their mode of living rather than with some inherent biological trait, it was assumed that this cultural immunity to plague should also be acknowledged as an advantage of all wandering peoples in the British Raj.<sup>136</sup>

Whereas one may be tempted to place emphasis on the affirmative, dialogical aspect of this valorisation of 'native traditions', Leach and Hewlett alert us towards a more critical perspective. Examining postcolonial epidemiological practices, their critique focuses on the contribution of anthropology in 'elucidating and re-valuing local cultural models of disease and framings of system dynamics, and on identifying valuable, health-enhancing local knowledge and cultural categories which can be blended productively with scientific knowledge'.<sup>137</sup> The two anthropologists are mainly concerned with the policy implications of this narrative on native culture. Yet they also offer an epistemological reading critical of

the operation of translating non-Western rationalities, social relations, and institutions into ‘cultural models’ compatible with and profitable to biomedical understandings of public health. This affirmative narrative, they claim, following Byron Good’s Morgan Lectures, results in ‘perpetuat[ing] a particular notion of “culture” as confined to local settings; the impression is that rural Africans have culture, while people and institutions in more globally linked settings do not’.<sup>138</sup> From this perspective, we should approach the valorisation of peripatetic lifestyles in British India, and other similar affirmative representational strategies, in tandem with the negative or denigrating strategies used by colonial authorities at the time. Rather than forming two separate modes of approaching native others, these in fact formed a single colonial apparatus of capture.

A useful example of this entanglement can be found in the writings of the French colonial doctor Charles Broquet on plague in Guangzhouwan, a South Chinese town, in 1902.<sup>139</sup> As Florence Bretelle-Establet has argued, Broquet supported the idea that the Chinese took to fleeing from infected localities once plague cases occurred—a habit which, he believed, was beneficial as a plague prophylactic.<sup>140</sup> Yet at the same time, Broquet tells us that the locals also believed the disease to be a punishment brought upon them by their imperial Ming ancestors: a retribution for not rising against the Manchu usurpers of the throne. Hence, at the same time as praising fleeing as an effective measure against plague, Broquet maintained an anthropological typology that posited the Chinese as decisively premodern and unable to distinguish between utility and superstition, truth and fable. What is important here, from both a historical and an anthropological perspective, is to examine the ways in which these two strategies were entangled in generating ‘folk models’ of disease, leading to the objectification of ‘native tradition/culture’.

### GNOSIOLOGICAL ENCLOSURES

Writing on medicine in colonial India, historians like Mark Harrison and Niels Brimnes have stressed that the valorisation and denigration of non-Western medical or therapeutic practices cannot be simply mapped chronologically as subsequent stages in the European encounter with and subjugation of non-Western societies.<sup>141</sup> As Brimnes notes, it is imperative ‘to distinguish between different layers in the European discourse about non-European medicine’.<sup>142</sup> Hence, in his examination of smallpox vaccination in India, he sees early nineteenth-century British perceptions of

non-Western medical systems as ‘a reservoir of potentially useful knowledge’ as an impediment to ‘the vision of colonial medicine as part of a civilizing mission’.<sup>143</sup> This, Brimnes claims, would be overcome only with the reorganisation of colonial power around the new notion of natives as ignorant others in the second half of the century. We should be careful here, however, not to assume that the colonial valorisation of non-Western medical knowledge or practices was simply a philosophical relic of the Enlightenment. If by the midst of the nineteenth century figures like Claude Bernard showed interest in Amerindian uses of curare, this was no longer because they saw indigenous users of the poison as ‘naked philosophers’, but rather because they treated them as raw resources to be plundered both on a material and on what we today would call an informational level.<sup>144</sup>

As Philippe Descola has noted, in the course of the Enlightenment non-Western societies ‘were used as foils to civic morality or as models of now-vanished virtues’.<sup>145</sup> This mode of othering (exemplified in Montaigne’s work) was based on a complex genealogy of ideas about the noble savage: a figure that, as Dan Edektein has demonstrated, witnessed significant transformations as a model of political morality in the hands of opposing philosophical schools and social classes in the course of the seventeenth and eighteenth centuries.<sup>146</sup> By contrast, when not dismissed as mired in ignorance and superstition, at the time of high colonialism (but often enough in the postcolonial era also) native subjects were seen as ‘proto-naturalists, quasi historians and nascent economists: in short, precursors who fumble at a way of apprehending things and human beings that we ourselves are believed to have discovered and codified better than anyone else’.<sup>147</sup> Whereas in the course of the Enlightenment ‘noble savages’ were imagined as morally and epistemically autonomous enclaves of a lost Golden Age, by the late nineteenth century colonised societies had come to be seen as possessing ‘primitive’ forms of knowledge. In other words, forms of knowledge that needed to be understood from a social evolutionary perspective: as a ‘prefiguration’ or ‘residue of positive knowledge’.<sup>148</sup>

In these terms, the valorisation of non-Western medical systems and of native medical practitioners as ‘incipient scientists’ formed part of a much broader operation of gnosiological primitive accumulation that sought to sever and objectify what was considered potentially useful or profitable, whilst at the same time discarding its social and ritual context as foolish or harmful superstition.<sup>149</sup> This operation, Descola argues, has

been accompanied by a 'taxonomic mania for picking out specialized fields of inquiry that are given the name of a recognized science preceded by the prefix ethno-', such as ethnobotany or ethnomedicine.<sup>150</sup> No longer interested in native subjects as paradigms of some sort of lost wholeness or integrity, this has been a long-standing operation with profound implications for the way in which Western societies approach others as well as themselves.

First, this was a process that facilitated a particular form of objectification by 'mak[ing] it possible to reify certain blocks of native knowledge by dint of rendering them compatible with the modern division of sciences'.<sup>151</sup> It was, in other words, what we may call an operation of ontological enclosure in that, as we will see in detail in the third chapter of this book, it rendered diverse practices and systems of knowledge compatible with what, from the Empire's point of view, was the true scientific understanding of the physical world: as a world separate from and pre-existing culture awaiting to be known, classified, and controlled by humans. In this way what Descola has described heuristically as the naturalist ontology of scientific thought assumed the guise of an autonomous principle awaiting its universal realisation under the guidance of colonialism's civilising mission.

Second, this process also reified 'native tradition' in the Marxist sense of the term. The operation of selecting, severing, appropriating, instrumentalising, and reintegrating fragments of non-Western medical knowledge was underscored by a process that transformed them into alienated commodities, as we can still see in the much-contested field of patenting, especially relevant to ethnobotany, today.<sup>152</sup> More than simply using native knowledge to extract raw material that could be transformed into marketable products, however, this was a process of alienation of the native knowledge in question from its plane of immanence. On the one hand, it generated a commodity out of a particular morsel of knowledge, which could be traded and circulated in the broader network of what Warwick Anderson has called 'biocolonial exchange'.<sup>153</sup> And, on the other hand, it created a spectacle of native culture as a pool of customs, knowledge, skills, and aptitudes destined either to be refined and improved scientifically, to be rendered into marketable folk items, techniques, and stories, or to perish on their way towards a socially evolved modernity and civilisation.

## LOCAL EPIDEMICS, GLOBAL EPIDEMIOLOGY: THE SCOPE OF ANTHROPOLOGICAL CRITIQUE

At the same time as being a part of an apparatus of capture vis-à-vis colonised societies, plague ethnography was implicated in the broader biopolitical operation of configuring new concepts of epidemicity and endemicity: ideas about disease transmission, models of the relation between human and non-human hosts of the disease, and, overall, novel modes of epidemiological reasoning about plague in what quickly shaped into a global science of the disease. From this perspective, Sujit Sivasundaram's maxim that 'considering global processes makes it possible to understand how "Western science" became "Western science" and how "indigenous knowledge" became "indigenous knowledge"' is pivotal to the understanding of the ethnographic configuration of plague at the turn of the nineteenth century.<sup>154</sup>

Focusing on this key epistemological entanglement, the case examined in this book consists in plague research conducted on the Chinese-Russian frontier between 1894 and 1923 (the principal period of outbreaks in the region), a corpus of work of unique global importance as regards the definition of two significant aspects of the disease: its sylvatic, zoonotic origins, and its pneumonic clinical form and airborne mode of transmission. The area under examination comprises roughly what is today the north-western corner of Heilongjiang province and the north-eastern corner of the Inner Mongolia Autonomous Region of China, together with the southern reaches of the Transbaikalia Province of Russia, in South Siberia, and the north-eastern corner of the Mongolian Republic's Dornod province. This is a region where plague is believed to be endemic, forming one of the most important natural foci of the disease on the planet.<sup>155</sup> As of the mid-1890s, outbreaks of plague started to come to the attention of medical and lay authorities in the region. Although scientific verdict is still pending, these initially limited outbreaks as well as the major epidemics of pneumonic plague that followed appear to be epidemiologically distinct from the third plague pandemic. In other words, they appear not to have originated (at least in the short term) from the Yunnan sylvatic reservoir of the bacillus, believed to be the source of the third pandemic. And yet, at the time, these outbreaks were perceived and treated as part of the pandemic in its lethal course across the globe. Hence from a political and social as well as epistemological perspective they were indeed part of the third plague pandemic. As a result, plague-related knowledge and epidemiological



reasoning produced in the course of these local and regional outbreaks formed part of what we may call third plague pandemic epistemology and should be treated as an unalienable part of it.

As already mentioned, the importance of medical and epidemiological research on the north-east Chinese-Russian frontier for the global science of plague consisted in elucidating two crucial aspects: (a) the sylvatic zoonotic origins of plague, and (b) the pneumonic and airborne form of the disease. In examining the role of ethnography in how medical experts came to reason about these and other aspects of plague, my purpose is not to draw a history of the disease, of the measures against it in the region, or of plague science in Russia and China in general. Such histories would have to dwell on administrative, political, and social aspects of the outbreaks, as well as investigate in depth anti-plague policies and local and subaltern reactions to and perceptions of them and the disease as a whole. These areas remain outside the scope of this book, whose aim is to examine the ways in which medical experts studied and interpreted lay perceptions of the disease and its hosts or vectors in the region, as well as the ways in which the former used ethnographic data to construct wider arguments about plague and reason about various aspects of its epidemiology. This then is a historical anthropological analysis of the use of ethnography and ethnographic data by medical experts in a particular time and region of the world, in the context of the third plague pandemic, and their contribution to the formation of a global science of plague. It hence dwells on a long tradition of anthropology not simply as an analytical but also as a critical discipline.

In the second chapter of the book, I employ the tools of anthropological critique in the analysis of the first efforts to record native perceptions of plague north of the Chinese-Russian border. I describe how Russian doctors stationed in the region produced the first accounts of an alleged traditional knowledge of plague amongst Mongols and Buryats. Examining these studies closely, I enquire what ethnomethodological processes and fallacies were involved in generating what I call the native knowledge hypothesis. In the third chapter, I expand my anthropological critique of medical interpretations of Mongol and Buryat practices and beliefs. I focus on ways in which international medical experts sought to interpret native myths and rituals as ways of preventing plague. This, I argue, was based on what Mary Douglas has called medical materialism, a reductionist method of explaining away complex symbolic, cosmological, and ritual systems as means of securing public and personal hygiene. The fourth

chapter of the book examines another crucial way in which ethnographic data about plague were employed. Drawing on Marta Hanson's notion of the geographic imagination of epidemics, I argue that the ethnographic configuration of plague worked hand in hand with a geographic problematisation of the disease. Aiming to know the geographic origin of plague infection, and eager to identify the trajectory and the endemic foci of the disease, medical experts drew on and manipulated ethnographic data so as to generate geographic models of plague transmission. The fifth chapter of the book examines the ways in which plague ethnography was employed in the context of the Manchurian pneumonic plague epidemic of 1910–11. By examining the writings of China's leading plague expert, Wu Liande, I argue that ethnographic data were manipulated so as to generate a binary anthropological typology. This typology, on the one hand, lauded native Mongol and Buryat hunters as holders of ancient traditions of plague-prevention, and, on the other hand, accused Chinese migrant workers, so-called 'coolies', of being ignorant and inept, a 'floating population' responsible for spreading plague. The sixth chapter also relates to how in the course of the 1910–11 epidemic the hypothesis that plague in the region originated in Siberian marmots, and the hypothesis that native hunters possessed knowledge of the disease (and how to prevent it), became epistemologically clustered. I explore the consequences of this cluster in the aftermath of the great Manchurian epidemic and examine its impact on the epidemiological reasoning of Wu Liande during his reign as director of the North Manchurian Plague Prevention Service, China's first comprehensive epidemiological apparatus. Finally, the overarching questions about how ethnographic data are rendered into epidemiological evidence, and what are the epistemological effects and distortions resulting from this operation both in colonial times and today, are explored in the conclusion of the book.

It is the objective of this monograph to show that far from validating the old saying of Sir Thomas Maitland that 'the plague, was the plague, the whole plague, and nothing but the plague', research following the bacteriological identification of its pathogen generated multiple, transient, contradictory, and enduringly messy identities of the disease.<sup>156</sup> As current debates in the medical press indicate, this process of configuring plague is still ongoing and as yielding in thought-provoking ideas today as it was in 1894.<sup>157</sup> What is more, as recently seen in the cases of the 2003 SARS and 2014 Ebola outbreaks, the ethnographic configuration of infectious disease and epidemics is an ongoing operation with significant consequences

for both the understanding of zoonotic diseases, and for global health approaches of populations at risk. This analysis of a key progenitor of such approaches, at the dawn of the age of epidemiology, aspires to throw critical light on what is often taken as simply a benevolent, social scientific contribution to global health approaches of epidemic crises.

## NOTES

1. The year 1894 saw the first bacteriologically confirmed case of plague in Hong Kong, from where the disease spread to the rest of the world. The pandemic is believed to have originated in a series of regional epidemics of plague between forty and one hundred years earlier in the Chinese south-western province of Yunnan (Benedict 1996). The spread of the disease from the Yunnan highlands, where it is believed to be endemic, to the plains and to the rest of South China is often attributed to the social and economic disturbance caused by the Panthay Rebellion (1856–1873).
2. A. Yersin, ‘La Peste Bubonique a Hong Kong,’ *Annales de l’Institut Pasteur*, vol. 8 (1894): 662–667.
3. Fl. Bretelle-Establet & Fr. Keck, ‘Les épidémies entre “Occident” et “Orient.”’ *Extrême-Orient, Extrême-Occident*, vol. 37 (2014), p. 11; Echenberg (2007) notes the impact of this idea as regards the perception of Porto, Portugal, where plague broke out in 1899, as the gateway for the return of the Black Death in Europe.
4. M.J. Echenberg, *Plague Ports: The Global Urban Impact of Bubonic Plague, 1894–1901* (New York: New York University Press, 2007); R. Peckham, ‘Infective Economies: Empire, Panic and the Business of Disease,’ *The Journal of Imperial and Commonwealth History*, vol. 41, no. 2 (April 2013): 211–237; D. Arnold, ‘Disease, Rumour and Panic in India’s Plague and Influenza Epidemics, 1896–1919.’ In R. Peckham (ed.) *Empires of Panic: Epidemics and Colonial Anxieties*, pp. 111–130 (Hong Kong: Hong Kong University Press, 2015).
5. This corpus includes the following monographs: C.A. Benedict, *Bubonic Plague in Nineteenth-Century China* (Stanford: Stanford University Press, 1996); M. Chase, *The Barbary Plague: The Black Death in Victorian San Francisco* (London: Random House, 2004); Echenberg, *Plague Ports*; M.J. Echenberg, *Black Death, White Medicine: Bubonic Plague and the Politics of Public Health in Colonial Senegal, 1914–1945* (Oxford: James Curry, 2002); J.C. Mohr, *Plague and Fire: Battling Black Death and the 1900 Burning of Honolulu’s Chinatown* (Oxford: Oxford University Press, 2004); C.F. Nathan, *Plague Prevention and Politics in Manchuria 1910–1931* (Cambridge: Harvard East Asian Monographs, 1967); G.B. Risse,

- Plague, Fear, and Politics in San Francisco's Chinatown* (Baltimore: Johns Hopkins University Press, 2012); W.C. Summers, *The Great Manchurian Plague of 1910–1911: The Geopolitics of an Epidemic Disease* (New Haven: Yale University Press, 2012).
6. W. Beinart, K. Brown & D. Gilfoyle, 'Experts and Expertise in Colonial Africa Reconsidered: Science and the Interpenetration of Knowledge,' *African Affairs*, vol. 108, no. 432 (July 2009), p. 424.
  7. G. Gooday, 'Placing or Replacing the Laboratory in the History of Science?,' *Isis*, vol. 99, no. 4 (December 2008): 783–795. As 'modern plague', historians and epidemiologists define the third plague pandemic and its aftermath until our days.
  8. Centres for Disease Control and Prevention, 'Maps and Statistics: Plague in the United States, Plague Worldwide.' Available at: <http://www.cdc.gov/plague/maps/>.
  9. B. Lee Ligon, 'Plague: A Review of its History and Potential as a Biological Weapon,' *Seminars in Pediatric Infectious Diseases*, vol. 17, no. 3 (July 2006): 161–170.
  10. See especially the papers in the inaugural issue of *The Medieval Globe*, edited by Monica H. Green (2014).
  11. An indication of rapid changes in plague science may be glimpsed in a recent paper by Gonzalez et al. (2015), which challenges prevalent models of how *Yersinia pestis* reaches the lymph nodes.
  12. K.L. Gage & M.Y. Kosoy, 'Natural History of Plague: Perspectives from More than a Century of Research,' *Annual Review of Entomology*, vol. 50 (2005): 508–509.
  13. Gage & Kosoy, 'Natural History of Plague'; S. Davis, M. Begon, L. De Bruyn, V.S. Ageyev, N. Klassovskiy, S.B. Pole, H. Viljugrein, N. Chr. Stenseth, & H. Leirs, 'Predictive Thresholds for Plague in Kazakhstan,' *Science*, vol. 307 (2004): 736–738; S. Davis, P. Trapman, H. Leirs, M. Begon & J.A.P. Heesterbeek, 'The Abundance Threshold for Plague as a Critical Perlocation Phenomenon,' *Nature*, vol. 454 (2008): 634–637; D.J. McCauley, D.J. Salkeld, H.S. Young, R. Makundi, R. Dirzo R, R.P. Eckerlin, E.F. Lambin, L. Gaffikin, M. Barry, K.M. Helgen, 'Effects of land use on plague (*Yersinia pestis*) activity in rodents in Tanzania,' *American Journal of Tropical Medicine and Hygiene*, vol. 92 (2015): 776–783; Lei Xu, B.V. Schmid, Jun Liu, Xiaoyan Si, N. Chr. Stenseth & Zhibin Zhang, 'The Trophic Response of Two Different Rodent-Vector-Plague Systems to Climate Change,' *Proceedings of the Royal Society Biological Sciences*, vol. 282, no. 1800 (2014): 2014 1846; Ben Ari, T., S. Neerincx, K.L. Gage, K. Kreppel, A. Laudisoit, H. Leirs, N. Chr. Stenseth, 'Plague and Climate: Scales Matter,' *PLoS Pathogens*, vol. 7, no. 9 (September 2011): e1002160. For a critique of the role of host and vector abundance see Brinkerhoff et al. (2010).

14. S.K. Collinge, W.C. Johnson, C. Ray, R. Matchett, J. Grensten, J.F. Cully Jr., K.L. Gage, M.Y. Kosoy, J.E. Loye, A.P. Martin, 'Landscape Structure and Plague Occurrence in Black-tailed Prairie Dogs on Grasslands of the Western USA,' *Landscape Ecology*, vol. 20, no. 8 (December 2005): 941–955; Wilschuta, L.I., E.A. Addinka, J.A.P. Heesterbeek, V.M. Dubyanskiy, S.A. Davise, A. Laudisoitf, M. Begon, L.A. Burdelov, B.B. Atshabar, S.M. de Jonga, 'Mapping the Distribution of the Main Host for Plague in a Complex Landscape in Kazakhstan: An object-based approach using SPOT-5 XS, Landsat 7 ETM+, SRTM and multiple Random Forests,' *International Journal of Applied Earth Observation and Geoinformation*, vol. 23 (August 2013): 81–94. For a more general discussion on plague's landscape ecology see Gage and Kosoy (2006).
15. For a definitive synopsis of this line of work see Pollitzer (1954).
16. K.L. Gage & M.Y. Kosoy, 'Recent Trends in Plague Ecology,' *USG Survey* (2006), p. 218.
17. S. Ayyadurai, L. Houhamdi, H. Lepidi, C. Nappiez, D. Raoult & M. Drancourt, 'Long-term Persistence of Virulent *Yersinia pestis* in Soil,' *Microbiology*, vol. 154 (2008): 2865–2871; R.J. Eisen, J.M. Petersen, C.L. Higgins, D. Wong, C.E. Levy, P.S. Mead, M.E. Schriefen, K.S. Griffith, K.L. Gage & C.B. Beard, 'Persistence of *Yersinia pestis* in Soil under Natural Conditions,' *Emerging Infectious Diseases*, vol. 14, no. 6 (June 2008): 941–943; J. Ratovonjato, M. Rajerison, Soanandrasana Rahelinirina & S. Boyer, 'Yersinia pestis in *Pulex irritans* Fleas during Plague Outbreak, Madagascar,' *Emerging Infectious Diseases*, vol. 20, no. 8 (August 2014): 1414–1415; S. Ayyadurai, Fl. Sebbane, D. Raoult & M. Drancourt, 'Body Lice, *Yersinia pestis* Orientalis, and Black Death,' *Emerging Infectious Diseases*, vol. 16, no. 5 (May 2010): 892–893. It should be noted that whereas, since Bacot and Martin's classic study of the development of plague in the Oriental rat flea (*X. cheopis*) (1914), it has been assumed that only flea species which manifest a blockage of the preventriculus (as a result of being infected with plague) are efficient transmitters of the disease, recent studies have suggested that other modes of transmission (such as early phase transmission) may also play a significant role in the spread of plague, hence implicating more flea species than traditionally believed to be efficient transmitters (Eisen et al. 2015; Gage 2012; Webb et al. 2006).
18. On camels see note 31; on cats see note 22; on canines see Chap. 2, note 43.
19. K.L. Gage, 'Factors Affecting the Spread and Maintenance of Plague.' In A.M.P. de Almeida & N.C. Leal (eds) *Advances in Yersinia Research*, pp. 79–94 (New York: Springer, 2012), p. 79.
20. All clinical data, unless otherwise stated, are derived from the CDC and WHO plague factsheets: <http://www.cdc.gov/plague/symptoms/> <http://www.who.int/entity/mediacentre/factsheets/fs267/en/>.

21. D.L. Erickson & B.J. Hinnebusch, 'Pneumonic Plague.' In B. Anderson, H. Friedman & M. Bendinelli (eds), *Microorganisms and Bioterrorism, Infectious Diseases and Pathogenesis* (New York: Springer, 2006), p. 157.
22. J.M. Doll, P.S. Zeitz, P. Ettestad, A.L. Bucholtz, T. Davis & K.L. Gage, 'Cat-Transmitted Fatal Pneumonic Plague in a Person who Traveled from Colorado to Arizona,' *American Journal of Tropical Medicine and Hygiene*, vol. 51, no. 1 (July 1994): 109–114.
23. D.L. Erickson & B.J. Hinnebusch, 'Pneumonic Plague,' p. 157.
24. D.L. Erickson & B.J. Hinnebusch, 'Pneumonic Plague,' p. 157.
25. D.L. Erickson & B.J. Hinnebusch, 'Pneumonic Plague,' p. 158.
26. D.L. Erickson & B.J. Hinnebusch, 'Pneumonic Plague,' p. 158.
27. D.L. Erickson & B.J. Hinnebusch, 'Pneumonic Plague,' p. 158.
28. The former was observed to occur *en masse* amongst infected Chinese soldiers during the 1920–21 Manchurian plague epidemic (Wu 1926).
29. See also Kool (2005) and Levison (2000).
30. Other categories have been altogether abolished. For example, Roufflandis (1905) claimed there were four principal forms of plague: bubonic, cutaneous, pulmonary, and intestinal.
31. A.A. Bin Saeed, N.A. Al-Hamdan, R.E. Fontaine, 'Plague from Eating Raw Camel Liver,' *Emerging Infectious Diseases*, vol. 11 (2005): 1456–1457; A. Arabia, S. Kharabsheh, S. Al-Azab, M. Al-Kayed, Z.S. Amr, M. Abu Baker & M.C. Chu, 'A 12-Case Outbreak of Pharyngeal Plague Following the Consumption of Camel Meat, in North-Eastern Jordan,' *Annals of Tropical Medicine and Parasitology*, vol. 99, no. 8 (December 2005): 789–793.
32. Br. Latour, *The Pasteurization of France* (Cambridge MA: Harvard University Press, 1988), p. 95.
33. Latour, *The Pasteurization of France*, p. 95.
34. S. Kitasato, 'The Bacillus of Bubonic Plague,' *The Lancet*, vol. 144, no. 3704 (August 25 1894): 428–430.
35. Latour, *The Pasteurization of France*, p. 95.
36. Latour, *The Pasteurization of France*, p. 99.
37. Latour, *The Pasteurization of France*, p. 96.
38. Latour, *The Pasteurization of France*, p. 98.
39. Br. Latour, 'Give me a Laboratory and I will Raise the World.' In K. D. Knorr-Cetina & M. Mulkay (eds), *Science Observed: Perspectives on the Social Study of Science*, pp. 141–169 (London: Sage Publications 1983), p. 146.
40. Latour, 'Give me a Laboratory and I will Raise the World,' p. 147.
41. Latour, 'Give me a Laboratory and I will Raise the World,' p. 154.
42. Br. Latour, 'The Costly Ghastly Kitchen.' In A. Cunningham & P. Williams (eds), *The Laboratory Revolution in Medicine*, pp. 295–303 (Cambridge: Cambridge University Press, 1992), p. 303.

43. A. Cunningham, 'Transforming Plague, The Laboratory and the Identity of Infectious Disease.' In A. Cunningham & P. Williams (eds), *The Laboratory Revolution in Medicine*, pp. 209–244 (Cambridge: Cambridge University Press, 1992), p. 209.
44. Cunningham, 'Transforming Plague,' pp. 242, 213. See also Cunningham (2002).
45. Cunningham, 'Transforming Plague,' p. 213. Here Cunningham follows Latour in taking the laboratory as 'a specific place to produce facts'; see Latour (1992: 299).
46. Cunningham, 'Transforming Plague,' pp. 218, 219.
47. Cunningham, 'Transforming Plague,' p. 224.
48. Cunningham, 'Transforming Plague,' p. 234; emphasis in the original.
49. R.E. Kohler, *Landscapes and Labscapes; Exploring the Lab-Field Border in Biology* (Chicago: The University of Chicago Press, 2002); Eugene Cittadino, *Nature as the Laboratory: Darwinian Plant Ecology in the German Empire, 1800–1900* (Cambridge: Cambridge University Press, 1990); Gooday, 'Placing or Replacing the Laboratory in the History of Science?'
50. R.E. Kohler, 'Lab History: Reflections,' *Isis*, vol. 99, no. 4 (December 2008), p. 767. For an early critique of Latour's lab see Schaffer (1991).
51. Br. Latour, *Pandora's Hope: Essays on the Reality of Science Studies* (Cambridge MA: Harvard University Press, 1999), p. 30.
52. Cunningham, 'Transforming Plague,' p. 236. It should be noted here that Cunningham follows Wu Liande's memoirs from 1959, so as to assert the unique importance of the laboratory. Self-staging his lab coat-clad image as a 'plague fighter' before a Western audience, through this autobiography Wu claimed a central place in the modernisation of China (Wu 1959).
53. S.H.-L. Lei, 'Sovereignty and the Microscope: Constituting Notifiable Infectious Disease and Containing the Manchurian Plague (1910–11).' In A.K. Ch. Leung & C. Furth (eds), *Health and Hygiene in Chinese East Asia: Policies and Publics in the Long Twentieth Century*, pp. 73–106 (Durham: Duke University Press 2011), pp. 78–79. For an expanded analysis see Lei (2014).
54. Tucker, J. *Nature Exposed: Photography as Eyewitness in Victorian Science* (Baltimore: Johns Hopkins University Press, 2013), p. 182.
55. Lei, 'Sovereignty and the Microscope,' p. 80.
56. Lei, 'Sovereignty and the Microscope,' p. 81.
57. Lei, 'Sovereignty and the Microscope,' p. 81.
58. Lei, 'Sovereignty and the Microscope,' pp. 81–82.
59. For a history of the Commission see Catanach (1988).
60. House of Commons Parliamentary Papers [Cd.139], p. 291.
61. House of Commons Parliamentary Papers [Cd.140], p. 2.
62. House of Commons Parliamentary Papers [Cd. 810], p. 61.

63. It should be noted that much of this reservation could be attributed to the long and controversial debate unfolding over the diagnosis of plague in Calcutta by William J. Simpson in 1896. For a contemporary discussion of this case see Crake (1908).
64. House of Commons Parliamentary Papers [Cd. 810], p. 61.
65. Cunningham, 'Transforming Plague,' p. 213.
66. Cunningham, 'Transforming Plague,' p. 219.
67. A. Yersin 'L'Epidemie de Peste à Nha-Trang,' *Annales d'hygiène et de médecine coloniales*, no. 2 (1899): 373–385.
68. S. Au, *Mixed Medicines: Health and Culture in French Colonial Cambodia* (Chicago: Chicago University Press, 2010), p. 43.
69. J.M. Swan 'Some Features of Medical Practice not Sufficiently Emphasised in Books,' *The China Medical Journal*, vol. 27, no. 6 (November 1913), p. 391. Swan claimed the defining facial features of plague to be 'a restless, rather dull, staring expression of the eyes, somewhat drawn features, anxiety, often a quivering of the lips' (1913: 392).
70. Noel Bernard & R. Pons, 'Recherches sur les formes anormales de la peste au Cambodge et en Conchichine,' *Bulletin de la Société de pathologie exotique et de ses filiales de l'Ouest-Africain*, vol. 17 (1924): 55–84.
71. N.S. Deodhar, V.L. Yemul, K. Banerjee, 'Plague that Never Was: A Review of the Alleged Plague Outbreaks in India in 1994,' *Journal of Public Health Policy*, vol. 19 (1998): 185–199; S. Kumar, 'Confirmation of Indian Plague Outbreak?,' *Lancet*, vol. 345, no. 8947 (1995), p. 443.
72. A.K. Dutt, R. Akhtar & M. McVeigh, 'Surat Plague of 1994 Re-Examined,' *Southeast Asian Journal of Tropical Medicine and Public Health*, vol. 37, no. 4 (July 2006): 755–760.
73. For press coverage of the original 'findings' see Hotz (2015a); for coverage of recanting on the original 'findings' see Hotz (2015b).
74. For an example of the remoteness narrative see Bertherat et al. (2011).
75. Latour, *The Pasteurization of France*, p. 97.
76. T. Solomon, 'Hong Kong, 1894: The Role of James A. Lowson in the Controversial Discovery of the Plague Bacillus,' *The Lancet*, vol. 350, no. 9070 (July 5 1997): 59–62; Latour (1988: 95) notes this parenthetically, with this datum having no impact on his argument.
77. Latour, *The Pasteurization of France*, p. 98.
78. Yersin speculated that rats may carry plague, but he also believed flies and the soil carried and might spread the disease. The first photographs of rats from the third plague pandemic come from Sydney in 1900 (New South Wales State Library, PXE 90—PXE 95).
79. My current project is in the process of collecting an exhaustive database of photographs of the third plague pandemic (*Visual Representations of the*



- Third Plague Pandemic*); the information stated here is valid to the point of completing this monograph (September 2015).
80. *Archives de l'Institut Pasteur* [YER.6—A1/13]. On medical photography's entanglement with modes of visual commemoration see Kelly (2016).
  81. S. Kitasato, 'The Bacillus of Bubonic Plague'; D.J. Bibbel & T.E. Chen, 'Diagnosis of Plague: an Analysis of the Yersin-Kitasato Controversy,' *Bacteriological Reviews*, vol. 40, no. 3 (September 1976): 633–651.
  82. R. Peckham, 'Matshed Laboratory: Colonies, Cultures, and Bacteriology.' In R. Peckham & D.M. Pomfret (eds), *Imperial Contagions: Medicine, Hygiene, and Cultures of Planning in Asia*, pp. 123–147 (Hong Kong: Hong Kong University Press, 2013): 124–125.
  83. Peckham, 'Matshed Laboratory: Colonies, Cultures, and Bacteriology,' p. 132.
  84. J.A. Mendelsohn, 'The Microscopist of Modern Life,' *Osiris*, vol. 18, no. 1 (February 2003): 150–170.
  85. Peckham, 'Matshed Laboratory: Colonies, Cultures and Bacteriology,' p. 164.
  86. Mendelsohn, 'The Microscopist of Modern Life,' p. 154.
  87. E.T. Jennings, *Dalat and the Making and Undoing of French Indochina* (Berkeley: University of California Press, 2011).
  88. Latour, 'Give me a Laboratory and I will Raise the World,' p. 158.
  89. Latour, 'Give me a Laboratory and I will Raise the World,' p. 158.
  90. Latour, 'Give me a Laboratory and I will Raise the World,' p. 154.
  91. Latour, 'Give me a Laboratory and I will Raise the World,' p. 164.
  92. Latour, *Pandora's Hope*.
  93. Kohler, 'Lab History: Reflections.'
  94. Peckham, 'Matshed Laboratory: Colonies, Cultures and Bacteriology,' p. 144.
  95. Latour, 'Give me a Laboratory and I will Raise the World,' p. 155; emphasis in the original.
  96. Latour, *The Pasteurization of France*, p. 45.
  97. Au, *Mixed Medicines: Health and Culture in French Colonial Cambodia*.
  98. Echenberg, *Black Death, White Medicine*; D. Arnold, 'Touching the Body: Perspectives on the Indian Plague, 1896–1900,' *Subaltern Studies*, vol. V (1987): 55–90; M.W. Swanson, 'The Sanitation Syndrome: Bubonic Plague and Urban Native Policy in the Cape Colony, 1900–1909,' *The Journal of African History*, vol. 18, no. 3 (1977): 387–410; Risse, *Plague, Fear, and Politics in San Francisco's Chinatown*.
  99. For recent studies of the question of quarantine, colonialism, and international relations in the nineteenth century see Barnes (2014), Bashford (2004), Bezio (2013), and Harrison (2013).

100. H. Heilbronner, 'The Russian plague of 1878–79,' *Slavic Review*, vol. 21, no. 1 (March 1962): 89–112. See, in Chap. 2 of this book, the case of Vetlianka. This international exchange would be further institutionalised in the course of the tenth International Sanitary Conference (1897) held in Venice; see Proust (1897) and Howard-Jones (1975).
101. See, for example, the report of the German Commission: G.T.A. Gaffky & G.M.M.J. Sticker, *Bericht über die Tätigkeit der zur Erforschung der Pest im Jahre 1897 nach Indien entsandten Kommission* (Berlin: Springer, 1899).
102. The dissertations on contagion by Aiton (1832) are often based on such information so as to debunk 'aerial' or 'atmospheric' theories of disease.
103. The term ethnography is here to be understood not in its strict, anthropological meaning, but in the wider sense of a more or less systematic field-based study of the social life and beliefs of a given human group over a period of time. In the majority of cases, these studies consisted in a combination of longer-term information gathering, such as in the case of missionaries, and shorter-term, purpose-targeted collection of data, such as in the case of scientific expeditions.
104. Arnold, 'Touching the Body,' p. 82. See also Klein (1988), Mukharji (2012), and Sriraman (2013). See also Sarkar (2011).
105. Arnold (1987: 66–67) also touches upon this issue. Not mentioned by Arnold, however, are the concluding remarks of the Commission's Report, which state that 'there is nothing in the evidence which have been placed before us to show that there is anything in the practice of corpse inspection which is generally repugnant to native opinion' (House of Commons Parliamentary Papers [Cd. 810], p. 500).
106. On the subject of rumours in the case of India see Arnold (1987: 68–77) and Chandavarkar (1992). For a study of the impact of plague and panic in industrial relations in India see Sarkar (2014). For a discussion of plague and rumours in Hong Kong and Cape Town, and in Colombia, see Sutphen (1997b) and Valderrama (2001).
107. I.J. Catanach, 'Plague and the Tensions of Empire: India 1896–1897.' In David Arnold (ed.) *Imperial Medicine and Indigenous Societies* pp. 149–171 (Manchester: Manchester University Press, 1988), p. 157. See also Benedict (1996) for a discussion of British-Chinese biopolitical dynamics during the 1894 Hong Kong outbreak and colonial strategies around the issue of hospitals.
108. R. McLeod, 'Introduction,' *Osiris*, vol. 15 Nature and Empire: Science and the Colonial Enterprise (2000), p. 10.
109. The case of opium being allegedly smoked by the Chinese in order to prevent plague is a striking example. This ethnographic datum initially underlined a conflict between colonial medical officers in Hong Kong (Hong Kong Government [Gazette GA 1895 no.146]). It was then adopted, during the 1911 plague outbreak, by Chinese doctors as evidence of the culpability of

- ‘coolies’ in the Manchurian plague outbreak of 1911 (Wu 1912). It consequently acquired a nationalist anti-Japanese hue in the hands of the North Manchurian Plague Prevention Service in Republican China (Wu 1923a), and was finally used in campaigns against opium-trafficking cartels (Wu 1925; Millott Severn 1925).
110. M. Leach & B.S. Hewlett, ‘Haemorrhagic Fevers in Africa: Narratives, Politics and Pathways.’ In S. Dry and M. Leach (eds) *Epidemics; Science, Governance and Social Justice* (London: Earthscan, 2010), p. 54.
  111. See also Leach (2008). This narrative was employed for colonised populations, as well as for the working classes in Europe and America, including large groups of immigrant communities, such as the Chinese in Honolulu and San Francisco.
  112. Starting with the cases examined in Arnold (1988), there is a growing literature on this issue as regards diseases and medical issues other than plague (e.g. Anderson 2006; Beinart et al. 2009; Bhattacharya 2012; Bhattacharya et al. 2005; Buckingham 2002; Echenberg 2011; Lynteris 2011; Lyons 1992; Stern 2006).
  113. For the purposes of this introduction I will mainly limit myself to examples from China and India; the third plague pandemic affected over 100 locations across the globe and a comparative survey across these with regards to such strategies is not within the scope of this study.
  114. House of Commons Parliamentary Papers [Cd. 809], p. 51.
  115. House of Commons Parliamentary Papers [Cd. 810], p. 143. For a discussion on cementing floors as an anti-plague measure see Ramanna (2004).
  116. House of Commons Parliamentary Papers [Cd. 810], p. 121; Echenberg, *Black Death, White Medicine*, p. 169; E.R. Brigoo & A. Dodin, ‘Étude du comportement du bacilli de Yersin en milieu tellurique à Madagascar,’ *Archives d’Institut Pasteur de Madagascar*, vol. 34, no. 1 (1965): 9–18. For a persistence of medical accusations of *famadihana* as contributing to the spread of plague, see Andrianaivoarimanana et al. (2013). On the question of the persistence of plague in corpses in general see Calmette (1912).
  117. For discussion on tropical medicine and the question of racial types see Cell (1986) and Anderson (1992).
  118. I am using the term ‘outbreak narrative’ throughout this book in the sense developed by Wald (2008).
  119. M. Rouffiandis, ‘Théories Chinoises sur la peste,’ *Annales d’hygiène et de médecine coloniales*, no. 6 (1903), p. 343.
  120. Rouffiandis, ‘Théories Chinoises sur la peste,’ p. 344.
  121. Rouffiandis, ‘Théories Chinoises sur la peste,’ p. 345.
  122. Gauducheau (1911: 25), a French colonial military doctor, writing on the hygiene of the Chinese in Canton, notes: ‘the Chinese on the contrary do not use but minimally these means of artificial protection. Their spirit is not

- as inquisitive as ours, they are not preoccupied by the future to the extent that we are, and prevention seems to them a pointless burden.’
123. R. Rogaski, *Hygienic Modernity; Meanings of Health and Disease in Treaty-Port China* (Berkeley: University of California Press, 2004).
  124. For detailed discussion on this see Lynteris (2013a).
  125. On the contest over the scientific value of spirit photography see Tucker (2013) and Smith (2013).
  126. E. Kinnear, ‘Propitiating the Plague Spirits,’ *The China Medical Missionary Journal*, vol. 26, no. 4 (October 1902), p. 204.
  127. N. Shah, *Contagious Divides: Epidemics and Race in San Francisco’s Chinatown* (Berkeley: University of California Press, 2001); R. Barde, ‘Prelude to the Plague: Public Health and Politics at America’s Pacific Gateway, 1899,’ *Journal of the History of Medicine and Allied Sciences*, vol. 58, no. 2 (April 2003): 153–186; Risse, *Plague, Fear, and Politics in San Francisco’s Chinatown*; Mohr, *Plague and Fire*.
  128. J.M. Atkinson, *A Historical Survey of Plague in Hong Kong since its Outbreak in 1894* (Hong Kong, n. p. 1907); for discussion see Lei (2011).
  129. For this debate, and Lau’s letters, see Hong Kong Public Records Office [HKRS203-1-27]. For broader discussion on how subaltern subjects, including native officers and medical practitioners, often tapped colonial antagonisms as well as colonial configurations of native systems of medical knowledge to their advantage see Flint (2001). For papers discussing the contestation of Western medicine in different colonial contexts see Cunningham and Andrews (1997).
  130. For discussion of the history of the Tung Wah Hospital, including its role in the 1894 plague outbreak, see Benedict (1996), Ma (2011), and Sinn (1989).
  131. K. Brown, ‘Poisonous Plants, Pastoral Knowledge and Perceptions of Environmental Change in South Africa, c. 1880–1940,’ *Environment and History*, vol. 13, no. 3 (August 2007), p. 310. See also: Brown et al. (2013).
  132. J. Jackson, *Reflexions on the Commerce of the Mediterranean Deduced from Actual Experience During a Residence on Both Shores of the Mediterranean Sea* (London: W. Clarke & Sons, 1804). For a wider discussion on the alleged plague-preventative properties of olive oil see Aiton (1832).
  133. K. Brown, ‘From Ubombo to Mkhuzi: Disease, Colonial Science and the Control of nagana (livestock trypanosomosis) in Zululand, South Africa, c1894–1955,’ *Journal of the History of Medicine and Allied Sciences*, vol. 63, no. 3 (July 2008), p. 294.
  134. R. Siebenga, ‘Colonial India’s “Fanatical Fakirs” and their Popular Representations,’ *History and Anthropology*, vol. 23, no. 4 (2012): 445–466.
  135. For discussion see Hankin (1905).

136. House of Commons Parliamentary Papers [Cd. 810], p. 460.
137. Leach & Hewlett, 'Haemorrhagic Fevers in Africa,' p. 57.
138. Leach & Hewlett, 'Haemorrhagic Fevers in Africa'; B.J. Good, *Medicine, Rationality and Experience: An Anthropological Perspective* (Cambridge: Cambridge University Press, 1997).
139. Ch. Broquet, *Un foyer de peste bubonique dans la Chine méridionale* (Paris: Jean Gainche, 1902).
140. Fl. Bretelle-Establet, 'From Extending French Colonial Control to Safeguarding National Prestige: The French Medical Dispensaries in Southern China.' In Iris Borowy (ed.) *Uneasy Encounters: The Politics of Medicine and Health in China 1900–1937*, pp. 63–92 (Frankfurt am Main: Peter Lang, 2009). Confirming the same practice in the area, Etienne Tardif (1902) linked it to a supposed association of plague to rats.
141. M. Harrison, 'Medicine and Orientalism.' In B. Pati & M. Harrison (eds) *Health, Medicine and Empire. Perspectives on Colonial India*, pp. 50–54 (New Delhi: Orient Longman, 2011); N. Brimnes, 'The Sympathizing Heart and the Healing Hand: Smallpox Prevention and Medical Benevolence in Early Colonial South India'. In H. Fischer-Tiné & M. Mann (eds) *Colonialism as Civilizing Mission. Cultural Ideology in British India*, pp. 191–204 (London: Anthem Press, 2004).
142. Brimnes, 'The Sympathizing Heart and the Healing Hand,' p. 194.
143. Brimnes, 'The Sympathizing Heart and the Healing Hand,' p. 194.
144. On Claude Bernard and curare, see Barbara (2008) and Napier (2002).
145. Ph. Descola, *Beyond Nature and Culture* (Chicago: Chicago University Press, 2013), p. 81.
146. D. Edelstein, *The Terror of Natural Right: Republicanism, the Cult of Nature and the French Revolution* (Chicago: Chicago University Press, 2009).
147. Descola, *Beyond Nature and Culture*, p. 81.
148. Descola, *Beyond Nature and Culture*, p. 82. Tilley (2010: 114) places this transformation roughly after 1860 whence 'inquiries into "primitive knowledge" came to be more carefully defined and systematized'.
149. Mary Kingsley, quoted in Tilley (2010: 117).
150. Descola, *Beyond Nature and Culture*, p. 82.
151. Descola, *Beyond Nature and Culture*, pp. 82–83.
152. H. Tilley, 'Global Histories, Vernacular Science, and African Genealogies; or, Is the History of Science Ready for the World?,' *Isis*, vol. 101, no. 1 (March 2010), p. 116.
153. W. Anderson, 'The Possession of Kuru: Medical Science and Biocolonial Exchange,' *Comparative Studies in Society and History*, vol. 42, no. 4 (October 2000): 713–744.

154. S. Sivasundaram, 'Introduction, Focus: Global Histories of Science,' *Isis*, vol. 101, no. 1 (March 2010), p. 96.
155. Regarding the wider area being the natural focus where *Yersinia pestis* evolved from *Yersinia pseudotuberculosis* approximately 10,000 years ago see Suntsov and Suntsova (2006). For a phenotypical study challenging this theory see Kiefer et al. (2012).
156. Sir Thomas Maitland, 'Dispatch to the Right Honourable Earl Bathurst, K. G., dated Corfu 8th April 1819, on the Subject of Plague,' *The London Medical and Physical Journal*, vol. 54 (July–December 1825), p. 122.
157. See in particular the debate between Wagner et al. (2014), Green et al. (2014), and Drancourt and Raoult (2014) regarding the origins of *Yersinia pestis* and the three pandemics in the October 2014 issue of *The Lancet Infectious Diseases*. See also Little (2011).

## The Native Knowledge Hypothesis

The first ethnographic observations on plague in South Siberia date from the second half of the nineteenth century within a context of increasing Russian interest in epidemics. This was initially fuelled by the dramatic events of 1770–71, when bubonic plague broke out during the Turkish-Russian War. What was at first an illness amongst Russian soldiers in the Crimean front soon appeared further north, devastating large areas of European Russia, before attacking Moscow itself and causing the infamous Plague Riot (September 15–17, 1771).<sup>1</sup> This acute crisis rendered the relation between public health and public order a kernel of imperial reconstruction, on the basis no longer of mere subjects but of a population, which could be scientifically known and controlled by the imperial state.<sup>2</sup>

The 1770–71 epidemic linked this new, biopolitical aspect of state power to a problematisation of the imperial frontier as a sphere of danger to Russian sovereignty. This danger was conceived in terms of a Tatar invasion of an altogether different kind from the one playing a prominent role in the classical imperial imaginary. As Dmitry Mikhel has noted, during the events of 1770–71 the association of plague to ethnically identified enemies of the Russian Empire from the south-east reflected itself at the highest ranks of power.<sup>3</sup> This is evident in the correspondence of Catherine the Great with her philosopher-protégé, Voltaire, where the Empress faulted the Crimean Tatars for introducing the disease to Russia proper.<sup>4</sup> These events formed a fertile ground for the development of an Imperial Russian version of what Marta Hanson has called the geographic imagination

of epidemics.<sup>5</sup> On the basis of its imagined proximity to nature, the south-eastern imperial frontier was rendered a zone of hygienic threat contributing to its status as a prime area for Imperial Russia's golden age of scientific exploration.

### THE GREAT SIBERIAN EXPEDITION

The Great Siberian Expedition, organised by the Russian Geographical Society, was a pivotal step in the integration of imperialist and scientific interests as regards the exploration of the south-eastern reaches of the Empire. Mark Bassin has provided a concise history of the Society and its inspiration by pan-Slavism and messianic populism in Russia during the 1840s and 1850s.<sup>6</sup> In light of his analysis, the Great Siberian Expedition must be seen as far more than a mapping of flora and fauna or a profiling of indigenous subjects of the Tsar. In effect, the expedition constituted Transbaikalia as a geographical and ethnological entity in Russian imperial imagination. Comprising of the lands south, south-west, and south-east of the great Baikal Lake, it was rendered legible as a region populated by various animals, plants, and human societies, whose enclosure within the imperial project could be achieved only by scientific means.

A prominent figure in the expedition was the naturalist Gustav Ferdinand Richard Radde, one of the most influential explorers of mid-nineteenth-century Russia. His contribution to science was celebrated by a startling spectrum of individuals, including the anarchist prince and geographer Piotr Kropotkin, who eventually wrote an obituary to the man who provided us with the first scientific observations on what we today recognise as a principal host of plague in the region: the Siberian marmot.<sup>7</sup> Radde's 1857 expedition to East Siberia lasted five years and covered a wide area, including Dauria, the Eastern Sayan Mountains, a large portion of the Amur River, and Buryatia. His role in the expedition was not without controversy. Back in Moscow, he was accused as anti-Russian because of his supposedly 'pure science' perspective—a deplorable sin against proper, patriotic science.<sup>8</sup> What was really distinctive, however, was Radde's commitment to the study of socio-cultural customs and ways of life alongside more naturalist research. It is to this approach that we owe the first scientific examination of the Siberian marmot, as well as the first ethnographic notes on human interactions with the animal in the region.

Providing the taxonomic appellation *Arctomys bobac*, Radde dedicated several pages to the particular marmot species.<sup>9</sup> Although no mention



of a disease amongst its kind was made, the publication of his report in German was pivotal in providing a first, scientifically sanctioned link between the animal and native hunters. As the key ethnographic passage is often alluded to but never fully cited in contemporary literature, it is worth including it here in full:

The pagan hunters, who were very familiar with the bobac's habits, it being for them a basic source of food, and who we can certainly believe as simple and uninfluenced observers, certify that in the summer the bobacs grind leaves of grass between the upper side of their arm and the front side of their belly so as to soften them and then use them in order to support their nests. They also say that the really deep sleep of the bobacs begins only in December and that when one unearths them at the end of autumn without smoking them out one can never hand-handle them, as since the start of digging and hitting the ground with a pick-axe, the animal hears the shovelling and digs from within its nest new burrows, so that, fast as it is, all effort to capture it is wasted. This is why the Tungus, once they reach the end of the plug sealing the winter abode, so as to pierce through it, light a fire with the help of wet manure from which the smoke is directed into the nest and suffocates its inhabitants. They do the same in springtime, a little before the time comes for bobacs to leave their nests. The pagan hunters thus rest only during the three months during which bobacs lay motionless, as they say, in deep sleep.<sup>10</sup>

Though Radde also provided some interesting ethnographic snippets on native marmot-related beliefs—which I will examine in the next chapter—no connection was made between the animal and any sort of disease in the area, or of any native perception of danger harboured by it, medical or otherwise.<sup>11</sup> Rather than arising out of the growing body of scientific work in the region, the first mention of a disease amongst Siberian marmots was made in 1865 by a Russian hunter. In his published memoirs Cherkassov dedicated many pages to what, following the local name for the animal, he called the *tarbagan*.<sup>12</sup> Though he dwelt extensively on the habits of the animal, as well as on native ways of hunting it, Cherkassov made no more than a passing reference to a disease infesting it: 'there are years when natives stop eating *tarbagan*, due to rampant disease in the latter; they are dying like flies and many natives, reckless enough to feed on roasted marmots, have paid with their lives.'<sup>13</sup> In spite of the popularity of the book at the time, there is no indication that either geographers or medical scientists took immediate notice of Cherkassov's mention of human infection by way of eating marmot meat during large-scale epizootics. Instead, what came to function as a

trigger for the systematic study of plague in the south-eastern regions of the Russian Empire was an event that took place many thousands of miles away to the west, at the mouth of the Volga, in 1878–79.

### THE VETLIANKA PLAGUE

With its epicentre at the small fishing village of Vetlianka, some forty-five miles north of Astrakhan, the outbreak, a mixed epidemic of bubonic and pneumonic plague, functioned as an ideal terrain for the exercise of imperial medicine.<sup>14</sup> The disease first appeared in October 1878, although it would be officially acknowledged only in late December, when it had already killed a large part of the inhabitants of the village as well as medical staff trying to help patients.<sup>15</sup> News about the events in Vetlianka swiftly spread to St Petersburg. Still the Tsar appeared aloof, in spite of the fact that the medical board he had summoned ominously declared the disease to be plague.<sup>16</sup> What soon dampened the monarch's cool was the unexpected geopolitical character assumed by the crisis, when Otto von Bismarck forged an anti-pestilence alliance with Habsburg Austria in response to news from the Volga. The strict quarantine measures imposed on travellers coming from the Russian Empire forced the Tsar to abandon his Olympian calm. On January 29, 1879, the Committee of Ministers was summoned and the acting Minister of Interior proposed draconian measures aimed at containing the outbreak: 'that all settlements in which the disease had appeared be razed to the ground, that the healthy population be resettled, and the ill be placed under strict quarantine'.<sup>17</sup> Meanwhile, German, British, Austrian, French, Ottoman, and Romanian medical commissions were invited to inspect anti-plague measures in the area, in an attempt to turn Vetlianka from a national embarrassment to an international showcase of Russian sanitary resolve and responsibility. The deployment of hundreds of troops and Cossacks and the state of emergency imposed on the region continued for many months.<sup>18</sup>

Largely because of its international political implications, the Vetlianka epidemic had a fundamental impact on the relation between the Russian monarchy and its south frontier subjects. The outbreak, which left 400 dead, functioned as a catalyst for the geographic imagination of epidemics in Russia. On the one hand, it reinforced the perception of plague as a frontier issue. Deciphering the exact route of importation proved a subject of heated international debate. Some doctors claimed that the disease derived from Persia, where an epidemic had recently ravaged Resht on

the Caspian shore.<sup>19</sup> Others, like the illustrious Professor August Hirsch, argued instead that it had been unwittingly brought over by Cossack troops engaged in the Russian-Turkish war near Kars, from where they somehow procured silk dresses derived from plague-infected Baghdad for their wives.<sup>20</sup> On the other hand, a third origins hypothesis implicated not cross-border importation but culpable frontier populations. According to this theory, the epidemic was of ‘spontaneous’ origin. This was a problematisation that involved both indigenous ways of life (Kalmyk and ‘Kirgiz’, on either side of the Volga) and the daily practices of Russian settlers along the banks of the great river. Of particular interest to both Russian and international doctors was the prolific fish-curing industry that dominated the regional economy. Watagas, big salt brine vats, had a ‘detestable reputation’, with their presence in the midst of Vetlianka seen as a possible source of pestilence.<sup>21</sup> This was due to the smells emanating from these vats: ‘in Summer a Wataga in full activity poisons the air for hundreds of yards around so that in descending the river the existence of one of the factories is recognized by the nose long before it is in sight.’<sup>22</sup> Be it imported or native, most doctors agreed that local living conditions were congenial to plague, with the British medical delegate Joseph Frank Payne claiming that ‘the habits of the people, though more cleanly than has been represented, assisted in maintaining a putrid atmosphere’.<sup>23</sup>

Hence, situated at a crucial geographic threshold between both Russia and the East, and Russia and its colonial subjects, Vetlianka and the wider area of what was at the time known as the ‘Kirgiz Steppes’ attracted the attention of pioneers of germ theory in Russia.<sup>24</sup> And at the same time, it provided ideal grounds for those proponents of social medicine whose interest lay less with hunting microbes, and more with the eradication of supposedly pestilential habits among indigenous populations. The combination of these two strands of research and problematisation, one microbiological and the other sociological and ethnographic, whilst still embryonic in 1878–79, formed the basis for rendering the south-eastern frontier and its populations a scientifically intelligible and controllable sphere of the Russian Empire.

## PLAGUE IN TRANSBAIKALIA

Within the context of growing concerns with frontier and cross-border epidemics, the first reports of outbreaks also made their appearance far to the east, in Transbaikalia. In October 1888 at Barakhol, twenty-seven miles

from the village of Kulusutai, five Buryat herders were reported dead in a single yurt (later said to have eaten a tarbagan). In the absence of a district physician, Dr Ashman, a medical doctor stationed at the hospital of the nearby military base of Aksha, was summoned to the focus of the infection alongside his feldscher, Yudin.<sup>25</sup> Examining the two corpses, they judged the disease to be typhus (a common misdiagnosis of pneumonic plague, also employed early on with regards to the Vetlianka outbreak). Two days later, however, on October 19, 1888, both Yudin and Ashman developed axillary buboes, symptoms generally understood at the time as signs of plague; they both succumbed to the disease within twenty-four hours.<sup>26</sup>

The next three years saw an increased number of incidents in Transbaikalia, especially focused around the Cossack settlement of Suktui, a few miles north of Aksha. On August 2, 1889, following a reported marmot epizootic, Evdokia, the fifteen-year-old daughter of Abram Epov, a Cossack hunter, died after suffering from high fever, vomiting, and axillary pain over a period of three days. Soon, three more family members and a Buryat boy, who had been playing with the sick children, were attacked by the disease and died. The last victim was a Cossack woman, who had washed Evdokia's underwear. All bore the same symptoms: 'fever, headache, sometimes diarrhoea, constipation and pain under the armpits and groins, where some people noticed swollen glands'.<sup>27</sup>

In September 1891, another outbreak was reported to the military authorities of Aksha, after a Cossack named Daniil Gurulev allegedly visited a settlement across the Mongolian frontier. There, at the home of a Tungus shepherd, he cooked and ate a Siberian marmot, which he himself had previously caught and skinned. After returning home, the hunter, his pregnant mother, and five more members of his family succumbed to the disease.<sup>28</sup> What made this small outbreak important was the fact that it was attended by a well-trained medical team: Dr Utkin, the feldscher Savateev, and Dr Reshetnikov. Based on his experiences from the outbreak, the latter would compose a short report, which appeared in July 1895 as a three-page-long entry in the chronicles section of the *Review of Public Health and Judicial Practice of Medicine*. The paper bore the evocative title 'On Tarbagan Plague, Transmittable to People'.<sup>29</sup>

Besides giving the first systematic account of the 1888, 1889, and 1891 outbreaks, Reshetnikov, who had been stationed in Aksha since 1882, identified the tarbagan as Radde's *Arctomys bobac*.<sup>30</sup> Noting that marmots were a local delicacy resembling pork, he claimed that when the summer is dry, and not a drop of water is to be gotten, great epizootics

amongst marmots break out.<sup>31</sup> The ill animal, Reshetnikov underlined, ‘walks sluggishly, staggers like a drunk’.<sup>32</sup> As it lies sleepy on top of its mound, it was said to become an easy target for predators and shepherds alike. Reshetnikov reported the former as free to eat the ill animal with impunity. On the contrary, according to ‘local old-timers’, humans who ate the animal or otherwise used its products (fat, skin) for domestic purposes were infected by what was locally known as ‘tarbagan plague’.

Reshetnikov stressed that individuals were infected only through direct contact with the animal’s body fluids.<sup>33</sup> At the same time, he was careful to note that whilst Utkin had claimed that the disease in Suktui was bubonic plague, he, having attended to three individual fatal cases, failed to notice swollen glands or lymph-related pain. Rather than dissuading him from his original diagnosis, this observation led Reshetnikov to the conclusion that plague contracted by eating marmots did not necessarily infect the lymphatic system.<sup>34</sup>

### NATIVE KNOWLEDGE HYPOTHESIS

This was already an important series of observations, yet the outbreak that would play the most crucial role in the establishment of the connection between human plague and marmot epizootics came three years later, in September 1894, once again at the village of Suktui. On September 22, the feldscher Savateev received a call to attend the Mirsanov family, consisting of a married couple, five children, and two elderly in-laws. Arriving there the following day, Savateev recorded that Philip Mirsanov, the father-head of the Cossack family, had been taken ill on September 2, 1894, and died after three days. On September 14 his youngest son, Mitrofan, was also taken ill, bearing axillary and inguinal swellings; he died on September 16. As a result of the two ominous deaths, the Cossacks were removed to the deserted house of the previously afflicted Epov family (victim of the 1889 outbreak). In the isolation of the old Epov house, four more members succumbed to the disease. Savateev only had the chance to observe the final victim of the epidemic, twenty-year-old Natalya, who fell ill on September 23 and, though showing signs of recovery, relapsed, ‘became mad’, and died on October 3.<sup>35</sup>

Limited as the outbreak may appear, Savateev’s death led his superior, Mikhail Eduardovich Beliavsky, a senior doctor at Aksha’s military hospital, to pay closer attention to the situation. Beliavsky ordered the disinfection of the Epov and Mirsanov abodes with sulphuric acid and asked the local

Ataman to make sure they were not inhabited for the next twenty-four months.<sup>36</sup> He then undertook himself a short expedition to the outbreak epicentre. Belivsky's field observations on 'tarbagan plague' were published as a six-page-long paper in the April–July 1895 issue of the *Review of Public Health and Judicial Practice of Medicine*, under the chronicles section, which also featured Reshetnikov's piece. We need to pay close attention to this short entry, as therein lies the birth of the native knowledge hypothesis, alleging that Mongols and Buryats in the region were aware of the fact that marmots harbour plague, as well as of the means to hunt the animal without being infected.<sup>37</sup> Belivsky's article began with a short, general description of the tarbagan, its habitat, and its hibernation habits. What made Belivsky's paper unique, however, was its ethnographic outlook, which was tightly tied with the doctor's apparent belief in the behavioural origins of human epidemics. Native hunters make their appearance already in the second paragraph of the article. There Belivsky recounted that Buryats hunted marmots for their fat, claiming that the mesenteric fat of the animal 'serves as an excellent means to grease leather belts, horse harness, [as well as for] lighting'.<sup>38</sup> The author noted that tarbagan meat, which tastes like goose, is eaten not just by the 'simple Buryat and Cossack population' of the region, but also by the educated classes, with 'many of the local intelligentsia not refusing a luscious piece of fat tarbagan'.<sup>39</sup> So desired was the meat and fat of tarbagans, Belivsky claimed, that '[e]ven at the time when they are hiding in their holes for the winter, they are not spared: while the ground is not frozen, [hunters] dig up pits and spend a lot of time trying to catch even just ten of these animals'.<sup>40</sup>

In the third paragraph of the article, we come across the core elements of what was to become the native knowledge hypothesis regarding plague in the region. Pointing out that, in some years, at the end of the summer and just before they went back into hibernation, 'the tarbagan would get ill with some deadly disease', Belivsky informed his readers of a series of important items of local knowledge and practice.<sup>41</sup> 'According to observation of the locals', he claimed, 'it turns out that those tarbagans who have not hidden in their holes by the end of September or mid-October are ill, or plague-ridden as they are called.' The doctor acknowledged that 'the nature of the poison of the plague-ridden tarbagan is not known, and it would be highly interesting to investigate this using microscopic analysis of the blood of plague-ridden tarbagans'. As we will see, the conclusive bacteriological identification of the pathogen involved in 'tarbagan plague' came only in 1923. Still, if medical science was as yet

unable to elucidate the identity of the pathogen, locals were said to possess a formidable range of knowledge about the disease, which could be used to supplant medical ignorance. To begin with, Believsky claimed that they had a range of ways of detecting ill marmots. These were said to display an unusual silence and lack of coordination: ‘they do not bark, become sluggish, their walk is shaky, they often develop a reddish tense tumour under the shoulder; and when they go far away from their holes they cannot get back in, and become easy targets for their enemies.’<sup>42</sup> If hunters were said to be able to diagnose an ill marmot from afar, due to its wobbliness and conspicuous silence, they were also praised for possessing a traditional mode of diagnosis of captured marmots. Believsky claimed that the natives were able to distinguish a pestiferous ‘tumour behind the shoulder’. Yet, as this was said to be not always present or visible, they also had a more experimental method of diagnosis at their disposal. Making a cut on the sole of a captured targaban, they observed if there was ‘gore in the wound’, in which case ‘they consider this targaban plague-ridden and pass it to be eaten by dogs’.<sup>43</sup> Believsky reasoned that out in the steppes ill targaban ‘fall more easily prey to predators’, hence ‘contribut[ing] to the infrequency with which people are infected from the targaban’.

Nonetheless marmot-to-human infection was not totally excluded by Believsky, who claimed that, following information related to him by ‘a local old-timer from Suktui’, in the past thirty years three such instances had been observed in the Cossack settlement. And yet, Believsky claimed that locals not only possessed traditional knowledge of plague as a marmot-derived disease and of how to prevent marmot-to-human infection, but they also knew that once carried by humans plague became contagious: ‘all locals know very well the deadly danger of ill targaban for people who come in contact with them, so everyone is very careful in this respect. This very clear understanding that contagion is old and easy to catch also explains why there are so few cases of human illness.’<sup>44</sup>

Rather than limiting himself to the description of human-animal relations, Believsky provided an ethnographic sketch of local society stricken by plague. He claimed that whilst confined to the old house of the Epovs, the Mirsanov family was not in fact under absolute isolation, but rather under the distant but constant care of neighbours who procured provisions for the sufferers of the deadly disease: ‘the locals, out of pity, out of a sense of humanity, brought them food, water and firewood, taking the following precautions: all that was needed was brought and placed on the street next to their house; then they called out at someone, who

would come out pick up and bring in [the provisions], as they themselves quickly departed.<sup>45</sup> Beliaevsky's use of these ethnographic data was clearly stimulated by a much wider concern regarding the possibly contagious nature of plague.<sup>46</sup> This is also evident in his interest in burial practices of the surviving members of the family, who placed the bodies of the Mirsanovs in a specially fenced corner of the local cemetery so that they would not be dug out in the future. Beliaevsky's awareness of international medical debates current at the time is evident in that he noted that the said family members took special care of ridding themselves of the clothes and underwear of their perished kin.<sup>47</sup> Rather than focusing on potentially infectious objects, however, Beliaevsky was determined to pinpoint the behavioural context of the outbreak, hence tracing its origins to marmot hunting. Philip Mirsanov, the first victim of the dreaded disease, was said to have been on his way to a court hearing at Tsagan-Olui, when the dog that was accompanying him chased and caught six marmots. Mirsanov was further said to have hid the animals and to have recovered them after the hearing, carrying them back home on August 31. Two days later he lay sick with 'tarbagan plague'.<sup>48</sup> This then allowed Beliaevsky to formulate his epidemiological model of plague. On the one hand, he argued, plague was endemic amongst Siberian marmots, with the absence of recorded human epidemics in the region attributed to a rich and ancient corpus of knowledge regarding the disease, its zoonotic origins, and transmission pathways by hunters, who depend on the plague-carrying animal for food, fur, and fat. And, on the other hand, in cases when outbreaks did occur, these could consequently be attributed to hunting malpractices, such as the one evinced by Mirsanov.

Before concluding his article, Beliaevsky procured further ethnographic evidence to support this epidemiological model. In a short paragraph he related that he summoned a certain Badma Kishitkuev from the yurts closest to Suktui and asked him whether a marmot-to-human disease was a common phenomenon in the region, if it was treatable, and what happened with the homes and property of the deceased. The man reportedly answered by confirming that occasionally tarbagans were seen to be ill, in which case 'during harvesting their fat, each is tested for illness' through the aforementioned diagnostic method of making a cut in its paw. The informant also mentioned that in the past a treatment of this illness was often applied. This consisted in giving patients a concoction of black tea





Illustration 2 Hibernating marmot, captured 1921 (hand-written note on the back of the photograph by Wu Liande)

and ‘a powder made of glands that are located by the liver of the tarbagan’. However, this method was not effective and had since been abandoned, with the disease being currently ‘considered incurable and extremely contagious’. Badma Kishitkuev appeared to make no claim as to the cultural immunity of hunters as a result of their alleged knowledge. Though he mentioned that when the disease breaks out amongst humans, ‘they abandon to their fate the yurts with the ill, and migrate away to a new location’, returning only much later to ‘bury the dead and to burn the yurts with [their] possessions’, he also conceded that from time to time entire *ulus* (indigenous districts) were wiped out by the disease.<sup>49</sup>

Summing up the data he collected in Suktui, Beliavsky concluded by pressing for the institution of segregated, fenced-off burial of plague victims in lime, burning their clothes and furniture, and for the prohibition of newcomers or strangers to hunt marmots, as well as to procure or distribute marmot fat in the region.<sup>50</sup>



Illustration 3 Terminally ill plague-infected Siberian marmot (hand-written note on the back of the photograph by Wu Liande)

### INTERNATIONAL RECEPTION

The writings of Reshetnikov and Beliaevsky achieved immediate international repute in the context of unfolding plague epidemics in the Indian subcontinent (1896–1897). What was particularly important was Beliaevsky's theory of a sylvatic, animal origin of plague. For rather than adding yet another rodent to an already established list of animal hosts, Beliaevsky supported a direct, zoonotic origin of plague on the international medical stage three years before Paul-Louis Simond published his famous study on rats and their fleas.<sup>51</sup> In 1895, when Beliaevsky's paper was published, rats were largely considered to be 'co-victims of plague'.<sup>52</sup> During the 1894 Hong Kong epidemic, rat epizootics observed in the preceding years in south-west China were explained in terms of the plague bacillus being carried by telluric gases. As James Lowson, the medical officer in charge of the Hong Kong Epidemic Hospital, explained, rats' snouts are closer to the ground where from plague's gases emanate, hence

they naturally die first and in large quantities in contrast to humans, whose heads stand high above the ground.<sup>53</sup> As late as 1897, references to rat epizootics before human plague outbreaks in south China would elicit sarcastic remarks by leading medical figures like Adrien Proust, who simply commented: ‘the superstition of the Chinese sees these animals as messengers of the devil and they try to chase them away.’<sup>54</sup>

The notion of the tarbagan being a host of plague soon captured the interest of figureheads of plague research, such as Simond and Yersin, who would acknowledge Beliaevsky and Reshetnikov’s discovery, noting: ‘here, we believe, is a discovery of high value from the point of view of the aetiology of plague.’<sup>55</sup> The two international plague authorities described plague as a ‘wandering Jew’, who, departing from its original home of ‘pure enzooticity’ among ‘the marmots of Tibet’, travelled from area to area and town to town spreading death to humans.<sup>56</sup> This narrative was well embedded in a growing corpus of European literature on the subject which, following the writings of Patrick Manson and Emile Rocher, considered the Yunnan highlands as the origin of the third plague pandemic, as well as in racialised discourses of plague’s supposed propensity to travel.<sup>57</sup> Soon enough, leading texts on plague in various European languages would offer shorter or longer glimpses of the tarbagan hypothesis. Through such publications and their endorsement and adaptation of Reshetnikov and Beliaevsky’s plague ethnographies arose a thesis, which, in spite of the original papers’ elliptic nature, would assume a more or less coherent form in the shape of what I have called the native knowledge hypothesis.

German medical publications were amongst the first to notify the international scientific community of the findings. In 1899 an article appeared in the *Festschrift für Hygiene und Infektionskrankheiten*, authored by Dr Favre, of the University Laboratory of Krakow, who endorsed Beliaevsky and Reshetnikov’s discoveries in Transbaikalia. Favre’s description of the relation between what he called the *Sarabaganen* and plague underlined the natives’ supposed ability to avoid sick marmots whilst hunting for their fat and fur. The skill was said to be based on their readiness to spot plague-infested marmots through the latter’s inability to whistle or run fast. Favre also recounted the alleged ability of Mongols and Buryats to ascertain the existence of plague by performing an incision on the caught animal’s paw, as well as their propensity to abandon relatives suffering from *Sarabaganpest* in their yurts without taking any clothes or possessions with them.<sup>58</sup> This reproduction of Beliaevsky’s plague ethnography was in turn reflected in some of the most authoritative German works on

the disease, such as Müller and Pösch's *Die Pest*.<sup>59</sup> Whilst not adding anything new to the native knowledge hypothesis, these were very significant in diffusing it to an international audience. They also lent it considerable credence by articulating the allegations in German, the most authoritative scientific language at the turn of the century. Most importantly, the native knowledge hypothesis was later adopted by Georg Sticker, a veteran of the German Plague Commission during the Indian outbreak of 1896–97, in his book *Die Pest als Seuche und als Plage*, where he dedicated a few highly influential pages to the subject.

Sticker referred to Beliaevsky's findings emphatically, stressing how the plague-stricken marmot 'is sad and barks no more'.<sup>60</sup> In a narrative embroidered by anthropomorphic tropes, studied innuendos, and medical allegories, readers were told that this wild animal, which 'is otherwise so shy, that when a human comes near it escapes quick like an arrow into its nest, now crawls so slowly on the road, that it is easily found by the bullet of the hunter, even by the hand of a shepherd'.<sup>61</sup> Sticker paid considerable attention to the physical characteristics of the sick marmot, its 'dull eyes', its 'half unconscious' state and the alleged fact that it 'is hot to the touch', creating an intricate, exotic portrait of the ill animal.<sup>62</sup>

Widely read at the time, Sticker was a staunch defender of the native knowledge hypothesis, both in terms of the ability of Mongols and Buryats to diagnose ill marmots (from afar and via the axillary tumour and paw blood diagnosis) and their ability to comprehend and prevent human contagion. His graphic description of the latter is worth quoting, as an example of the way in which Beliaevsky's rather dry description became embellished in the process of its multiple translations in the medical press: 'there are times when whole settlements of Buryats die from the tarbagan plague. As soon as a spread of the evil is observed, the experienced steppe inhabitants abandon their yurts, isolate the sick and go to another area, coming back to the abandoned houses and tents only after a long time.'<sup>63</sup>

At the same time, colonial concerns over plague and its vectors also drew British interest to the tarbagan hypothesis. As early as August 1895 a *British Medical Journal* article informed the world of Beliaevsky's and Reshetnikov's research. In the following decade, many similar articles would appear in the English-speaking medical press. A great number of these were authored by the British Delegate to the Ottoman Board of Health in Istanbul, Franck Clemow, who was to transform into a true convert to Russian plague theories. Clemow would provide *The Lancet* and *The British Medical Journal* with frequent reports from the Eastern

plague front. It was these short and to-the-point articles that made both the tarbagan hypothesis and the native knowledge hypothesis known to the English-speaking medical world. The jewel in the crown of Clemow's outbreak narrative was his February 1900 article to *The Journal of Tropical Medicine*.<sup>64</sup> There, Clemow retold the story of the tarbagan and its native hunters in a most cohesive and eloquent manner. Deriding German sources as 'inadequate and incorrect in details', he took it upon himself to present readers with a coherent epitome of the research results of Beliaevsky and Reshetnikov. His article was to become a classic in plague literature, with future scholars, travellers, and journalists freely copying from it, often without acknowledging the source. Clemow's paper did not provide new evidence as such. Rather, the author's skill was in recombining old material in a tight, convincing, and in places fascinating narrative that confirmed the native knowledge hypothesis: 'the danger is perfectly understood by the people, and they are careful to avoid it.'<sup>65</sup>

Hence in the decade following the publication of Beliaevsky's and Reshetnikov's papers their hypotheses on the tarbagan origins of plague and on a local knowledge of the disease and its epidemiology were translated and widely circulated in the international medical press. Within the context of the third plague pandemic, which by 1900 had spread to South Asia, Australia, Europe, Africa, and North and South America, these speculations about the animal origin of the disease and about human-animal relations providing a conduit or hindrance to zoonotic infection spoke to the heart of global medical and epidemiological debates about the disease.

### EPISTEMOLOGICAL QUESTIONS AND ETHNOMETHODOLOGICAL MYSTIFICATION

I have elsewhere elaborated on the diagnostic contradictions inherent in the folk model of epidemiological knowledge proposed by Beliaevsky, which have led me to be sceptical about the actual existence of this epistemic faculty amongst Mongols and Buryats at the turn of the century.<sup>66</sup> I do not wish to repeat this argument here, as the epidemiological debate it engages in would distract us from the analytical focus of this book. It may, however, be of interest to the reader to keep in mind that, if our current understanding of plague epidemiology is correct, the methods reportedly used by native hunters to prevent the spread of plague could have but an extremely limited efficacy, and in some cases (such as feeding dogs with

ill tarbagan) would in all likelihood lead to an explosion of human plague cases. More importantly, from the analytical perspective of this book, what we need to examine here is the epistemological framework and methodology employed by the originators of the ethnographic configuration of plague in the region.

Both Beliaevsky and Reshetnikov talked of the disease they encountered in Transbaikalia by employing the category ‘tarbagan plague’, though they neglected to provide the term in Mongolian.<sup>67</sup> At the time, in Russian medical literature the scientific meaning of the term ‘plague’ (*chuma*) was principally derived from the Vetlianka experience of 1878–79, from which the main bulk of research literature arose. This is important, for by contrast to the Hong Kong 1894 epidemic, the Vetlianka outbreak was mainly a pneumonic and not a bubonic one.<sup>68</sup> After the initial confusion, this appears to have led to an expansion of the term plague to include pathologies not related to the presence of buboes, or at least to a consolidation of this inclusion. It is difficult to ascertain what was the interaction of this newly opened to reinterpretation diagnostic field with news coming from Hong Kong at the time, regarding the identification of plague’s pathogen. In the case of Reshetnikov, who undertook fieldwork in 1891, *chuma* was a term that could have no in situ bacteriological reference, as the discovery of the bacillus came in the summer of 1894.<sup>69</sup> There is also no sign of retrospective reference to bacteriological plague in his 1895 paper. Furthermore, Reshetnikov does not specify the evidence that ultimately convinced him that the disease is plague, albeit not of a bubonic form—a plague that did not infect the lymphatic system. We may thus reasonably assume that his diagnosis followed the precedent of Vetlianka, the outbreak on which the vast majority of Russian works on the disease focused until that time. As for Beliaevsky, knowledge of the bacteriological identity of plague was indeed possible. Beliaevsky undertook his study of plague in October 1894, by which time Kitasato’s and Yersin’s discoveries of the bacillus had been published in French and in English, with three papers dedicated to the Hong Kong outbreak also appearing in the Russian medical press.<sup>70</sup> News of the discovery could have made their way to the military post of Aksha by the time of the Suktui 1894 outbreak, although this is not acknowledged by the author. Notably, although their articles were published in the April–July 1895 issue of the medical journal, neither Reshetnikov nor Beliaevsky make reference to Yersin’s and Kitasato’s discovery, or to the existence of the bacillus. It is thus probable that the use of the term ‘plague’ in the two reports refers not to a disease carried

by a specific, bacteriologically identifiable pathogen, but to an epidemic that mortally afflicted humans with buboes or, in the case of Reshetnikov, other, pneumonic symptoms.

Furthermore, a careful reading of the two doctors' work places them at an epistemologically liminal zone, where the power of the laboratory to identify the pathogen is recognised, but the microscope is not actually deployed in order to perform such a task. Hence Beliavsky notes that '[t]he nature of the poison of the plague-ridden tarbagan is not known', placing his hopes for some answer to future microscopic blood examination of the 'plague-ridden marmot'.<sup>71</sup> It would take another sixteen years until Leonid Michailovich Isaev would claim to have seen the plague bacillus in marmot blood with the help of the microscope. Even then the discovery received little attention or credit outside Russia; as mentioned above, the final, laboratory identification of 'tarbagan plague' as caused by the plague bacillus came only in 1923.<sup>72</sup> In the meantime, field-based research would flourish, generating a large volume of data that, whilst not necessarily abiding with laboratory criteria of veridiction, would be of great importance for global scientific understandings of plague.

As Reshetnikov and Beliavsky laid the foundations for the ethnographic examination of plague on the Chinese-Russian border, it is vital to examine the methodology employed in securing such data in the field. Reshetnikov arrived at the scene of the outbreak in mid- to late September, a time of the year during which he may have had the opportunity to glimpse the last few days of marmot hunting in the case of a very warm autumn. Nonetheless he makes no reference to such experience, or to having actually seen a marmot, ill or otherwise. The only indication given by Reshetnikov about his sources is when he writes that 'according to observations by local old-timers, as well as from the conclusions [drawn from] the widespread extinction of entire families, what locals call "tarbagan plague" occurs in humans only through domestic use and by eating ill tarbagan'.<sup>73</sup> This phrase does not clarify whether the source of his data was native or settler, as both could be described as being 'locals'. It is perhaps indicative, however, that Reshetnikov does not use the term for 'indigenous' (*tuzemtsui*), current in Russian ethnology at the time. This, it may be noted, was the term used by Cherkassov in his memoirs to refer to the native population. A careful reading of Reshetnikov hence verifies that he did refer to some knowledge of plague, but fails to clarify whether this was a knowledge derived from and shared amongst Cossacks and Russian exiles/settlers or

native hunters, or both.<sup>74</sup> In this sense it cannot be said that Reshetnikov unambiguously asserted a native knowledge of plague in the region.

When we come to Believsky's longer and more complex narrative, we are once again faced with similar questions. Believsky arrived at the scene of the outbreak too late to observe marmots, which were already in hibernation. He hence never set an eye on a tarbagan, let alone experiencing marmot hunting, during his 1894 Suktui fieldwork. All his data regarding marmots were gathered by word of mouth or through interviews. Whether Badma Kishitkuev was his only native informant is not clear; he is however the only one designated as such.

In order to comprehend the ethnographic configuration of plague in the pioneering work of Reshetnikov and Believsky, we must pay close attention to the ethnomethodological discrepancy between several kinds of ethnographic data: 'actually observed behaviour, the analyst's generalisations, informant's recollections of past events, their statements of what should be done or what is usually done.'<sup>75</sup> The danger here, as Ladislav Holy and Milan Stuchlick have noted in one of anthropology's key ethnomethodological texts, is the following: 'the demand for the formulation of the social structure gives the illusion that all these differing data are data about it, that models built on different kinds of data are coincident and that one kind of data is an adequate substitute for another; observed events, events reconstructed by the informants, events reconstructed by the analyst, statements of juridical norms are all taken as informing about the same thing.'<sup>76</sup> If we then examine Believsky's ethnographic approach of plague from an anthropological perspective, we come to the conclusion that he provides us with four ethnomethodologically distinct types of data, which he nevertheless interweaves in such a way that often makes them hard to distinguish. This is a common mystifying effect of early ethnographic writing, where the truth-effect of one's narrative about the native other is predicated not simply upon the obscuration of the actual data-retrieval methods employed in the field, but, more importantly on the 'illusion', as Holy and Stuchlick put it, 'that one kind of data is an adequate substitute for another'.<sup>77</sup>

What were then the methods actually employed by Believsky in producing his plague ethnography? First, his narrative contains observed events, such as Cossack burial customs. Second, it contains events observed or reconstructed by the author's subalterns, namely feldscher Savateev, such as the symptoms and course of the disease. Third, it contains events reconstructed by the author himself based on indirect or unprovenanced



sources, such as the 1888 and 1891 outbreaks. Fourth, it contains events reconstructed by the author's only acknowledged informant, such as the claim that hunters were able to identify a plague-inflicted marmot by inflicting a cut on its paw and observing the quality of its blood.

As regards data supporting a native knowledge of plague, these fall under two categories: first, data reconstructed by Beliaevsky's only named informant, Badma Kishitkuev; second, unprovenanced data. On the source of the latter Beliaevsky gives us no information, although we know he could not have observed these practices himself due to the timing of his visit. Thus these may be either data reconstructed by Beliaevsky's feldscher, data reconstructed by Cossacks or Russian settlers and exiles, or data communicated to Beliaevsky by native informants who remain invisible in his narrative.

Here a further observation needs to be made. Like Reshetnikov, Beliaevsky does not use the term 'indigenous' in his text, but rather the more neutral 'local' in order to refer to a man in Suktui who claimed that the Cossack settlement had only witnessed three human victims of plague in the previous thirty years.<sup>78</sup> As regards the ambiguity of the term local in Beliaevsky's text, it is worth noting that he also uses it to refer to Russian Cossacks.<sup>79</sup> From his narrative one may thus cautiously surmise that the main source of information regarding plague came from within the Cossack settlement, with the exception of his only named native informant, Badma Kishitkuev, who Beliaevsky describes as having been summoned specifically for this purpose from 'the yurts nearest to Suktui'.<sup>80</sup>

Significantly the data given by the latter do not coincide with the full spectrum of plague-related knowledge described by Beliaevsky. Badma Kishitkuev referred to the paw-incision test, as well as to the custom of leaving afflicted individuals alone in the yurt, migrating for a time, and upon returning burying the body and burning the yurt. None of the other data constituent of the alleged native knowledge of plague was cross-referenced by the named native informant. This includes all information provided by Beliaevsky regarding prophylactic hunting practices and distant diagnosis of plague-affected marmots, the very core of the native knowledge hypothesis. In the decades to follow, however, the ethnographic ambiguities inherent to Reshetnikov and Beliaevsky's papers would be overlooked, with medical as well as lay authors freely drawing and interpreting their research in ways that took as their common ground the supposition of an indigenous knowledge of the disease and of ways to prevent its spread amongst humans. In this respect, it is more accurate to say that

the native knowledge hypothesis arose out of Reshetnikov and Believsky's writings, than to claim that it was actually or clearly articulated in them.

Hence the production of the first, and in many ways the paradigmatic, ethnographic approach to plague on the Chinese-Russian frontier was predicated upon three levels of mystification. On an epistemological level, what we are interpellated to know through the writings of Reshetnikov and Believsky is 'plague'. Yet the criteria that establish this object of knowledge as a distinct, identifiable disease are not transparent, allowing for a potential transference of the term from one epistemic field to another. On an ethnomethodological level, what is in fact an assortment of information and data, collected in four methodologically distinct ways, is presented as a unified field of scientific evidence—one story with a single meaning, a coherent ethnographic profile—when in fact we only have desperate fragments of stated opinions, observed events, and reconstructed events. Finally, again on an ethnomethodological level, the identity, status, and agenda of the informants who provided the above data is obscured under the term 'locals', which allows the identification of ethnographic subjects, and ultimately denies them a voice of their own.

It may appear that such ethnomethodological distinctions are rather fine details when it concerns a way of collecting and systematising data that was only beginning to flourish at end of the nineteenth century. Yet if we make the effort to go back to the writings of Payne during the 1878–79 outbreak in Vetlianka, we soon see that this distinction was not as outlandish as it may appear. When Payne comes to the topic of fish-curing vats being a possible source of the epidemic, he is careful to distinguish between what he has heard about these watagas from what he has actually observed with his own eyes: 'it is said that in consequence of the high price of salt the brine is used over and over again till it becomes loaded with organic matter and is in the Summer putrid and highly offensive. This statement we were unable to verify since the season of fish curing had not commenced at the time of our visit and such Watagas as we visited had nothing offensive.'<sup>81</sup>

Was this process of mystification then simply an instant particular to Reshetnikov and Believsky's work? On the contrary, it was a process constitutive of the ethnographic configuration of plague on the Chinese-Russian frontier, reaching its most intriguing form in subsequent efforts to enrich the native knowledge hypothesis with new, highly interpretative elements.<sup>82</sup> These, as the next chapter will show, were centred around a medical exegesis of native mythology and ritual, which, from

an anthropological perspective, constitutes a prominent example of what Mary Douglas has critiqued as medical materialist reductionism.

## NOTES

1. J.T. Alexander, *Bubonic Plague in Early Modern Russia: Public Health and Urban Disaster* (Oxford: Oxford University Press, 2003).
2. I am following here the critique of the notion of the population as developed by Foucault (2007).
3. When used here with regard to events and diagnoses before 1894, the term plague refers to a disease whose victims showed symptoms of fever and non-venereal buboes. It should however be noted that buboes are not as common in bubonic plague as often assumed, something already observed by Murray (1880: 132), who in his review of the 1830s plague outbreaks in Kumaon, India, noted that ‘the appearance of buboes is not an essential characteristic of plague’. For a detailed discussion and bibliography of clinical descriptions of plague before 1894 see Walløe (2008).
4. Mikhel, D., ‘Chuma i épidemiologicheskaya revolyutsiya v Rossii, 1897–1914.’ *Vestnik Evrazii*, no. 3 (2008), p. 143.
5. M.E. Hanson, *Speaking of Epidemics in Chinese Medicine: Disease and Geographic Imagination in Late Imperial China* (London: Routledge, 2011). For further discussion of this concept see Chap. 4.
6. M. Bassin, ‘The Russian Geographical Society, the “Amur Epoch”, and the Great Siberian Expedition 1855–1863,’ *Annals of the Association of American Geographers*, vol. 73, no. 2 (June 1983): 240–256.
7. P. Kropotkin & D.W. Freshfield, ‘Obituary: Dr. Gustav Radde,’ *The Geographical Journal*, vol. 21, no. 5 (May 1903): 563–565.
8. Bassin, ‘The Russian Geographical Society, the “Amur Epoch” and the Great Siberian Expedition 1855–1863,’ p. 254, nt. 7.
9. The term *Arctomys bobac* is no longer in use, and the Siberian marmot is known as *Marmota sibirica*. Although Radde uses the term liberally, the Siberian marmot should not be confused with the true bobak, which is a distinct species (*Marmota bobac*).
10. G. Radde, *Reisen im Süden von Ost-Sibirien in den Jahren 1855–1859*, 2 vols. (St Petersburg: Buchdruckerei der Kaiserlichen Akademie der Wissenschaften, 1862), vol. I, p. 162. By request of the author, the translation has retained its epoch idiom as best as possible in English.
11. See Chap. 3.
12. Tarbagan is the standard Russian transliteration of the Mongolian *tavvaga*. It was also spelled *tarabagan* by several Russian authors, as well as in many of Wu Liande’s reports. The correct transliteration of the hunter’s name is

- Cherkasov; here I retain the transliteration of his name in the recent English translation of his book.
13. A.A. Cherkassov, *Notes of an East Siberian Hunter* (Bloomington: Authorhouse, 2012), p. 365 (amended translation by the author).
  14. C. Zuber, 'Rapport sur une mission médicale en Russie; La peste du gouvernement d'Astrakhan,' *Recueil des travaux du Comité Consultatif d'hygiène publique de France et des actes officiels de l'administration sanitaire* (Paris: A. Lahure, 1880) vol. 9, pp. 87–167; G.N. Minkh, *Chuma v Rossii (Vetlyanskaya épidemiya 1878–1879gg.)*. (Kiev 1898).
  15. H. Heilbronner, 'The Russian Plague of 1878–79,' *Slavic Review*, vol. 21, no. 1 (March 1962): 89–112.
  16. Heilbronner, 'The Russian Plague of 1878–79,' p. 93.
  17. Heilbronner, 'The Russian Plague of 1878–79,' p. 98.
  18. Heilbronner, 'The Russian Plague of 1878–79,' p. 104.
  19. H. Ebrahimnejad, *Medicine, Public Health and the Qājār State: Patterns of Medical Modernization in Nineteenth-Century Iran* (Leiden: Brill, 2004).
  20. Zuber, 'Rapport sur une mission médicale en Russie'; J. Lawrence-Hamilton, 'The Report of the Imperial German Medical Commission on the Plague which Prevailed in the Province of Astrakhan during the Winter of 1878 and 1879,' *Transactions of the Epidemiological Society of London*, vol. 4 (1875–76, 1880–81): 376–390; J.F. Payne, 'On Certain Points Connected with the Epidemic of Plague in the Province of Astrakhan, Russia, in the Winter of 1878–79,' *Transactions of the Epidemiological Society of London*, vol. 4 (1875–76, 1880–81): 362–375; W.H. Conwill, 'Notes on the Recent History of Plague in the Province of Baghdad,' *Transactions of the Epidemiological Society of London*, vol. 4 (1875–76, 1880–81): 9–29.
  21. Zuber, 'Rapport sur une mission médicale en Russie,' p. 92.
  22. Payne, 'On Certain Points Connected with the Epidemic of Plague in the Province of Astrakhan,' p. 365. These vital resources were in consequence destroyed by the Russian anti-plague task force. It should be noted, however, that Zuber (1880: 138–139) rejected the spontaneous origins hypothesis.
  23. Payne, 'On Certain Points Connected with the Epidemic of Plague in the Province of Astrakhan,' p. 369.
  24. For a discussion of plague research in the region see the work of Dmitry Mikhel, whom I would like to thank for sharing ideas about this topic.
  25. Originally founded by settler farmers from the western Russian town of Totma (1750), in the 1830s Aksha became an exile site for Decembrists and soon developed into the military centre of ten surrounding Cossack settlements.

26. A. Reshetnikov, 'O chumê tarbaganov, perenesennoï na lyudeï,' *Vestnik obshchestvennoï gigiennoi, sudebnoï i prakticheskoi meditsinui*, vol. 23, no. 2 (April–June 1895), p. 6.
27. M.E. Belavsky, 'O chumê tarbaganov: zapiska po povodu 7 smertnuikh sluchaev ot upotrebleniya v pishchu surkov, porazhennuikh chumoyu v poselkê Soktuevskom,' *Vestnik obshchestvennoï gigiennoi, sudebnoï i prakticheskoi meditsinui*, vol. 23, no. 2 (April–June 1895), p. 3. Later the boy would mistakenly be said to have been Tungus rather than Buryat (Wu 1926: 68).
28. Reshetnikov, 'O chumê tarbaganov,' p. 7.
29. Reshetnikov, 'O chumê tarbaganov.'
30. Reshetnikov, 'O chumê tarbaganov,' p. 6. Reshetnikov claimed that the first mention of 'tarbagan plague' appeared in the Transbaikalia Oblast Gazette in 1892, which warned against the 'domestic use' of the animal (1895: 9).
31. The idea of drought leading to human plague epidemics was a persistent theme during the third pandemic, having first been formulated during the 1894 Hong Kong outbreak. The possibility of Reshetnikov simply mirroring this discourse does not preclude the potential importance of drought as regards the relation between plague epizootics and human epidemics; for a recent discussion of the latter see Schmid et al. (2015).
32. Reshetnikov, 'O chumê tarbaganov,' p. 8.
33. Reshetnikov, 'O chumê tarbaganov,' p. 8.
34. Reshetnikov, 'O chumê tarbaganov,' p. 9.
35. Belavsky, 'O chumê tarbaganov,' p. 4.
36. Belavsky, 'O chumê tarbaganov,' p. 5.
37. Although Belavsky's paper would be constantly referred to in consequent literature on plague on the Chinese-Russian border, there exists no direct translation of the tracts laying the bases of the native knowledge hypothesis. Future authors borrow phrases, even entire sentences, from this 1895 report, yet not once is a direct quote or translation offered to the reader. This omission has allowed for many liberties in the interpretation of Belavsky's words, as well as in matters of emphasis.
38. Belavsky, 'O chumê tarbaganov,' p. 2.
39. Belavsky, 'O chumê tarbaganov,' pp. 1–2. Belavsky never tasted a marmot himself, and the bizarre comparison of its meat with goose may be simply due to a linguistic slip between tarbagan and *ptarmigan*, the Arctic grouse. For such a confusion in the American medical press see Chernin (1989).
40. Belavsky, 'O chumê tarbaganov,' p. 2.
41. Belavsky, 'O chumê tarbaganov,' p. 2. All quotes below from the same page.
42. 'Interspecies' anthropologists would be fascinated to know that locals, according to Belavsky's narrative, were not the only ones aware of the

- danger posed by marmots bearing such symptoms: ‘it is said that if an ill tarbagan makes it to the hole, it is chased out by others, and if it dies, it is taken out immediately to the surface. So there are almost no ill tarbagan inside the holes’ (Beliavsky 1895: 2).
43. Dogs and wolves, who ‘consume tarbagan in great quantities’, were said by Beliavsky not to suffer from plague; this is an interesting claim given our current knowledge that canines do in fact carry plague. Recent cases of human infection via dogs in China and the USA suggest that if in fact Mongols and Buryats threw plague-infected marmots to their dogs, it would not be long before they themselves caught the disease via them (Wang et al. 2011; Ge et al. 2014; Coffman 2014).
  44. Beliavsky, ‘O chumê tarbaganov,’ p. 2.
  45. Beliavsky, ‘O chumê tarbaganov,’ p. 4.
  46. The first systematic research on whether plague can be transmitted from human to human took place in India two years later. See Chap. 4 for a discussion on notions of plague as highly contagious.
  47. Beliavsky, ‘O chumê tarbaganov,’ pp. 4–5. As we have already seen in the Introduction, burial practices and clothing items were often suspected of aiding the transmission of plague. See Chap. 4 on the impact of the concern on clothing items in the geographic imagination of plague.
  48. Beliavsky, ‘O chumê tarbaganov,’ p. 6. In the following years authors, like Clemow, would claim that the tarbagans in question were covered under roadside hay. Beliavsky speculated that the very fact that a dog could so easily harvest six marmots meant they were sick.
  49. All quotes from Beliavsky, ‘O chumê tarbaganov,’ p. 5.
  50. Beliavsky, ‘O chumê tarbaganov,’ p. 6.
  51. P.-L. Simond, ‘La Propagation de la Peste,’ *Annales de l’Institut Pasteur*, vol. 62 (1898): 625–687.
  52. J.C. Mohr, *Plague and Fire: Battling Black Death and the 1900 Burning of Honolulu’s Chinatown* (Oxford: Oxford University Press, 2005), p. 12.
  53. Hong Kong Government Gazette [GA 1895 no.146].
  54. A. Proust, *La défense de l’Europe contre la peste et la conférence de Venise de 1897* (Paris: Masson et Cie, 1897), p. 5.
  55. P.-L. Simond & A. Yersin, ‘Les epidemies de peste en Extrême-Orient,’ *XIIIe Congrès International de Médecine, Paris, 1900; Section de Médecine et de Chirurgie Militaires; Sous-Section Coloniale* (Paris: Masson et c., 1900), pp. 35–36. On Zabolotny and his plague research see Chap. 5.
  56. Simond & Yersin, ‘Les epidemies de peste en Extrême-Orient,’ pp. 36 and 59.
  57. W. Rocher, *La Province Chinoise du Yunnan* (Paris: Lerous, 1879); P. Manson, ‘Dr. Manson’s Report on the Health of Amoy for the Half-year ended 31st March 1878,’ *Customs Gazette, Medical Reports*, no. 2 (January–March 1878), pp. 25–27. For a discussion and defence of this

- hypothesis see Benedict (1996). On plague's geographic trajectories see Chap. 4.
58. F.A.E. Favre, 'Ueber ein pestähnliche Krankheit,' *Zeitschrift für Hygiene und Infektionskrankheiten* (May 1899), pp. 361 and 363.
  59. H.F. Müller & R. Pösch, *Die Pest* (Vienna: A. Hölder, 1900).
  60. G. Sticker, *Abhandlungen aus der Seuchengeschichte und Seuchenlehre*, 2 vols. (Gießen: Töpelmann, 1908), vol. I (*Die Pest als Seuche und als Plage*), p. 120.
  61. Sticker, *Abhandlungen aus der Seuchengeschichte und Seuchenlehre*, p. 120.
  62. Sticker, *Abhandlungen aus der Seuchengeschichte und Seuchenlehre*, p. 120. See Chap. 4 for further discussion of ethnographic aspects in Sticker.
  63. Sticker, *Abhandlungen aus der Seuchengeschichte und Seuchenlehre*, p. 406.
  64. Fr. G. Clemow 'Plague in Siberia and Mongolia, and the Tarbagan (*Arctomys bobac*),' *The Journal of Tropical Medicine*, vol. 3 (February 1900): 169–176. A short summary of Clemow's article was published in French two years later under the bibliographic section of the *Archives of Naval Medicine* (Clemow 1902).
  65. Clemow, 'Plague in Siberia and Mongolia, and the Tarbagan (*Arctomys bobac*),' p. 170. For further discussion of Clemow's paper see Chap. 4.
  66. C. Lynteris, 'Ignoring Native Ignorance: Epidemiological Enclosures of Not-Knowing Plague in Inner Asia.' In Roy Dillely & Thomas (eds) *The Anthropology of Ignorance* (Oxford: Berghahn Press, 2015) 50–59.
  67. The Mongolian term 'tarvagyn tahal' is in use today, but it is unclear whether this is a translation from the Russian or vice versa. Nineteenth-century dictionaries contain the term *tahal* to refer to a contagious disease, with no reference to its source; the term '*tarvagyn tahal*' is not featured in dictionaries from the period. The existence of the term today often misleads observers to presume of the prescientific character of the category. In either case, the correct translation would be 'tarbagan disease' and not 'tarbagan plague'. I am grateful to Caroline Humphrey for this information.
  68. On the debates around the nature of the Vetlianka outbreak, on pneumonic symptoms, and on the proliferation of suggested taxonomies at the time see Payne (1879).
  69. S. Kitasato, 'The Bacillus of Bubonic Plague,' *The Lancet*, vol. 144, no. 3704 (August 25 1894): 428–430; A. Yersin, 'La peste bubonique à Hong Kong,' *Annales de l'Institut Pasteur*, vol. 8 (1894): 662–667.
  70. A. Skibnevsky, 'Chuma v Gong-Konge,' *Meditsinskoe obozrenie*, no. 14, vol. 42 (1894): 189–191; Anon., 'K Voprosu o microbe chumui,' *Vestnik obshchestvennoï gigeniui sudebnoï i praktichesknoï meditsinui*, vol. 1 no. 1 (July 1894): 116–117; N. Vendt, 'Chuma v Kitae,' *Vrach*, no. 28 (1894): 801–802.

71. Beliaevsky, 'O chumê tarbaganov,' p. 2.
72. See Chap. 5 for discussion.
73. Beliaevsky, 'O chumê tarbaganov,' p. 7.
74. It must be noted that some of these Cossacks may have in fact been ethnically Buryat, which complicates matters further. Beliaevsky nonetheless insisted on opposing the former to the latter, such as when he writes: 'the Tungus and the nomadic Buryats engage in hunting the tarbagan for obtaining its fat, which is ubiquitously sold to our Cossacks. In addition, the Buryats and almost all Cossacks use the tarbagan as food' (Beliaevsky 1895: 6).
75. L. Holy & M. Stuchlik, *Actions, Norms and Representations: Foundations of Anthropological Inquiry* (Cambridge: Cambridge University Press, 1983), p. 11.
76. Holy & Stuchlik, *Actions, Norms and Representations*, p. 11.
77. Holy & Stuchlik, *Actions, Norms and Representations*, p. 11.
78. Holy & Stuchlik, *Actions, Norms and Representations*, p. 2.
79. Holy & Stuchlik, *Actions, Norms and Representations*, p. 7.
80. Holy & Stuchlik, *Actions, Norms and Representations*, p. 5.
81. Payne, 'On Certain Points Connected with the Epidemic of Plague in the Province of Astrakhan,' p. 365.
82. It should be noted that the first decade of the twentieth century saw the application of ethnographic methods of studying plague across the southern frontier of the Russian Empire; see, for example, Strakhovich and Polënov (1907) and Bêlilovsky (1913).



## Medical Myths and Mythic Medicine

In his ever-popular sketch of a world history on the basis of an examination of infectious disease pandemics, William McNeill writes regarding plague in Mongolia: ‘nomad tribesmen of the steppe region, where these animals lived, had mythic explanations to justify the epidemiologically sound rules for dealing with the risk of bubonic infection from marmots.’<sup>1</sup> This assertion should be read critically against the background of the work of Mary Douglas who, in one of the most influential books in anthropology, famously confronted approaches that interpret eating taboos as hygienic practices thinly covered by a symbolic or ritual cloak. ‘Some argue’, Douglas claimed in *Purity and Danger*, ‘that even the most exotic of ancient rites have a sound hygienic basis. Others, though agreeing that primitive ritual has hygiene for its object, take the opposite view of its soundness [...] But both these medical approaches to ritual are fruitless because of a failure to confront our own ideas of hygiene and dirt.’<sup>2</sup> It is of particular interest to see what Douglas has to say about the first case, which corresponds to some crucial interpretive outputs of the ethnographic approach of plague. Douglas argued that this approach implies ‘that if we only knew all the circumstances we would find the rational basis of primitive ritual amply justified’.<sup>3</sup> Hence, she claimed, the import of incense burning would be interpreted not in terms of its cosmological importance, but as ‘a means of making tolerable the smells of unwashed humanity’.<sup>4</sup> More famously, in terms of her ground-breaking analysis of Leviticus, Douglas castigated this approach as explaining away the avoidance of pork in Judaism and

Islam ‘as due to the dangers of eating pork in hot climates’ or interpreting washing before eating as a practice that ‘may have given the Jews immunity in plagues’.<sup>5</sup> She hence argued that even in cases when practices of ritual avoidance do have a ‘marvelous correspondence’ with disease avoidance, as seen from a contemporary biomedical viewpoint, we should not assume this to be their underlying aim. ‘[I]t is one thing’, she claimed, ‘to point out to the side benefits of ritual actions, and another thing to be content with using the by-products as sufficient explanation.’<sup>6</sup> Following William James, in his critique of attempts to interpret religious experiences like Paul’s epiphany on the road to Damascus in medical terms, Douglas named this interpretivist fallacy ‘medical materialism’.<sup>7</sup>

In this chapter I want to argue that, at the turn of the century, the ethnographic configuration of plague on the Chinese-Russian frontier had at its heart an operation of medical materialist reductionism. This explained away Mongol and Buryat myths and cosmologies as metaphors of hygienic precaution and epidemiological prevention. I will here examine some prominent examples of this operation, arising out of the native knowledge hypothesis. These were formulated on the bases of the 1895 articles by Reshetnikov and Belavsky, whilst far surpassing the latter’s scope or intention. This operation of medical materialism, I will argue, further advanced the silencing and distorting of native subjects’ voices. Paradoxically, it did this by attributing knowledge and agency to these subjects, in a way which may at first sight seem at odds with prevailing Eurocentric and colonial approaches. This must not however be seen as an enlightened exception from or contradiction with the dominant paradigm, representing native subjects as fundamentally ignorant and unsanitary. On the contrary, it must be analysed as an interpretive strategy that dialectically reinforced this biopolitical narrative. This, I will argue, it did by instituting Mongols and Buryats as hygienic to the extent that, and only insofar as, they replicated scientifically defined methods of hygiene under ritual cloak.

### SHAPE-SHIFTERS

In the biography of Danilo Kirilovich Zabolotny, whose work I will examine in detail in the next chapter, Gleb Golubev tells us how during his 1898 expedition to Mongolia the internationally renowned plague expert asked Mongol hunters to organise a marmot hunt in order to check Belavsky’s native knowledge hypothesis.<sup>8</sup> Golubev narrates that amongst the hunting party, composed of men carrying rifles and bows, two Mongols bore

a shaggy black yak skin, which was employed in the capture of Siberian marmots. The process involved spotting a tarbagan, and then crouching on all fours, with the skin over one's back, as the hunters slowly approached their prey. This was said to arouse the curiosity of the marmot, which, suspending its natural suspicion and fear of predators, stood still watching the chimeric hunter approach, until he was close enough or in the opportune angle to shoot the animal.<sup>9</sup>

In the case narrated by Golubev, shooting the animal failed to kill it on the spot. Wounded, the marmot took refuge in a nearby burrow. Upon the suggestion of Zabolotny to pull it out of its hole, the native hunters reportedly replied: 'the devil pull it out! You know what we say in our places? If a tarbagan is killed on the spot, that's fine. If hit by an arrow it [escapes] in its burrow—too bad. There, underground, an evil spirit shape-shifts. You know what? Ten men cannot pull it out. Pull it out and death be revealed.'<sup>10</sup> Brushing aside scornful comments about yet another were-marmot myth, Zabolotny is said to have noted that the story made sense considering the disease carried by the tarbagan. Though marginal (and perhaps even fabricated), this colourful anecdotal passage is nonetheless indicative of an interpretive strategy central to the ethnographic configuration of plague at the time; a strategy that sought to interpret native myths as narratives about plague and its prevention.

Siberian marmots are animals of great significance in Mongolian and Buryat mythology. Evidence of transformational beliefs regarding these rodents can be traced back to Grigory Potanin's account of eleven marmot-related myths in Mongolia.<sup>11</sup> Collected during the ethnologist's famous expedition to the region in 1876, the mythic variants provide us with a rich subtext of human-marmot relations in the region. Origin myths at the time appear to revolve around the idea that marmots originate from Erkhei Mergen, a legendary archer who took up the task of salvaging the earth from the seven (or in other variants three or four) suns that were scorching its surface.<sup>12</sup> Famous for his skill in archery, Erkhei Mergen had little difficulty shooting down six of the suns. Yet when the hero took aim at the seventh sun (presumably the one we all know today) things took a bad turn. Meeting Erkhei Mergen's arrow in mid-air, a little bird (a *tel'gen*, or in other variants a lark) interrupted its course, with the arrow splitting the bird's tail into a fork. Such was Erkhei Mergen's anger and arrogance that he pledged to shoot the bird down. If he failed, he promised to chop off his mighty thumbs and bury himself in the ground. The myth usually concludes with the archer failing, outpaced and outsmarted by the

tiny bird. Fulfilling his terrible pledge, Erkhei Mergen hence proceeded to chop off his thumbs (marmots only have four claws) and bury himself underground, where he transformed into the first tarbagan.

This is only a crude summary of one of the central mythical themes regarding the origin of marmots. As such, it is not meant to substitute the subtlety of the mythic variations or their ethno-poetic efficacy, but simply to familiarise the reader with the basic precepts of the Mongolian tarbagan mythical constellation.<sup>13</sup> Other variants narrate how Erkhei Mergen's legendary arrogance led him to a killing spree that threatened to annihilate all animals and birds; Potanin notes that 'the main idea is the punishment of the boastful proud hunter'.<sup>14</sup> Yet what is important, from the perspective of the ethnographic configuration of plague, is not so much how Erkhei Mergen came to become a marmot, as his predicament once the transformation had taken place. A number of mythic variants recount the ur-marmot pledging not to eat or drink for nine months; yet one rather short variant recorded by Potanin stands alone from the rest in that it gives a stern warning, whose resonance with Zabolotny's shape-shifting encounter is immediately apparent: 'if one has shot down a marmot from the bow it is well; but if he escaped with the arrow into his hole it is evil. He changes himself into a *chetkur* (devil). Ten men, the whole *gachoun*, will not then dig him out. It will be hard for the whole Aimak to get him.'<sup>15</sup>

The above, exceptionally accurate, translation was provided only a few years after the original Russian publication of Potanin's work by Gardener, the British Consul at Ichang, for the ethnological journal *Folk-Lore*.<sup>16</sup> In light of Potanin's fame and impact both in Russia and on the international stage, it is tempting to suspect that Golubev simply gleaned elements from his work. A direct loan from Potanin is, for example, evident in the following turn: 'if a tarbagan is killed on the spot, that's fine. If hit by an arrow it [escapes] in its burrow—too bad.' Yet this was not blind repetition. For Golubev's account included a novel ethnographic datum, which is of great interest to us: 'there, underground, an evil spirit shape-shifts.'<sup>17</sup> This, and the reference to were-marmots in Golubev's account, appear to be resonant of a cultural theme in the region well known to anthropologists—*mafarism*. This was a scantily recorded but historically significant cult, which, following Sergei Shirikogoroff, revolved around were-animals believed to have accumulated power through thousands of years of breathing exercises in the quest of immortality.<sup>18</sup> In her discussion of Shirikogoroff's material on the cult, Caroline Humphrey has noted that animals included the fox, the yellow weasel, the python, the spider, the pheasant, the hedgehog,

the raccoon, the dog, the hare, and the badger.<sup>19</sup> Appearing for the first time at the turn of the century, these were-animals afflicted human communities with madness, illness, and disorder. By contrast to familiar animal spirits, shape-shifters had no ‘annals’, no ancestral or topological history and could not be mastered by a shaman.<sup>20</sup> Instead, alternative spirit possession methods were employed for neutralising or appeasing the mafa, whose brutal modus operandi was in direct contradiction to shamanic ethics and aesthetics.<sup>21</sup> Arguing for a connection between the rise of mafarism and the violent ushering in of modernity, Humphrey has underlined that, appearing at the turn of the century, this new, unorthodox cult was nonetheless applied to animals holding an already well-established cultural place. From this perspective, although Shirikogoroff makes no mention of it, the tarbagan makes for prime mafa material. Potanin’s 1875 *chetkur* variant could be precisely the mythological substratum on which a mafarisation of marmots took place. If this were indeed the case, the pre-existence of demonological beliefs as regards the tarbagan would affirm Humphrey’s opinion that shape-shifting lore, becoming central to native approaches of disastrous events in the turn of the century, was firmly based on already existing beliefs and practices.<sup>22</sup>

In the absence of hard evidence as regards the actual role of marmots in mafa cults in the region, this can but remain a tacit anthropological suggestion. Or indeed it may be best taken as a heuristic precaution, noting that the shape-shifting lore encountered by explorers and doctors at the turn of the century may have been a Mongol reaction to the encroaching and devastating effects of modernity. As such it should not be immediately and uncritically assumed to be an age-old tradition. Besides posing this open question for further investigation by ethnohistorians, what is pertinent for us here is to examine the fate of this shape-shifting lore and its mythological substratum in the hands of medical explorers and plague researchers in the region.

### MEDICAL MATERIALISM APPLIED

It is important to note that the first recorded variant of the marmot-transformation myth comes not from Potanin, but from the discoverer of the Siberian marmot, Gustav Radde. The interesting aspect assumed by this variant lies not only in the way in which it attributed certain collective guilt on the Mongols for ‘boasting’, but also in that it introduced

the theme of an eating prohibition regarding marmots. Radde's eloquent ethnographic description is worth quoting at length here:

An animal which, as we have seen, is important to the nomadic people of the high Gobi as food and clothing, has been connected by these people to their animal myths which are most widely known amongst them and have a certain poetic swing. They say that the bobacs in the dim past were humans, who worked the pastures arrogantly and boasted that they could kill all birds with the first shot. This boasting made the strongest of the evil spirits angry, so that he wanted to take revenge over this. He came amongst them and spoke to the best of the hunters: I want to see proof of your skill and will recognise it, when you kill a swallow on the flight with the first bullet. If you fail, however, I will punish you for your boasting. The brazen hunter loaded his weapon, the swallow flew, he shot. But only the middle of the swallow's tail was torn by the bullet. Since that time, say the steppe Tungus and Mongols, all swallows have a forked tail and the arrogant hunters were transformed by the anger of the evil spirit into marmots, which are animals and therefore edible except for a spot in the armpit. In this spot they demonstrate an usually somewhat lighter, whitish stain (which probably belongs to the hibernation gland), which is supposed to represent the human flesh, and they tear this out with great care before they eat the bobac.<sup>23</sup>

The 'armpit spot' referred to by Radde is no other than a body part of Siberian marmots known as *khun* (human), an axillary ganglion that is indeed usually removed by Mongols before cooking the tarbagan. Having as its object the last remaining part of the tarbagan that is human, *khun*-eating prohibition is a classic example of cannibalism avoidance. And yet, overlooking this ritual reading of the alimentary prohibition, Radde's Erkhei Mergen mythical variation was soon to become a terrain for medical materialist interpretations in the hands of influential medical authors.

Radde's description had a formative effect on Skrzhivan, a doctor appointed by the Russian Plague Commission to study the disease in Mongolia. Skrzhivan hired students from Urga to record 'Buryat epics' involving the mysterious tarbagan. Summing up data gathered from this ethnographic foray, in an article published in 1900, Skrzhivan presented readers with 'a fairy tale where you can probably find a hint of tarbagan disease—the risk of ingestion of lymphatic glands'.<sup>24</sup> The 'fairy tale' was, in fact, yet another variant of the myth regarding Erkhei Mergen's punishment for boasting about his archery skills. It reasserted the avoidance of the marmot's axillary gland by Mongol hunters who, Skrzhivan noted,

considered it to be ‘human meat’.<sup>25</sup> This interpretation was then taken a crucial step further by Georg Sticker who, in his influential book on plague, employed his usual embellishment technique in providing a bold exegesis of the prohibition:

The legend is childish; but the habit which is accentuated through it has a deeply serious reason. Legends which are kept alive among people, habits which they keep with religious zeal, always hide, even if they appear so strange and unexplainable to those who don’t know, an important truth. The tarbagan legend has remained until today an unheeded folk fiction. Now we can uncover its meaning.<sup>26</sup>

As seen by Sticker, the reason why Mongols avoided the marmot’s axillary gland was no other than plague prophylaxis. Following this interpretation, by considering marmot axillary glands (which were believed by medical researchers at the time to be prone to plague infection) to be taboo, Mongols effectively protected themselves from infection.<sup>27</sup> This was a typical medical materialist exercise, creating as it were a perfect match between myth and hygienic reality—if one can call reality the erroneous medical opinion that eating the cooked meat of a plague-infested marmot can actually give one the disease.<sup>28</sup> Dwelling on the sensational character of this naturalist correspondence, the same misinterpretation was subsequently reflected in a series of medical papers on plague, including an authoritative *Public Health Reports* article on the tarbagan by Paul Preble, assistant surgeon of the US public health and marine-hospital service.<sup>29</sup> In this way the embeddedness of the *khun*-eating prohibition’s relation to the wider tarbagan metamorphosis complex, as well as the implications of historical shifts in the latter during a time of monumental political, economic, and symbolic crisis, were all but lost. Instead of leading to an understanding of the way in which Mongols ‘organize their experience, in particular in relation to nonhumans’, or to an examination of the existential and social angst expressed and negotiated through tarbagan transformation mythology, the observation of this ritual prohibition was de-rooted of all its mythopoetic efficacy and reduced to a proto-hygienic mechanism.<sup>30</sup>

The medical materialist exegesis of shape-shifting tarbagan myths was not the monopoly of plague researchers, but seemed also to have an impact on ethnologists working in the area. This is evident in the work of Smolev, a Kyakhta-based scholar with a rich record of ethnological studies

of Mongol and Buryat legends and myths. Following a small plague outbreak in Mongolia in 1899, Smolev wrote a report on Buryat tarbagan legends for the local subsection of the Imperial Russian Society of Geography. Smolev did not claim to have discovered a new myth or mythic variant, but rather to have come across an already recorded legend, whose relation to the plague he wished to clarify.

This was no more than a recombination of variants of the Erkhei Mergen myth collected by Potanin two decades earlier. Yet, whilst Potanin's variants were listed as individual entries, with no relation between them established or explored, in Smolev's narrative these were merged in a mythic meta-narrative that incorporated the aforementioned *chetkur* variant. The latter was a major innovation in that in Potanin's original account the latter stood alone, with no stated relation to the Erkhei Mergen mythology. In Smolev's hybrid-variant, Erkhei Mergen is once again seen boasting that he can shoot down all the suns in the sky.<sup>31</sup> Following the usual trajectory of failure, submersion, and transformation, the archer is portrayed as uttering a curse: 'now people will look for me as a treat, but I swear that shall someone kill me with an arrow, then every time I shall go to the people and to cause human disease, and death.'<sup>32</sup> Reading this mythic utterance as a marmot-hunting prohibition, Smolev supported his interpretation by listing another variant of the same prohibition story. According to this rather confused story, Buryats opted never to hunt Siberian marmots with bow and arrow. This, Smolev explained, was on account of the belief that there once was a man who hit a tarbagan, but it managed to escape into its nest. The man foolishly tried to dig the marmot out, only to find in the ground not a demon, as in Potanin's *chetkur* variant, but a 'little man with bow and arrow'.<sup>33</sup> Rather than being an isolated episode of medical materialist exegesis at the imperial frontier, Smolev's story was to find international recognition in the writings of a protagonist of plague research, Heinrich von Jettmar. An Austrian doctor who before becoming globally known for his pioneering research on plague in Manchuria had already been a Transbaikalia old hand, Jettmar adapted Smolev's story in a way that portrayed the moment of subterranean transformation of the defeated Erkhei Mergen and the curse he is supposed to have uttered most colourfully: 'I will now retire for a long time under the earth, and there I shall not drink or eat. But I take with me my arrows. Woe to the wicked, who digs up my den and disrupts my sleep! My arrow will then meet the presumptuous disturbers of my rest!'<sup>34</sup> Jettmar was quick to conclude that 'these legends are probably the consequences of the observations on the



epidemiology of plague in Mongolian natives: the tarbagan is the true carrier and keeper of plague, that is, of the ‘arrow’ which meets all of those excavating the tarbagans or approach their *butans* [burrows] or disturb the beast somehow’.<sup>35</sup>

The appeal of this type of medical materialist exegesis of tarbagan-related myths for the international epidemiological elite is evident not only in Jettmar’s writings but also in the work of other well-known authorities such as Lister Institute’s illustrious researcher, George Ford Petrie. Best known for his research on rats and plague in Egypt, Petrie went as far as asserting that ‘Russian Cossacks’ also understood the danger entailed in handling the animal: ‘indeed their appreciation of the risk gives point in an interesting and unmistakable fashion to the tarbagan legends.’<sup>36</sup> In an even bolder attempt at comparative religion, Wu Liande, the leading plague expert in China, would connect the supposed ‘medical’ myth of the Mongols to data from a plague outbreak in 1907 at Atbashinsk District (in modern Kyrgyzstan). This was an outbreak caused by a ‘*black marmot* caught, killed and skinned by the first victim’. Following his authoritative *Treatise on Pneumonic Plague*, published under the auspices of the League of Nations, ‘the first reports reaching the authorities about this epidemic bore a legendary character’: the black marmot had allegedly called out at its captor in a human voice, warning him not to kill it. Noting the ‘similarity to the tales of Buryats and Mongols concerning the tarbagan’, Wu concluded that ‘it seems possible that the story was not an invention of the moment but conforms to an older tradition based on past experience’.<sup>37</sup>

Yet this medical materialist reduction of tarbagan-related myths to a hygienic, plague-prophylactic blueprint was not simply based on a bricolage of myths, rituals, and stories from across Central and Inner Asia. It was also an operation of naturalist enclosure diligent in ignoring and silencing myths about the animal that did not fit this exegetical scheme and, at the same time, in screening out indigenous explanations of outbreaks that contained no mention of marmots, such as the ones provided by prominent plague researchers in the region, like Jean-Jacques Matignon and Talko-Hryntsewich.<sup>38</sup>

### IGNORING INCONVENIENT ETHNOGRAPHIC DATA

Matignon’s 1898 research in Eastern Mongolia, which I will explore in detail in the next chapter, produced one of the most influential texts regarding the disease in the region. A military doctor stationed at the

French Legation of Peking, who became famous for his role during the siege of the city by the Boxers, Matignon demonstrated a keen interest in ethnography. Three years before his influential plague expedition to Eastern Mongolia, he studied Mongolian courtly medicine, interviewing the Living Buddha's chief physician in Urga.<sup>39</sup> For all his ethnographic interest, however, Matignon made no mention of marmot epizootics. The only reference to the animal in his report from Urga concerned the alleged anti-dysmenorrhoea properties of the targaban's internal organs.<sup>40</sup>

This is not to say that Matignon was indifferent to indigenous understandings of plague. On the contrary, in his popular monograph on 'Chinese superstition', the military doctor claimed that in the course of his plague expedition to Eastern Mongolia 'serious people recounted that [the 1896] plague was brought by a black bull, every night, some times before the appearance of the first cases, between 9 and 10 o'clock, bellowing in a terrible manner, pouring fire from his eyes and nose, having descended in gallop from the Mongolian plateau to the Selenga valley'.<sup>41</sup> Matignon claimed that many people in the region said they had seen the animal with their own eyes, a belief shared by a Chinese priest 'who despite practicing the Christian religion, was not totally stripped of Celestial superstition'.<sup>42</sup> As for the end of the epidemic, this was said to be indigenously attributed to the appearance of two red-tunic Lamas walking alongside the Selenga River radiating a resplendent flame from their headgear.<sup>43</sup> However, in spite of repetitive references to Matignon's plague work in turn-of-the-century plague literature, these ethnographic data, casting serious doubt as regards Mongols' supposedly unitary, proto-scientific response to and understanding of plague, were not reproduced or even acknowledged in the medical press.

A similarly selective omission of material from an oft-quoted work concerns the plague research of Julian Talko-Hryntsewich, the eminent Polish physician, anthropologist, and archaeologist. Stationed in the region since 1891, he was at the heart of marmot-plague research at the time.<sup>44</sup> In 1899 he was transferred to Urga on a plague-finding mission.<sup>45</sup> In his report to the Imperial Geographical Society's Kyakhta branch, he recounted the results of his 1899 expedition 'on the nature of the contagious disease that prevailed th[at] autumn in Mongolia'.<sup>46</sup> Talko-Hryntsewich attributed a plague outbreak in Mikhan-Guna to a group of Chinese and Mongols who, after digging ancient tombs in search of a treasure, as indicated by a Lama oracle, threw an eight-day-long celebration for their success. They feasted on mutton and targaban, which abounded in the region.

Although he admitted that he could not verify whether the *tarbagans* in question were ill, Talko-Hryntsewich informed readers that twenty men died the following day, and that people from neighbouring regions who came to see the treasure or partake in the feast were also infected.<sup>47</sup> As panic spread, the doctor claimed that ‘strict quarantine’ was imposed and the *yurts* of the inhabitants were burned. Yet rather than attributing this action to a spontaneous action from below, Talko-Hryntsewich claimed these measures were imposed by the Mongolian authorities.<sup>48</sup> As for the native population, it opted to bury the treasure back into the soil in order to appease the earth-spirit whose anger was understood to be the cause of the epidemic.<sup>49</sup> Following this outbreak narrative, it was explained that the first victims were the Lama oracle who got paid with the treasure-derived silver, and a Chinese debt collector, also paid from the same source. An alternative explanation given by native informants was that the outbreak was the result of a Lamaist curse: a recital, in Talko-Hryntsewich’s words, from the ‘*Kharanor* (the Black Book)’ commissioned by Chinese traders who had been recently evicted from the region.<sup>50</sup> If anything, this short ethnographic study by Talko-Hryntsewich is an excellent example of the entanglements of meaning, explanation, and interpretation surrounding outbreaks of infectious disease in human societies, where hardly ever a single aetiological narrative is able to assume discursive monopoly or overall hegemony.<sup>51</sup> It hence provides a picture of native lay and medical approaches of plague that is consistent with what we know from both historical studies of explanations and reactions to the disease in the Middle Ages and in early modern Europe, as well as from Florence Bretelle-Estabet’s recent review of South Chinese approaches of the disease at the turn of the nineteenth century: as irreducibly ambivalent and multifaceted understandings and practices embedded in internally complex, and often divided or antagonistic, social and cultural milieus.<sup>52</sup> However, once again these ethnographic data, and the ethnoepistemological complexity they revealed, were ignored and not reproduced or acknowledged in medical articles that otherwise quoted works where this information was relayed.

The ethnographic configuration of plague hence operated on two distinct but interrelated levels: first, on the level of field research; second, on the level of armchair reconstruction. On the first level we have various field-research practices incorporating in one way or another ethnographic methods. In other words, methods of more or less direct collection of information from and about individuals inhabiting an epidemic or endemic area. As we saw in the previous chapter, this data collection in the field

involved interviewing, distanced observation, participant observation, or a combination of the above. As regards plague research on the Chinese-Russian frontier at the turn of the century, this led to a broad output, including information about human relations to animal hosts as well as about habitation, burial rites, and social reactions to human epidemics. As Beliaevsky's research on plague in Transbaikalia was the first to propose a plausible zoonotic source, and was overall treated as a pivotal reference text on the disease in the region, his plague ethnography weighed heavily on the interpretation of further data collected after 1895.

This does not mean that all plague researchers sought to corroborate what through reproductions and adaptations of Beliaevsky's work became the native knowledge hypothesis, although many did. Several important works, such as the one by Matignon and Talko-Hryntsewich, provided ethnographic evidence about native perceptions of the cause of human plague, which were at odds with Beliaevsky's hypothesis. Yet when it came to the second level of ethnographic configuration, armchair assembling, selecting, summarising, and reconstructing evidence from the primary research literature, evidence that in one way or another was in dissonance to Beliaevsky's ethnographic model was silenced and ignored, even when the authors procuring it were otherwise celebrated and other aspects of their work endorsed. It is through this selective engineering of evidence that we end up with the authoritative meta-narrative of Georg Sticker, or, more recently, the assertion by William McNeill that Mongols 'had mythic explanations to justify the epidemiologically sound rules for dealing with the risk of bubonic infection from marmots', with which we began this chapter.

This medical materialist exegesis then portrayed a wide range of ideas and practices amongst Mongol and Buryats as a univocal belief system geared towards one single and overdetermining task: the prevention of plague infection.<sup>53</sup> Whereas this may at first glance appear as an affirmative strategy of representation, it has to be recognised, within its immediate ideological context, as a gesture that denied these groups any form of epistemological autonomy. A gesture that projected a single, totalising form of teleological knowledge (knowledge for survival) upon complex, multifaceted, and often contradictory mythological and cosmological systems. This was a medical materialist operation that alienated and subsumed human/non-human interactions amongst these groups within a pre-determined and hegemonic epistemological regime—a regime that defined in universal and unilateral terms what is knowable, what cannot

be ignored, and what can simply be imagined. This is, in other words, an operation of ontological enclosure, which, as Philippe Descola has noted regarding the wider impact of so-called ethnosciences, made it ‘increasingly difficult to escape from the illusion that the objectivation of reality is everywhere organized following a similar natural tendency the progress of which is blocked here and there by big blocks of magical thinking, moving testaments to a still imperfect recognition of the regularities of the physical world and an ambition to exercise firmer control over it’.<sup>54</sup>

### RITUAL INTO HYGIENE

One may ask here in a pragmatic spirit: but did such practices prevent plague from spreading or not? I have examined elsewhere why the practices described by Belivsky could only have a very limited efficacy in preventing the transmission of plague from marmots to humans.<sup>55</sup> Rather than returning to this discussion, I would like here to tackle the issue of a practice that could in fact have significant preventative results, so as to stress that we



Illustration 4 A Mongol yurt

should follow Mary Douglas in analytically differentiating between what that practice may have achieved and what it was meant to achieve.

In plague literature we often come across the allegation that Mongols and Buryats knew not only how to protect themselves from plague-infested marmots, but also how to stop the spread of the disease among humans by means of draconian isolation measures. This idea had its roots in Beliakovsky's 1895 article. Like other elements of the native knowledge hypothesis, it was taken up by a series of medical authors and was systematised and integrated in the outbreak narrative of the great Manchurian plague of 1910–11; there indigenous isolation practices were lauded as an exemplar of public-mindedness and self-discipline.

As I will explore in detail in the fifth chapter of this book, during the First International Plague Conference held at Mukden in April 1911, Mongol and Buryat hunters were hailed as bastions of vigilance against pneumonic plague contagion. In an enthusiastic response to an influential article by the British doctor Reginald Farrar, Dr Martini, Germany's delegate, seconded his colleague's praise of Mongols and Buryats. Martini claimed the latter's adoption of the practice to sew up plague patients in their yurts until no smoke rose from the top was 'excellent from the public health point of view'. Reading this as an effective public health measure guaranteeing collective survival, Martini noted: 'when that kind of procedure was operative, one did not require medical officers for health. It showed quite a virile public spirit.'<sup>56</sup> A similar interpretation of the practice of isolation was promoted by the Manchurian old hand Dr Roger Baron Budberg:

Naturally one cannot speak of any immunity of the Mongols. The fact that the plague is not spread as an epidemic amongst them and that we don't hear of any sickness amongst them is surely because these nomads have known since old times about the infectious nature of the disease. In case an incident appears, they certainly isolate the sick person immediately, as they do for cattle killed by an epidemic disease, and then move out of the infected area immediately.<sup>57</sup>

Mongols (and occasionally Buryats too) were thus praised for putting the common good above individual or familial sentiments, and for being able and eager to stay a human epidemic in much the same way as they stayed cattle epizootics. However, what underscored this outbreak narrative was its failure to understand ethnographic reality from an emic point of view: in other words, in the terms of the subjects so eagerly praised for their hygienic conscience. Rather than reflecting emic motives and ascribed

meanings of the above practices, this narrative presented what may be best summed up as a full reversal of ethnographic reality. It appears, from available non-medical ethnographic sources, that in case of various forms of affliction individuals were indeed kept alone in their family's abode. Coming from a bacteriologically habituated background, saturated by what Descola has termed a naturalist ontology, one is of course tempted to see this as a method of isolation, aimed at preventing contagion and protecting the larger community from the disease at hand. Yet this practice was applied to a great range of afflictions, many of which we would classify as non-infectious diseases or mental disorders, and not solely or primarily to what we see as transmissible disease. More importantly, however, we should seek to understand what medical materialist exegeses always neglect to take into consideration: native ascriptions of meaning. What was the stated reason for these practices? How were they made sense of? Paradoxical as this may sound to us, these appear to have been practices intended to protect the afflicted person from the community around him or her, rather than the other way around—the reason being that what we would consider as healthy individuals (or individuals at risk) were seen as threatening to further compromise the sufferer's spiritually fragile or susceptible condition.<sup>58</sup> Taking the native point of view seriously, we need to understand that the death of the afflicted person was often held to result from such unwarranted encounters.

Recognising the epistemological autonomy of social groups under historical or anthropological examination requires accepting that the systems of knowledge available and practiced by individuals in these groups are not only adequate for their survival and well-being but are also based on forms of rationality that allow for the development of dynamic social relations and cultural forms. This fundamental principle is violated by medical materialist exegesis, an operation based on the method of interpreting social action and cultural concepts as teleologically predetermined. The key to this operation is, of course, the notion of survival, as the ultimate goal of all life. And it is on the basis of this social evolutionist ideology that the need to explain away 'ritual avoidance' as 'avoidance of contagious disease' arises.<sup>59</sup> As we have seen, in order to achieve this, the social relation between the avoided and the avoider had to be reversed. Yet, the native knowledge hypothesis did not simply turn ethnographic reality on its head. It rather defined human life in the region as possible only to the extent that it spontaneously based itself on epidemiological principles of social

organisation. For the Mongols and the Buryats to be able to survive in an area where they depended upon a mammalian reservoir of plague, what was needed was to fashion their culture in accordance to plague-prevention. From this point of view, Mongolian and Buryat culture had to adapt to its plague-riddled environment in a way that approximated the ideal of cultural immunity to the disease. In this case, what Mary Douglas saw as the hygienic by-products of ritual action were not simply mistaken ‘as a sufficient explanation’ of Mongol and Buryat social life.<sup>60</sup> They were, in fact, rendered its biological precondition. Without these ‘cultural prophylactics’, if I may here coin a term to match the idea of ‘cultural vectors’ once again championed by epidemiologists today, groups inhabiting areas where plague was endemic were seen as condemned to perish under the bane of the dreaded disease.<sup>61</sup>

## NOTES

1. W.H. McNeill, *Plagues and Peoples* (London: Penguin, 1976), p. 146. McNeill fails to explain how this accounts for his (much-contested today) theory regarding the Mongol origins of the Black Death.
2. M. Douglas, *Purity and Danger, An Analysis of the Concepts of Pollution and Taboo* (London: Routledge, 1993), p. 29.
3. Douglas, *Purity and Danger*, p. 29.
4. Douglas, *Purity and Danger*, p. 29.
5. Douglas, *Purity and Danger*, p. 29.
6. Douglas, *Purity and Danger*, p. 29.
7. W. James, *The Varieties of Religious Experience: A Study in Human Nature* (Rockville: Arc Manor, 2008), p. 19; Douglas explicitly acknowledges James in her work (Douglas 1993: 32).
8. G. Golubev, *Zhitie Daniila Zabolotnogo* (Moscow: Molodaya Gvardiya, 1962).
9. Golubev, *Zhitie Daniila Zabolotnogo*, p. 39. This method of catching the marmot was first noted by Cherkassov (2012) who, however, noted that skins other than marmots’ were preferred, as using the latter often resulted in hunters simply killing each other in misrecognition. See also: Carruthers with Miller (1913: 342), Loukashkin (1937b), Pegg (2001: 246), Vainshtein (1980: 182). We must be careful here so as not to confuse or confound such descriptions of hunting marmots with methods used in



hunting animals further north in Siberia, which have been recently described by Willerslev as ‘mimetic hunting’—a practice embedded in Siberian animism. Mimetic hunting involves a subtle yet dynamic dialectic of identity and alterity between prey and predator, predicated upon the spiritual continuity of the two agents (Willerslev 2007). William Summers’s recent book on the Manchurian plague perpetuates this misinterpretation by linking such practices to shamanism (Summers 2012: 123). There is no historical ethnographic evidence to support such link. The fact that the ‘marmot-hunting dance’ is not related to mimetic hunting becomes clear from an account by Roy Chapman Andrews (1921) where the associate curator of the American Museum of Natural History and leader of its second Asiatic expedition describes the use of a dog-skin in a similar manner.

10. Golubev, *Zhitie Daniila Zabolotnogo*, p. 39.
11. G.N. Potanin, *Ocherki Severo-Zapadnoi Mongolii*, 4 vols. (St Petersburg: V. Bezobrazov, 1881–1883).
12. Erkhei means ‘thumbs’, while mergen ‘sharp-shooter’ or ‘archer’ in Mongolian.
13. For a contextualisation of the myth within Mongolian cosmology see Nassen-Bayer and Stuart (1992). The myth is closely related to the Chinese myth of Houyi, a mythic archer punished for shooting dead nine of the ten suns (Birrell 1993); I would like to thank Marta Hanson for bringing this to my attention.
14. G.N. Potanin, *Ocherki Severo-Zapadnoi Mongolii*, 4 vols. (St Petersburg: V. Bezobrazov, 1883) vol. 4, p. 767.
15. G.N. Potanin, *Ocherki Severo-Zapadnoi Mongolii*, 4 vols. (St Petersburg: V. Bezobrazov, 1881) vol. 2, p. 152.
16. C. Gardener, ‘Folk-Lore in Mongolia,’ *The Folk-Lore Journal*, vol. 3, no. 4 (1895), p. 319. The only dissonance is that Potanin’s original text spells ‘chitkur’ rather than ‘chetkur’, and ‘khoshun’ rather than ‘gachoun’.
17. Golubev, *Zitniye Daniila Zabolotnogo*, p. 39.
18. S.M. Shirokogoroff, *Psychomental Complex of the Tungus* (London: Paul Kegan, 1935); C. Humphrey with Urunge Onon, *Shamans and Elders: Experience, Knowledge, and Power Among the Daur Mongols* (Clarendon Press: Oxford 1996), p. 329.
19. Humphrey with Urunge Onon, *Shamans and Elders*, p. 329.
20. Humphrey with Urunge Onon, *Shamans and Elders*, p. 333.
21. Humphrey with Urunge Onon, *Shamans and Elders*, p. 333.
22. On other were-animal-related beliefs and their relation to plague outbreaks in North China see Haylen (2004: 159–160).

23. G. Radde, *Reisen im Süden von Ost-Sibirien in den Jahren 1855–1859*, 2 vols. (St Petersburg: Buchdruckerei der Kaiserlichen Akademie der Wissenschaften, 1862), vol. 2, pp. 168–169. On Radde's use of the term *bobac* see Chap. 2, note 8.
24. Skrzhivan, F., 'Nashi svêdênïya o tarabagan'eï chumê,' *Russkïi arkhiv patologïi, klinicheskoi meditsinui i bakteriologii* (July 30 190), p. 610.
25. Skrzhivan, 'Nashi svêdênïya o tarabagan'eï chumê,' p. 611.
26. G. Sticker, *Abhandlungen aus der Seuchengeschichte und Seuchenlehre*, 2 vols. (Gießen: Töpelmann, 1908), vol. 1, pp. 404–405.
27. In a recent article, avoiding any medicalising interpretation, Dimitriev et al. (2001) have argued that the separation of the gland from the rest of the meat was a separation of the spiritual aspect of the tarbagan from its material aspect.
28. The raw or inadequately cooked meat of any plague-infected animal can of course lead to human infection.
29. Writing in the aftermath of the great Manchurian plague of 1910–1911, Preble (1912: 36) claimed that 'in a majority of the diseased tarbagans, a tense reddish swelling is found under the shoulder'.
30. Ph. Descola, *Beyond Nature and Culture* (Chicago: Chicago University Press, 2013), p. 364.
31. Ya. S. Smolev, 'Buryatskaya legenda o tarbagane,' *Trudui Troitskosavsko-Kyakhhtinskago otdelênïya Priamurskago otdêla Imperatorskago Russkago geograficheskago obshchestva*, vol. 3 (1900–1901): 101–103.
32. Smolev, 'Buryatskaya legenda o tarbagane,' p. 102.
33. Smolev, 'Buryatskaya legenda o tarbagane,' pp. 102–103. This is the only extant record of such prohibition; it contradicts all known ethnographic information regarding marmot hunting in the region. It is possible that Smolev confused the tarbagan with a creature known in Mongolia as *tarbagan khun*—marmot-man or marmot-person, indicating a type of dwarf or gnome (see Humphrey 2013).
34. In G. Kaminski, *Pestarzt in China: das Abenteuerliche Leben des Dr. Heinrich Jettmar* (Vienna: Löcker, 2010), p. 40.
35. Kaminski, *Pestarzt in China*, p. 41. It should be noted that for European authors the long-standing link of plague with arrows since classical antiquity (including St Sebastian's martyrdom in the aftermath of the Black Death) would make this association very appealing. On pre-modern plague iconography see Boeckl (2000). On an explicit link between Apollo Smintheus and Transbaikalian plague-carrying rodents see Powell (1929).
36. G.F. Petrie, 'A Commentary on Recent Plague Investigations in Transbaikalia and Southern Russia,' *The Journal of Hygiene*, vol. 22, no. 4 (July 1924), p. 400. For Petrie's work in Egypt see Bacot et al. (1914).

37. Wu L.-T., *Treatise on Pneumonic Plague* (Geneva: League of Nations, 1926), pp. 66–67, emphasis in the original.
38. See Chap. 4. The treasure-digging attribution to plague was mentioned, in passing only, by Sticker (1908: 121).
39. J.-J. Matignon, ‘La médecine des Mongoles,’ *Archives cliniques de Bordeaux*, vol. 4, no. 11 (1895): 515–523.
40. Matignon, ‘La médecine des Mongoles,’ p. 522.
41. J.-J. Matignon, *Superstition, Crime et Misère en Chine (Souvenirs de Biologie Sociale)* (Paris: Masson et cie. 1899), p. 29.
42. Matignon, *Superstition, Crime et Misère en Chine*, p. 29.
43. Matignon, *Superstition, Crime et Misère en Chine*, p. 30.
44. On a contemporary account of plague research in Transbaikalia see Rudenko (1901).
45. J. Supady, ‘Julian Talko-Hryniewicz—Physician, Anthropologist and Siberia Explorer,’ *Polskie archiwum medycyny wewnętrznzej*, vol. 117 (November-December 2007): 531–533. For a detailed review of his work in Transbaikalia, see Eil’bart (2001).
46. Yu. D. Tal’ko-Gryntsevich, ‘O chumnuikh zabolévaniyakh v Mongolii,’ *Trudni Troitskosavsko-Kyakhtinskago otdelēniya Priamurskago otdēla Imperatorskago Russkago Geograficheskago Obshchestva*, vol. 1, no. 1–2 (1899), p. 96.
47. Tal’ko-Gryntsevich, ‘O chumnuikh zabolévaniyakh v Mongolii,’ p. 97; It is obvious, given the necessary incubation period of plague (see Introduction) that either Talko-Hryniewicz’s information was incorrect or the disease in question was not plague.
48. Tal’ko-Gryntsevich, ‘O chumnuikh zabolévaniyakh v Mongolii,’ p. 98.
49. Talko-Hryniewicz, ‘O chumnuikh zabolévaniyakh v Mongolii,’ p. 98.
50. Tal’ko-Gryntsevich, ‘O chumnuikh zabolévaniyakh v Mongolii,’ p. 97. Similarly other ethnographic data, such as Mongolian riddles where the tarbagan is commonly depicted as an amber-coloured, meditating Lama or a Buddhist pagoda, or records on the use of live tarbagans as tributary items in the imperial past, are also conspicuously ignored in the medical materialist literature (Taylor 1954; Franke 1983; Schafer 1985).
51. For a classic ethnographic example of this see Evans-Pritchard (1937).
52. R. Horrox (ed.), *The Black Death* (Manchester: Manchester University Press, 1994); S.K. Cohn, *Cultures of Plague: Medical thinking at the end of the Renaissance* (Oxford: Oxford University Press, 2010); D. Gentilcore, ‘Purging Filth: Plague and responses to it in Rome Florence 1656–57.’ In M. Bradley (ed.) *Rome, Pollution and Propriety: Dirt, Disease and Hygiene in the Eternal City from Antiquity to Modernity*, pp. 153–168 (Cambridge: Cambridge University Press, 2012); Fl. Bretelle-Establet, ‘Les épidémies

- en Chine à la croisée des savoirs et des imaginaires: le Grand Sud aux xviii<sup>e</sup> et xix<sup>e</sup> siècles,' *Extrême-Orient Extrême-Occident*, vol. 34 (2014): 21–60. For earlier analyses of responses and understandings of plague in South China see Benedict (1996) and Katz (1995).
53. In this respect, it was an operation similar to other colonial misrecognitions of plural systems of native knowledge and belief, including therapeutic systems and practices. This also ignored the exchange of ideas between the groups in question and neighbouring groups, a process sometime referred to as hybridisation. For a discussion of this mode of unifying native multiple systems of knowledge into a spectacle of unified culture as applied to pharmacology in colonial India see Mukharji (2008).
  54. Ph. Descola, *Beyond Nature and Culture* (Chicago: Chicago University Press, 2013), p. 83.
  55. C. Lynteris, 'Ignoring Native Ignorance: Epidemiological Enclosures of Not-Knowing Plague in Inner Asia.' In Roy Dilley & Thomas (eds) *The Anthropology of Ignorance* (Oxford: Berghahn Press, 2015).
  56. In R. Farrar, 'Plague in Manchuria,' *Proceedings of the Royal Society of Medicine*, vol. 5 (Section of Epidemiology and State Medicine) (October 1911), p. 23.
  57. R.B. Budberg, *Bilder aus der Zeit der Lungenpest-Epidemien in der Mandschurei 1910/11 und 1921*. (Hamburg: Verlag Von Conrad Behre, 1923), pp. 292–293. Budberg admitted that '[r]egrettably I was always too much bound to Harbin, I never had the possibility and opportunity to collect observations on location in Mongolia' (1923: 128). On Budberg's work in Manchuria see Gamsa (2010).
  58. N. Ikeshiri, *Da wo er zu (The Daur Nationality)* (Daur History, Language and Literature Society: Hohhot, 1943/1982). See also: Humphrey with Urunge Onon (2006).
  59. Douglas, *Purity and Danger*, p. 30.
  60. Douglas, *Purity and Danger*, p. 30.
  61. For a fuller discussion of 'culture vectors' see Conclusion. On a similar evolutionary epidemiological perspective as regards plague in the region under examination see Formozov et al. (1994).

## Ethno-Geographic Entanglements

The ethnographic configuration of plague on the Chinese-Russian frontier was not limited to zoonotic aspects of the disease, or to the exploration of human relations with non-human hosts of the disease. Of equal importance was the role of ethnography in the problematisation of the locus and the transregional or international directionality of plague. Prior to the 1910–11 outbreak in Manchuria, the geographic configuration of plague was developed by medical experts who contended for the explanation of three vital and interlinked questions that formed part of the wider understanding of the third plague pandemic: where did plague come from? In which areas was plague endemic? What was the direction of the spread of plague and why? Ethnographic data provided key elements for answering these questions, contributing to a global debate about the origins, persistence, and transmissibility of plague in the course of the third pandemic.

This chapter will examine consecutive attempts to draw an ethno-geographic problematisation of plague in the region, based on an epidemiological reasoning about the disease that considered the location and geography of socio-cultural practices as key aspects of the epidemiology of the disease. It will do so by examining three pivotal plague research projects in the region. Starting with the examination of how this epidemiological rationality was manifested in the research of the highly influential French military doctor Jean-Jacques Matignon, I will then move to consider how it was consequently developed by Russia's most illustrious plague expert, Danilo Kirilovich Zabolotny, and how it reached its paradoxical conclusion with the work of Ivan Stepanovich Dudchenko-Kolbasenko.

Central to these problematisations was the question regarding the transmissible nature of the disease. The discussion around the contagiousness of plague was already centuries old by the 1890s, with the debate having been famously developed between the sixteenth and the nineteenth century by medical luminaries like Girolamo Fracastoro and Richard Mead.<sup>1</sup> These theories came under attack in the early nineteenth century in the context of prevalent miasmatic theories of epidemic disease, with Napoleon Bonaparte's encounter with plague in Jaffa during the Egyptian Campaign in 1804 functioning as an emblematic case against proponents of contagionism.<sup>2</sup> Still the idea that this was a head-on battle, with miasma and contagion as mutually exclusive explanations, is erroneous. Besides the hubbub created by zealots of each theory, the two ideas often mingled in aetiological discourses and explanations of outbreaks.<sup>3</sup> Already present in the definition of plague in the first edition of *L'Encyclopédie*, this dialogical co-existence of miasma and contagion is most pronounced in the forty-first volume of the *Dictionnaire des sciences médicales*. Published in Paris in 1820, this pivotal work of early nineteenth-century medicine defended at length the miasmatic origins of plague. And yet, at the same time it defined it as 'an eminently contagious disease'.<sup>4</sup> Stressing that they originally arose as telluric miasmata produced in Lower Egypt, the dictionary explained that plagues thence arrived in Europe by being 'transmitted through the communication of Saracens, Arabs, Moors and Turks with Europeans'.<sup>5</sup> This problematisation set the scene for the two key questions in the course of the third pandemic: where did plague reside, and how did it spread? The first question related to what we have today come to call endemic foci of the disease, or what by the mid-nineteenth century were called the 'seats' of plague.<sup>6</sup> It concerned identifying areas, landscapes, or soils where plague resided diachronically. These were configured as regions where small outbreaks of plague were constant and from where plague travelled to other loci where it would, for a variety of reasons, assume the form of larger-scale epidemics. The second question, as Mark Harrison has recently demonstrated, related to pressing debates on whether plague could be carried (or even generated) by objects as well as by people; these raged ever stronger as plague struck a series of important commercial loci in the Middle East and Persia after 1850.<sup>7</sup> With the question of plague's contagious nature assuming vital importance for international trade under the bane of quarantine laws, the interrelation of the two aspects of plague (we would today call them its endemicity and epidemicity) assumed global geopolitical and biopolitical importance. It

is in the context of and in relation to these global debates that plague research on the Chinese-Russian frontier articulated an innovative, if in places perplexing, interrelation between the ethnographic and geographic profile of the disease.

### IMPORTING PLAGUE

Jean-Jacques Matignon was born in Eynesse of Gironde in 1866. He graduated with a medical degree from Bordeaux in 1892 and was stationed in Beijing as an army doctor attached to the French Foreign Legation in 1895. Matignon was a prolific author on a wide array of medical issues in China, dwelling principally on exotic and sensational topics: Forbidden City eunuchs, ‘acromegalo-gigantism’, foot-binding, Japanese maladaptability to Western boots, the Kangxi Emperor’s ‘neuropathy’, an alleged anti-leper pogrom in Nanjing, and a type of food poisoning, which he named atriplicism, inflicting North Chinese beggars subsiding on an unwashed spinach-like herb.<sup>8</sup> This medico-orientalist trajectory reached its apex in the publication of his magnum opus on Chinese ‘superstition, crime and poverty’.<sup>9</sup> At the same time, Matignon was a practical man. He was active in the Peking Hospital as well as in medical expeditions to Manchuria, covering public health aspects of the Russian-Japanese War.<sup>10</sup> Yet what sealed Matignon’s international fame was his role as acting chief doctor of the Peking Foreign Legations during the Boxer siege of 1899–1900. All in all it may be said that by 1905 Matignon was one of the most internationally recognised and admired French doctors operating in the Qing Empire.

It is not quite clear what first drew Matignon’s attention north of the Great Chinese Wall. As noted in the previous chapter, Matignon developed a long-standing ethnographic interest in the region, which included a brief research field trip to Uрга. Whereas in the Mongolian capital Matignon investigated courtly medicine, in the case of his better-known research in Eastern Mongolia his interest consisted in the study of bubonic plague. The results of his plague expedition were first presented to the French Academy of Sciences in December 1897 and published in an extensive article in the *Annales d’hygiène et médecine légale* in 1898, and republished a year later in the *Archives de médecine et de pharmacie militaires*.<sup>11</sup>

Matignon began his paper, *La peste bubonique en Mongolie*, by giving a brief account of the displacement of Mongols by the Chinese and their confinement in ‘the great plateaux of Mongolia’.<sup>12</sup> Describing the

mountain ranges separating China from Mongolia, Matignon noted that the region, which was once covered in woods, now stood completely deforested as a result of the recent authorisation of Chinese migration by the ruling Qing.<sup>13</sup> Twelve days' horse ride away from Beijing to the north-east took Matignon to the valley of Sô-leu-kôn (Selenga) and the village of Toung-kia-Yng-tze, where 'since nine years reigns the bubonic plague'.<sup>14</sup> Matignon wrote that the population of the valley was entirely Chinese: 'the first colonisers consisted in Christians from the Manchurian border, who appealed to workers, mainly from the province of Shandong, to help them in their work of deforestation.'<sup>15</sup>

These Shandong migrants were looked down upon by the French doctor as paragons of hygienic backwardness. This discourse is typical of what I have described as the negative pole of colonial strategies of representation regarding infectious disease and native subjects. As Adrien Proust would put it, 'plague develops in the midst of populations that have been degraded by profound physical and moral misery. This is the essential condition for the generation of plague'.<sup>16</sup> Matignon hence provided what colonial doctors across the globe would recognise as the ideal setting of an outbreak: 'the hygienic conditions in which this population lives are most faulty. The houses are filthy slums, real huts of savages, made of mud walls and thatched roof.'<sup>17</sup> Describing the structure of the *huttes des sauvages* in detail, Matignon stressed the role of the heated floor-bed commonly found in the region (*kang*, spelled by Matignon as *kahn*)—a structure that was to be vilified by hygienists for many decades.<sup>18</sup> 'Come winter', he wrote, 'everything is carefully sealed and the air will not be renewed until the return of spring.'<sup>19</sup> Crowdedness in the local houses, and the tendency to keep sick members of the family indoors alongside healthy individuals, was seen as generating a 'revolting bodily filth' hence providing 'an excellent terrain' for the development of typhus, smallpox, and trachoma.<sup>20</sup> Matignon decried that nowhere else in China or Korea had he seen such 'sordid people': 'most inhabitants wash their body but once a year. Soap is a thing absolutely unknown.'<sup>21</sup> Reflecting a wider international debate regarding the role of clothing items in the spread of plague, Matignon noted that 'the clothes are a block of dirt, worn for years, until they fall, somehow, in deliquescence'.<sup>22</sup> He also stressed that the garments of those who died of 'contagious disease are not disinfected nor washed and are worn by some member of the family of the deceased'.<sup>23</sup> This range of ethnographic details touched upon by Matignon was typical of attempts



to problematise and decipher plague transmissibility during the first years of the third pandemic.<sup>24</sup> Another example of this aspect of Matignon's ethnographic gaze is his concern over the fate of the corpses of plague victims, an issue that, as we have already seen, preoccupied the international medical community as well as colonial administrators at the time.<sup>25</sup> Matignon underlined that, in general, corpses were so carelessly buried that the first big rain brought them up to the surface.<sup>26</sup> This attitude supposedly reached a climax in the context of mortality crises when, Matignon claimed, corpses were simply thrown down a ravine nearby the village where they were devoured by wolves when the night fell.<sup>27</sup> These ethnographic conditions, Matignon argued, were ideal for the development of plague: 'the habitats, houses, and soil are all receptacles in which Yersin's bacillus can easily wait for an opportunity to manifest its virulence.'<sup>28</sup>

The discourse is typical of the high noon of the third plague pandemic. Turning his ethnographic gaze to the 'breeding grounds' of the disease, Matignon was in dialogue with concerns regarding the proper medium of plague, especially these arising in the context of the epidemic in India, where the idea that plague passed from active phases of epidemicity to quiescent phases of dormancy and vice versa was hotly discussed.<sup>29</sup> What is important is that whilst Matignon related to this mode of problematisation, he also clearly favoured a contagion approach of plague. He thus claimed that to such general factors contributing to the spread of the disease one should add a more particular one: once plague appeared in a household, it was not only the parents who stayed in the house in constant contact with the sick, but also neighbours who visited, 'passing the hour, talking, smoking [and] drinking in the chamber of the plague-inflicted'.<sup>30</sup> Only after the epidemic reached great proportions, like in 1896, did fear force people to avoid patients. 'Plague-pneumonia', as Matignon described the prevalent form of the disease following medical conventions at the time, was seen as easily transported in the form of sputum sticking on clothes, shoes, and human hands: 'I saw a man removing with his fingers from the mouth of his daughter sputum so sticky that it stuck to the teeth and lips. This accomplished, our man wiped his hands on his pants and after a while, without washing, began to eat.'<sup>31</sup>

In supporting the idea that plague was a contagious disease, Matignon was interested in, and indeed had to stand by, a theory of where the disease came from. Plague, according to Matignon, first appeared in the village of Yan-che-kou, at the north-west of Toung-kia-Yng-tze in September 1888, when a twenty-year-old girl, who had never left the small valley, became

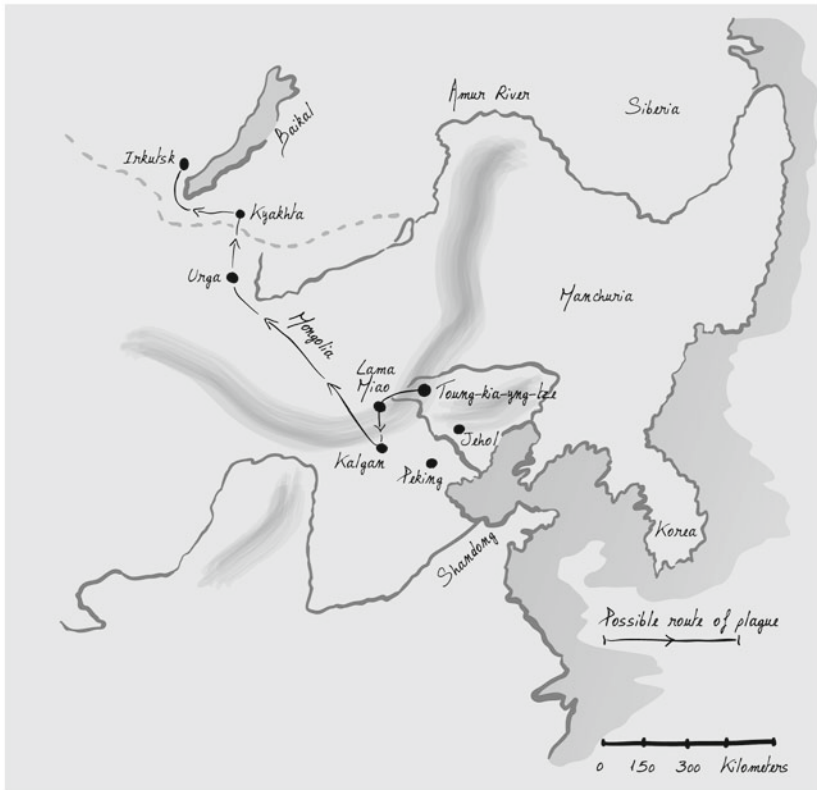
the first victim of the disease. Yet this was not deemed to be a ‘spontaneous’ outbreak. The pestilence, Matignon noted, ‘seems to me to have been imported by the workers, who, every year, arriving in spring from the southern provinces, mainly that of Shandong, to help the natives in their work’.<sup>32</sup> Matignon noted that Shandong was not a foyer of plague—hence the fact that Shandong ‘coolies’ were, supposedly, the true importers of the disease could only mean that they somehow brought plague with them (in their bodies or in items they carried or wore) from further afield. This was indeed Matignon’s bold theory of importation. As, he reasoned, a large number of these individuals were employed in cabotage across the numerous ports of East and South China, it was easy to see how the actual source of plague was Amoy and Canton, infamous ‘centres of plague’.<sup>33</sup> Shandong coolies loaded and unloaded cargo, containing clothes from regions and perhaps people who had fallen victim to the disease. These items were then purchased by migrant workers and brought north where plague spread, in a manner not dissimilar to the one assumed by August Hirsch in the case of the 1878–79 Vetlianka outbreak: ‘these clothes, purchased by workers, who go to Mongolia, have been used as a vehicle for germs of epidemic disease that seems now to have such strong roots in the valley of Sò-leu-kôn.’<sup>34</sup> Hence Matignon proposed that plague was not native to Eastern Mongolia, but was rather imported there from South China on the backs of pestilent coolies.

Matignon was interested in the geography of epidemics, and engaged in what Marta Hanson has called the ‘visualization of the geography of diseases’ through the production of plague maps.<sup>35</sup> In her exploration of plague cartography in China, Hanson follows Tom Koch in underlining how ‘disease mapping became an increasingly important tool for physicians working out answers to the pressing epidemiological questions of the day’.<sup>36</sup> In particular, Hanson has focused on how these maps ‘vizualize[d] current conceptions of knowledge’.<sup>37</sup> Yet medical mapping was not simply ‘a way of thinking through a problem’. It was also used as a support for colonial interventions in disease-stricken areas. Much like photomicrographic images or statistical diagrams, plague maps were utilised as ‘statements in an argument and as evidence furthering a specific hypothesis’ but also as ‘technologies of power that legitimated colonial control’. Taking a step further, we should here follow recent developments in the anthropology of cartography so as to consider plague maps as parts of an epidemiological performance, in the sense that they supported a configuration of plague that ‘link[ed] things through territory by fusing onto a common

plane (that of the map) multicoded images of the very world the map itself brings into being'.<sup>38</sup> The importance of this anthropological perspective consists in that it underlines a vital operation of maps beyond the realm of representation. By 'fusing signs' such as routes of transport, landscape features, habitat zones, and national borders into a dialogical plane of signification, maps 'encode these links' as pertaining to a single irreducible fact: plague.<sup>39</sup> Furthermore, they encode a set of relations that is deemed essential for anyone to be able to assert knowledge of plague in a given geographical area. They thus function, at one and the same time, as apparatuses that bring discrete elements of the environment and of human society together as aspects of an epidemic disease, and as apparatuses that confer the rules of veridiction regarding scientific statements about that disease.

Matignon's work was organised around two maps, both of which 'tri[ed] to isolate the underlying causes [of plague] via directionality and temporality'. The first map traced the progress of plague in Selenga valley, in 1896 and 1897.<sup>40</sup> In a fashion similar to the maps developed by Patrick Manson and Emile Rocher in mapping the spread of plague in Yunnan twenty years earlier, Matignon drew arrows showing the imagined spread of plague between Eastern Mongolian villages over time.<sup>41</sup> The second map (Map 4.1) provided a larger view of China, clearly demarcating Yunnan and the area around Canton as 'centres of plague', with no equivalent 'centre' in North China or beyond. In this it did not contradict the prevalent theory at the time regarding the origin of the third pandemic. The map suggested that, having reached the region from the south, plague then spread from Toung-kia-Yng-tze through Mongolia and Kyakhta (misspelled *Kiarta*) all the way to Irkutsk.<sup>42</sup> This indicated that Matignon had probably come across literature on plague in Transbaikalia, although, in fact, no plague outbreak had been reported in the city of Irkutsk itself. Moreover it suggested that he considered Eastern Mongolia as a transit point, rather than as an endemic centre of the disease.<sup>43</sup>

To the looming questions of whether plague, once imported by Shandong migrants and their clothes, was established as an endemic disease in the region, and whether it spread to animals and transform them into some sort of reservoir, Matignon simply noted that no rat epizootic accompanied the epidemics, although a great number of flies seemed to be lying dead in the chambers of plague-inflicted patients.<sup>44</sup> Here his identification of the disease as contagious is explicit: 'a grave typhoid disease, almost always complicated by buboes, sometimes by expectorations simi-



**Map 4.1** Hand-drawn map by Jean-Jacques Matignon showing the spread of plague from North China to Transbaikalia (Source: Archives de l' Institut Pasteur, Cote: IND.A1—Lieu: 4/151-153; map illustration created by Mick Cafferkey, Senior Illustrator, University of Cambridge)

lar to these of pneumonia, with very rapid development [and] fatal termination in 99 out of 100 cases: this is how Mongolian plague may be defined.<sup>245</sup> Matignon was confronted with the classic clinical diagnostic problem regarding pneumonic plague: a pulmonary condition resembling many other infections, with an erratic appearance of buboes, which were usually symptomatic only in the first victim of the disease (developing first bubonic and then secondary pneumonic infection), or, in rare cases, in patients suffering from pneumonic plague relapsing into bubonic form. In a stroke of medical insight, Matignon readily acknowledged this problem, and went on to provide some detailed clinical case studies of plague, which

he observed in the field. An advocate of drastic measures, he proposed that the only way of eradicating plague in the region was burning the Chinese migrant houses with all their possessions inside, as 'their value is anyway minimal', warning, at the same time, that this being accomplished, graves would still pose a source of infection.<sup>46</sup>

Drawing on ethnographic data, Matignon reasoned that the limited needs of Selenga valley inhabitants 'have so far been the best obstacle in the spread of plague. These Chinese seldom exit the valley, soil products being sufficient for their needs'.<sup>47</sup> However, he feared that protracted contact with commercial centres could pose an international public health danger, with the disease spreading to Russia in the north through the large trading centre of Lama-miao, which was in direct communication with Kalgan, the main Chinese hub for tea and skin exports from China to Russia via Mongolia. 'The day or the occasion when plague reaches Kalgan', Matignon reasoned, 'Russia will be seriously menaced.'<sup>48</sup> Hence, by bringing ethnographic and geographical data in rapport, Matignon perceived Eastern Mongolia as a potential transfer node of bubonic plague from South China to Mongolia and Transbaikalia. In this sense we can say that, focused on the role of trade in spreading plague, Matignon followed the orthodox epidemiological problematisation of the infection during the third plague pandemic: he added next to the great plague-spreading harbours of the south (Hong Kong and Bombay)<sup>49</sup> a continental sibling, which by means of caravan-led commerce could spread the disease along the ancient tea and fur trading routes of Tartary.

### MATIGNON'S INTERNATIONAL RECEPTION

Matignon's study of plague in Eastern Mongolia had a significant impact on international plague literature at the time, as evident a year later in the Twenty-Eighth Annual Report of Bruce Low, a highly influential medical officer of the British Empire.<sup>50</sup> Low placed emphasis on trade and dirt as factors in the generation and spread of the disease. As Robert Peckham has demonstrated in his recent work on the third plague pandemic, the two categories were intricately linked in the colonial imagination of pestilence, with the 'conflated idiom of trade and plague function[ing] at once as metaphor, even as it reflected the practical challenges posed by potentially-contaminated goods'.<sup>51</sup> Drawing on Matignon's report, Low stressed that these unhygienic subjects were not indigenous to the region but 'a people who originally came from the province of Shandong, or the frontier of

Manchuria'; roaming labourers who 'occasionally go to So-len-ko Valley to assist in agricultural work'.<sup>52</sup> A crucial event validating Matignon's importation theory in the eyes of the Colonial Office were the 1899 and 1901–02 bubonic plague outbreaks in the free harbour of Newchwang (Yingkou) in Southern Manchuria.<sup>53</sup> The link was retrospective insofar as Matignon's findings came before rather than after the manifestation of plague in the Manchurian harbour; still, it appeared both logically and geographically evident from the perspective of the imagined pathogeny of 'imperial interconnectedness', in Peckham's sense of the term.<sup>54</sup>

Medical journals were also quick to report Matignon's findings. In May 1898 *The British Medical Journal* made brief note of his expedition. Two years later (August 1900), possibly in response to the Chinatown plague crises in Honolulu and San Francisco, it returned to the subject, putting emphasis on the cramped and dirty Chinese dwelling conditions. The journal repeated Matignon's warning that 'the new [plague] centre is a considerable menace to Russia, which has a large trade in tea and skins with Kalgan, which is quite near'.<sup>55</sup> The same year Matignon's findings were presented to the medical world in Franck Clemow's highly influential article to *The Journal of Tropical Medicine*. As already mentioned, this paper was primarily a comprehensive review of Russian research on the tarbagan origins of plague in Transbaikalia.<sup>56</sup> More than that, however, Clemow was the first English-language author to bring Matignon's importation hypothesis face-to-face with the Russian tarbagan hypothesis. In an effort to balance the two theories and their mutually contradictory origin hypotheses, he argued for the existence of two epicentres of plague on the Chinese-Russian border region: on the one hand, Transbaikalia, where the disease was carried by marmots, and on the other hand, Eastern Mongolia, where plague appeared to have no such zoonotic link, as 'if these animals take any share at all in the spread of the disease, they do so to an incomparably less extent than in the case of the Siberian disease'.<sup>57</sup> Clemow warned his readers that despite similarities, the two plague epicentres differed profoundly:

The Siberian centre is, apparently, in a flat steppe country; the Mongolian in a valley amongst hills, and at a considerable height above sea-level. In the former the disease occurs only in certain years, in the autumn, and solely from contact with infected *tarbagans*. In the latter it breaks out every year, in the summer, and quite independently [...] of disease in the lower animals. In the former it attacks only the members of the household of the first per-

son affected, or others who have come into immediate contact with him or them, and each outbreak is thus limited to a clearly defined group of cases; in the latter it becomes epidemic over a considerable area, it is erratic in its course, it attacks a very large proportion of the entire community in a number of separate houses and separate villages, and there is no proof, apparently, that each fresh case is due to direct contact with a preceding case.<sup>58</sup>

The keen plague-watcher did not overlook the possibility of a connection between the two plague zones, although his support of this scenario was so haphazard as to attract the indignation of an anonymous correspondent in *The British Medical Journal*.<sup>59</sup> What is, however, truly perplexing is that although Matignon's famous report never once mentioned marmots, many authors tended to fuse Matignon's work with Believsky's tarbagan hypothesis.

A prime example of this is Louis Boucher's influential work on plague. In his 1901 address to the French Academy of Sciences at Rouen regarding a 'permanent foyer' of plague, Boucher referred to the 'so mysterious and so little explored' plateau 'among the populations of Eastern Mongolia'. This he described as 'perhaps the poorest on the plane', where from 'evil spreads to the north, in the region of Lake Baikal, and to the south, in the mountain massif of Yunnam [sic] where it is definitively established'.<sup>60</sup> This idea no doubt derived from a peculiar fusion of Matignon's research with the endemic hypothesis of Zabolotny (see below). Yet Boucher's major sleight of hand was in claiming that Matignon's findings related to zoonosis, and the tarbagan in particular: 'in these regions where poverty and uncleanness are incredible, there exists a sort of rodent of the marmot family, the *arctomys bobac* or *tarabagane* whose role in the conservations and transmission of plague would be considerable.'<sup>61</sup>

We cannot be certain about the source of this error, but a couple of possibilities appear more likely from a bibliographical perspective. The first scenario is that the confusion stemmed from a short appraisal of Matignon and his plague expedition by the future Nobel laureate Alphonse Laveran to the French Academy in 1900.<sup>62</sup> The illustrious Academician acknowledged Matignon's contribution to the study of plague in Mongolia, whilst at the same time mentioning Russian studies on 'tarabagan disease' in the area. Though a careful reading of his address makes evident that Laveran never actually claimed marmots to figure in Matignon's work, the proximity of the two outbreak narratives may have been the cause of the confusion at hand.<sup>63</sup> This is for example evident in Low's report on Laveran's appraisal

of Matignon's work, where the British colonial medical officer clearly succumbs to this confusion of sources.<sup>64</sup> An equally probable source is yet another short note, this time in the Dutch medical journal *Janus*, authored by Dr Stekoulis of Istanbul, a prolific but long-forgotten author on infectious diseases at the turn of the century. Stekoulis claimed that Matignon's expedition to Eastern Mongolia took place in response to an urgent call by Belgian missionaries operating in the region.<sup>65</sup> The said fathers were supposedly alarmed by news about the Indian plague epidemic, realising that a disease they described as 'tarbagan plague' amongst their flock may be no other than bubonic plague.<sup>66</sup> How the Istanbulite Greek came to this conclusion is not clear, for whereas Matignon does mention the missionaries briefly, he never claims that they were the ones who alarmed him to the existence of plague in the region, nor does he ever mention marmots. It is likely that this anecdotal information was the source of future portrayals of Matignon as supporting the tarbagan hypothesis. Interestingly, Clemow further contributed to the confusion by claiming that it was Matignon who wrote the November 1899 *Janus* article. Hence the words of Stekoulis regarding the Belgian fathers were put in the mouth of Matignon.<sup>67</sup>

Whatever the case may be, the ripple effect of Matignon's research and its mistaken fusion with the tarbagan hypothesis did not take long to reach St Petersburg, where in the spring of 1898 Duke Alexander Petrovich of Oldenburg's newly founded Plague Commission decided to investigate matters further, by dispatching an expedition to the outskirts of Mongolia.<sup>68</sup> The head of this fact-finding expedition was the most prominent and charismatic epidemiologist in Russia at the turn of the century: Danilo Kirilovich Zabolotny.

### ZABOLOTTY'S ENDEMIC THEORY

Zabolotny (1866–1929) was a well educated and progressive medical scientist from the Ukraine, who was expelled from the University of Odessa for student activism in 1889. Between 1896 and 1897 his participation in clinical and experimental research on plague in India during the great epidemic won him international repute.<sup>69</sup> The expedition party led by Zabolotny boarded the Trans-Siberian railway on June 4, 1898, and then crossed the Mongolian border into Qing territory via Kyakhta, reaching Beijing on horseback after having crossed the Mongolian plateau. Following further arrangements in the imperial capital, Zabolotny and his team crossed the imperial hunting grounds of Weichang and reached Toung-kia-yng-tze, lying on the outskirts of the great woodlands.<sup>70</sup> A



summary of the original findings of the expedition was first published in Russian in September 1899 and, concurrently, in the November 1899 issue of the *Annales de l'Institut Pasteur*.<sup>71</sup>

Zabolotny portrayed the village previously studied by Matignon as ‘a small but populous place composed of two-room straw houses accommodating large families of between ten and twenty individuals’.<sup>72</sup> He described the local population as Chinese, with a few Mongols who ‘spoke Chinese and already had Chinese mores’.<sup>73</sup> When it came to plague, he alleged that ‘almost all the plague-inflicted were Catholics; hence, in frequenting each other, they have more chances of being contaminated than the pagans who avoid the sick’.<sup>74</sup> Zabolotny was met by a group of missionaries (he seemed unsure if they were Belgian or Dutch) led by Father Léon Desmet, who provided ‘valuable information on the cases prior to our arrival, and on the current illness’.<sup>75</sup> According to the head of the mission, in the last dozen years the disease had been breaking out in the region annually in the summer months, having first made its appearance in Christian villages north of Selenga, not very far from Toung-kia-yng-tze.<sup>76</sup> Father Léon claimed that in that year alone twenty-four people had been taken ill in the area; all bore buboes or suffered from pneumonia, and all had succumbed to the scourge. Without delay Zabolotny set out to gather more information on the disease:

This is what we have learned. The epidemic has been known here for ten years now under the name of *ven-i*, *ven-tszay*, *khai-ven*; this was imported from North-East Mongolia. The Chinese consider the illness as incurable. They distinguish between two forms: the pulmonary form, whose gravest symptom is hemoptysis, and the bubonic form, which is characterised by the appearance of *gada*—the buboes. Entire families perish [of it]. When the Chinese perceive a bubo on someone in their family, they pinch, in the form of a treatment, the skin around the bubo.<sup>77</sup>

Zabolotny explained that there was no health service to aid people infected by the disease in the area, adding in a tone reminiscent of Matignon’s treatises on superstition, as well as wider discourses on the Chinese character in the West at the time: ‘[t]he Chinese, fatalist by his nature, does not care about death. We may announce to him, without digression, that he will die tomorrow or even today, without these news having the power to impress him; he will merely thank you, meet his family so as to bid him farewell and share with them his last wishes.’<sup>78</sup>

Establishing a laboratory in the local church, Zabolotny was himself able to observe the disease in the case of a few individuals, starting with a thirty-year-old Chinese doctor who was taken ill on September 3, 1898 at Maliento, a Chinese village near Toung-kia-yng-tze. Cultures developed from the ill man's bloody sputum appeared to contain plague bacilli. The patient died before twenty-four hours had elapsed from the first signs of fever. As performing an autopsy was not possible, Zabolotny opted to procure liquid from the man's lungs, hence developing cultures that proved to contain plague bacilli, which once injected into animals (rats and mice) gave positive results. Zabolotny concluded his experiments by diagnosing '*Pneumonia pestica*'.<sup>79</sup> He was also able to isolate plague bacilli from buboes of patients suffering from the bubonic form of the disease.<sup>80</sup> Zabolotny's conclusion was unambiguous: 'it is certain that we have found here, in Eastern Mongolia, present an endemic foyer of bubonic plague which, according to testimonies by missionaries and by doctor Matignon, exists already since more than ten years.'<sup>81</sup> Hence, whilst acknowledging Matignon's contribution to opening up the field of plague research in the region, Zabolotny was careful not to adopt his importation hypothesis. Declaring plague to be endemic, he sought to problematise the region as posing a threat to outlying territories: 'the proximity of many great routes render this foyer excessively dangerous for *China*, as well as for *Mongolia*, *Manchuria* and, by consequence, *Russia*.'<sup>82</sup>

What then of the marmots, the great zoonotic discovery of the first studies of plague in the region? Zabolotny acknowledged the link, noting that the disease is contracted by Buryats who do not cook tarbagan properly and consequently perish from the rare but utmost lethal form of septicaemic plague.<sup>83</sup> However, at the time, he did not linger long on this information, or on the overall zoonotic or sylvatic origin of plague in the region. As Zabolotny is best known in medical history for his defence of the tarbagan origins of plague during the Manchurian epidemic of 1910–11, it is paramount to note here his tacit stance on this matter during the preceding decade. Although he clearly kept clear of Matignon's importation theory, his mind lingered between two alternative possibilities: plague stemming from tarbagan in Mongolia proper, and plague being endemic around Weichang in Eastern Mongolia. A map (Map 4.2) taking the larger part of a page at the end of his 1899 article and also featuring in his second paper to the *Russian Archive of Pathology, Clinical Medicine and Biology* the same year portrayed regions believed by Zabolotny to be 'hearths' or



**Map 4.2** Endemic foyers [*ochagii*] of plague at the end of the nineteenth century. Mostly bacteriologically ascertained plague foyers (Source: Zabolotny 1899; map illustration created by Mick Cafferkey, Senior Illustrator, University of Cambridge)

foyers (*ochagii*) of plague.<sup>84</sup> The map (Map 4.2) was titled ‘Map of Endemic Foyers of Plague’. These included Asir (in Arabia), Mesopotamia, Kiziba, and Nyanza (by Lake Victoria), Tibet, Garhwal, and Kumaon (both British Indian districts at the time), Formosa (Taiwan), and Yunnan, as well as a region designated as Eastern Mongolia, which included four localities: Weichang, Toung-kia-yng-tze, Maliento, and the Khingan mountains. Transbaikalia is absent from these shaded territories, whilst Eastern Mongolia appears to be independent of the south-west Chinese endemic zone, with no infection trajectories such as the ones connecting Yunnan to

Canton, Hong Kong, and Nha Trang appearing on the map.<sup>85</sup> A second hand-drawn map portrayed the ‘Position of endemic foyers in Eastern Mongolia (Weichang region)’. It showed Weichang in shaded grey with broken lines connecting it with towns all the way to Beijing and Tianjin in the south. A second broken line connected Beijing with Kalgan and, ultimately, Urga, reflecting in many respects Matignon’s second map.<sup>86</sup> The geographic imagination of plague fostered by Zabolotny was, we can say, multifocal, and in this scheme of things Eastern Mongolia possessed a place next to other alleged international endemic zones of the disease. As we have already seen, this endemic hypothesis was adopted by international medical figures like Boucher, creating a third pole in the geographic imagination of plague in the region.

It is hence evident that, already by 1900, we have three distinct hypotheses regarding the origins of plague in Inner Asia: (a) Beliaevsky’s tarbagan hypothesis, which claimed that plague is harboured by Siberian marmots, spreads to humans in Mongolia and Transbaikalia via direct contact with the animal whilst harvesting its fat, meat, and fur; (b) Matignon’s importation hypothesis, which claimed that plague is brought from South China to Eastern Mongolia by Shandong coolies, and then potentially spreads to Mongolia and Siberia via the tea and fur trade routes; (c) Zabolotny’s endemic hypothesis, which claimed that plague is permanently present in Eastern Mongolia; this hinted at a possible involvement of marmots but did not commit itself to either a zoonotic or a human contagion link, leaving the question of pathogenesis open. The co-existence of these three distinct and conflicting hypotheses posed crucial questions to plague experts engaged in the systematic study of plague in Mongolia and Transbaikalia.

In the decade preceding the 1910–11 devastating pneumonic plague outbreak in Manchuria, which would render the question of the origins of the disease in the region a topic of international interest, dozens of research papers on plague in Transbaikalia and Mongolia were published in the Russian medical press. Although researchers largely sided with Beliaevsky’s tarbagan hypothesis, the mystery of the role of the Eastern Mongolian foyer lingered since no expedition was able to reach so far into Chinese territory after Zabolotny in order to re-examine the situation. The solution to this problem was to be given by the last major contributor to the study of plague in the region before the 1910–11 Manchurian outbreak: Ivan Stepanovich Dudchenko, also known as Dudchenko-Kolbasenko, who produced a startling ethno-geographical outbreak narrative.

## DUDCHENKO'S HYBRID HYPOTHESIS

Dudchenko was part of a Special Commission sent in 1908 to South Transbaikalia to investigate plague; he provided two major papers deriving from the expedition, which were to have a significant impact on medical perceptions of plague in the region. Dudchenko was interested in examining the extent to which Zabolotny's theory on the Weichang origins of plague was contradicted by plague cases found in Mongolia and Transbaikalia. Key to his argument was the observation that the actual place where one finds plague-infected individuals should not be mistaken as the place where the disease originates. Dudchenko noted that the entire region under question was thick with major human traffic networks. From Buddhist pilgrims and tea-block traders, to Imperial envoys and Bannermen, anyone who wanted to reach Urga from Beijing and further afar had to pass via Zabolotny's plague zone: 'it seems obvious that travellers from distant places, people weakened from travel hardship with low natural resistance, can easily be infected with plague, driving slowly on camels across the plague focus, staying there for the night.'<sup>87</sup> Thus, a road-weary pilgrim or trader would stop at one of the villages near Weichang, where he or she could contract the disease and thence travel into Mongolia carrying plague. So far, this hypothesis did not differ greatly from Zabolotny's speculations, or Matignon's fears. Yet at this point Dudchenko introduced a crucial link that would bridge it with the tarbagan hypothesis.

Dudchenko argued that as the infected pilgrim or trader reached Mongolia, he or she would soon succumb to the disease and would be subjected, according to the Lamaist custom, to a sky burial. In other words, the plague-infected corpse would be exposed to the elements and any carrion caring to eat its flesh. What rendered this ethnographic speculation an exegetical tool in defining the trans-regional dynamics of plague was the claim that the sky burial of plague victims offered the opportunity to marmots to eat the infected corpses of unfortunate travellers and hence infect native hunters in turn.<sup>88</sup>

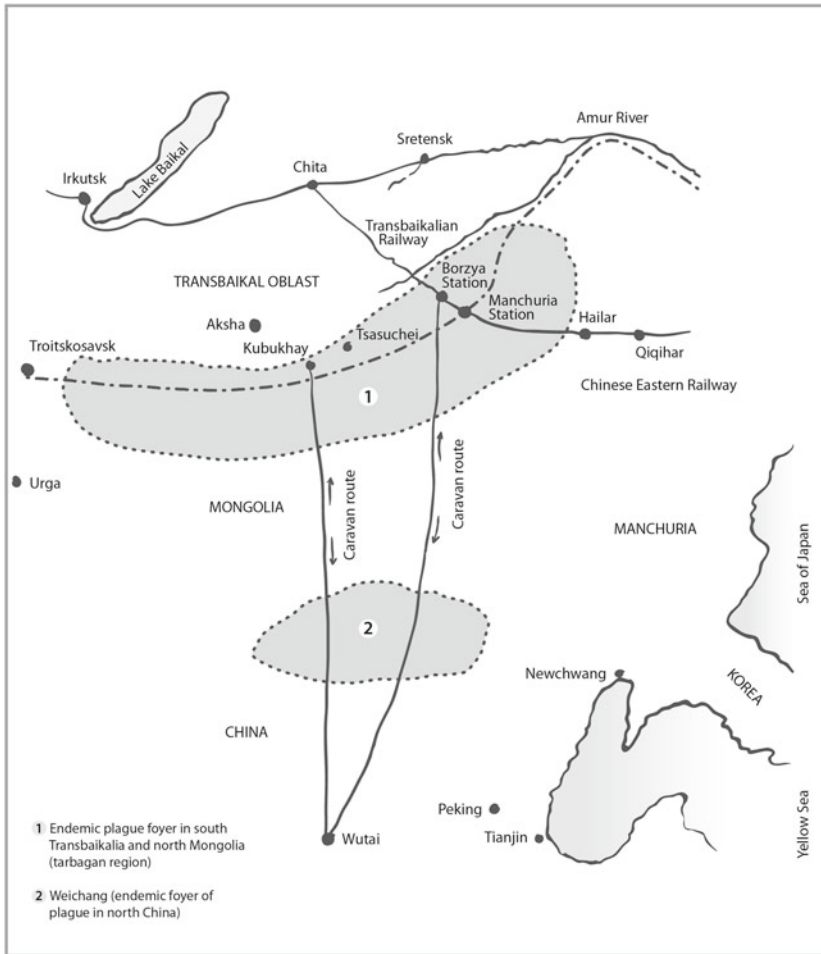
In support of this hybrid ethno-geographic hypothesis, which was to fascinate epidemiologists for decades, Dudchenko offered further evidence in a short entry to the *Russian Archive of Pathology, Clinical Medicine and Biology* in November 1909. There he revealed information secured by a local vet, according to whom although plague afflicted humans in the region, marmots did not suffer from the disease. This supposedly proved

that rather than being the original hosts of plague, the latter were in fact but secondary victims of this anthroponotic disease. Dudchenko attributed the lack of marmot infection in the particular region to the ethnographic fact that, as Muslims, the Kirgiz of the region in question buried corpses of plague victims rather than exposing them to sky burials like the Mongols:

The plague has not been apparently brought to the Akmolinsk District, but even if it was, it did not spread among marmot-tarbagan due to the burial customs of the Mohamedan Kirgiz who bury their dead underground and who therefore did not hitherto encounter this illness. It is known that dead bodies of the Mongols are left on the surface instead of burial, and that from plague-ridden corpses the plague is passed to the marmot-tarbagan as the latter devour dead bodies.<sup>89</sup>

This circular model of plague transmission was crucial for Dudchenko as it elucidated why humans were infected by plague only in the late summer or early autumn. According to his calculations, this was the season when pilgrims and traders returned from their travels south by way of Weichang, thus first infecting marmots, which then infected humans in turn.<sup>90</sup> Hence, reasoning ethnographically, Dudchenko claimed that the cause of more frequent outbreaks in Mongolia than in Transbaikalia was not only the former's proximity to Weichang but also because therein sky burial was religiously observed. By contrast, Transbaikalia was more distanced from the endemic focus of plague, and its population was also supposedly less observant of Tibetan Buddhist death rites.<sup>91</sup>

In order to illuminate this connection, Dudchenko published a map of this trans-regional infection route that warrants close examination (Map 4.3).<sup>92</sup> The map, or rather, to paraphrase Tom Koch, cartographic experiment was titled 'Schematic Map: endemic hearths of plague in northern China and adjacent parts of southern Transbaikalia and northern Mongolia'. It featured two encircled regions: Weichang and the Mongol-Chinese border. The former bore in brackets the note 'foyer [*ochag*] of endemic plague in North China', whilst the latter the note 'foyer of endemic plague in south Transbaikalia and north Mongolia (tarbagan region)'. Dudchenko drew two routes, both ending in Wutai, south-west of Beijing. The first crossed the western reaches of Weichang and ended up in Kubukhay village, a few miles south-east of Aksha. The second crossed the eastern reaches of Weichang and ended up in Borzya, a major railway station before the Chinese border on the Chinese Eastern Railway



**Map 4.3** Schematic map. Endemic foyers [*ochagi*] of plague in northern China and in adjacent units of South Transbaikalia and North Mongolia (border of foyers signified by *dotted line*) (Source: Dudchenko-Kolbasenko 1909; map illustration created by Mick Cafferkey, Senior Illustrator, University of Cambridge)

line. Both trajectories were marked as ‘caravan routes’, with arrows on the sides of the routes noting that the traffic (and perhaps the direction of infection) was bilateral. It is not clear whether Wutai referred to the city or to the region of Wutaishan, but given that the latter was at the time a major Tibetan-Mongol Buddhist hub, it is likely that Dudchenko

took the monastic complex for his reference. Wutaishan's significance as a pilgrimage destination for Mongols at the turn of the century has been recently studied by Isabelle Charleux, who has demonstrated that the particular religious practice was a process involving elites as well as ordinary Mongols. The pilgrims' motivation was karmic, therapeutic, and related to burying the bones of their parents in the holy land. Such goals did not exclude trade, which flourished in the monastic complex especially during the celebrated horse and mule fair on the sixth month of the Lunar year, coinciding with what Dudchenko considered as the peak of bubonic-plague-spreading patterns in the region.<sup>93</sup>

We need to pay close attention to the importance placed on this Buddhist link for two reasons: first, because it reproduced an internationally dominant ethnographic narrative connecting epidemics to pilgrimages in different parts of the globe; second, because it contradicted the particularly Russian narrative regarding the relation between Tibetan-Mongolian Buddhism, known at the time as Lamaism, and plague. This contended that the particular form of Buddhism functioned as an inhibitor rather than as a proliferator of the disease in the region.

The colonial ethnographic imagination of pilgrimages as modes of transmission and sustenance of plague was greatly embellished in the context of the 1897 Venice Conference on the defence of Europe against the disease.<sup>94</sup> With Zabolotny himself leading the problematisation of the hajj to Mecca in terms of its plague-spreading potential, soon other similar conventions took place in Alexandria and Istanbul. J.M. Eager would note in his report to the US Public Health and Marine-Hospital Service that '[t]hrough the efforts to prohibit Mohammedan excursions failed, attention was drawn to the improvement of quarantines and the prevention of clandestine pilgrimages'.<sup>95</sup> In this wider context of epidemiological rationality, the problematisation of other pilgrimages operated within the boundaries of an international geographic imagination whose role was to connect different regions of the world nosologically.<sup>96</sup> Pilgrimage thus functioned as gravitational point of colonial epidemiological reasoning. In other words, it should be seen as an ethnographic epidemiological operator that facilitated the pathologisation of colonised (or semi-colonised) subjects' religious activities and social networks.<sup>97</sup>

In this respect, it is interesting to note that the Mongolian pilgrimage to Wutaishan was not the sole object of epidemiological problematisation in the region. Dr Levin, empowered by the Tsar to investigate the 1898 plague outbreak in Anzob (in today's Tajikistan), claimed 'that



this disease and the outbreaks of plague on the lower Volga and on the Caspian [...] might be due to carriage of infection from Mongolia by means of Kalmuck and Kirghiz pilgrims who might have visited certain sacred Moslem shrines in Mongolia, and brought the disease back with them'.<sup>98</sup> In his influential book *Geography of Epidemics*, Clemow protested that distances between the two regions were impossibly vast, and that the Muslims 'of the Kirghiz steppes east of the Volga are said to have no holy places of any kind in Mongolia, while the Kalmucks, who live to the west of the Volga, are despised and looked upon as heathens by both Mongols and Kirghiz and are not allowed to pass the Kirghiz steppes'.<sup>99</sup>

What differentiated the epidemiological problematisation of the Wutai pilgrimage was its place within the wider Russian medical literature on plague at the time. This proclaimed that Lamaism acted as an inhibitor to the spread of bubonic plague. The idea was first proposed by Julian Talko-Hryntsewich who, as we have already seen, undertook a plague-related expedition to Mongolia in the autumn of 1899. In his report to the bulletin of the Imperial Geographical Society's Kyakhta branch, he provided a complex narrative on native understandings of outbreaks in the region, which hinted for the first time at a connection between the alleged native knowledge of plague and the teachings of Lamaism. In particular, Talko-Hryntsewich claimed that the true source of plague was known to Mongols: 'it is said that ancient Buddhist religious laws forbid the consumption of the tarbagan and entering a temple wearing a fur coat [made from] that animal.'<sup>100</sup> This information was, however, contradicted a few pages later when the anthropologist expressed serious reservations about Lamaist knowledge of plague in Mongolia. Interviewing a Lama on the disease, he reported that the Buddhist monk claimed that plague was a punishment sent by Burkhan (the Buddha) to sinners. Talko-Hryntsewich noted that the Buddhist monk did not seem to understand the symptoms of the disease. This conclusion was seconded by a High-Lama, who claimed that the monks were so terrified of any disease that they would immediately flee from patients and refuse to treat them. Talko-Hryntsewich went on to claim that, in Urga, Lamas preached to Mongols that plague was the result of the spring lunar eclipse, and that the disease would be particularly severe were the eclipse total.<sup>101</sup>

Overlooking the inherent contradiction in Talko-Hryntsewich's report, Russian plague experts sought to adopt the idea that Tibetan Buddhism endowed Mongols with a knowledge of plague. Hence in 1905 Skrzhivan would write in that knowledge of plague is evident in the writings of a

certain Badmasambaboï (evidently the lotus-born Padmasambhava), ‘who has foreseen the disease to come from soil to plants, from them to animals and then to people’.<sup>102</sup> Skrzhivan’s long and muddled portrayal of Tibetan theories of disease was sprinkled with quasi-ethnographic elements and inspired fascination amongst international epidemiological circles for the next three decades.<sup>103</sup>

Dudchenko did not ignore nor repudiate this Buddhist connection. Instead, he engaged in further ethnographic speculation about the alleged Lamaist knowledge of plague. What is striking is Dudchenko’s apparent confusion on the matter. On the one hand, he noted that Buddhist monks who functioned as doctors in the region had no understanding of contagion and thus spent entire days in the same yurt with patients—a fatal attitude in cases of pneumonic plague, the most common clinical form of the disease in the region.<sup>104</sup> And yet, at the same time, he seemed ready to forsake his scepticism and indulge in furthering the Lamaist embellishment of the native knowledge hypothesis. He hence provided a story that would resonate in epidemiological writings for many years to come. Dudchenko recounted that in October 1908 a Lama arrived at the monastery of Tosakh bringing news of a great epidemic that had devastated the encampment of Tsanid-Gegen in Ulyast, himself being the sole survivor. Following Dudchenko’s narrative, ‘the Tosakh Lama decided that the Tsanid-Gegen and his men had died of plague’. He hence ordered to lock up the monastery, letting no one in or out for thirty days: ‘every day at a certain hour of the day all the inhabitants of the monastery came out into the open courtyard of the monastery and formed a line. Lamas of neighbouring datsans appeared at the same time on a nearby hill, and through binoculars accounted for all the people under quarantine, according to prearranged signals.’<sup>105</sup> So successful was the ‘quarantine’, that, in the words of Dudchenko, no one died in the Tosakh joss-house.

What begs explanation at this point is how Dudchenko’s appraisal of Lamaism as plague-preventative relates to his simultaneous condemnation of it as the very means through which the disease spread across the region. The crucial link here is the mytheme of the man-eating Siberian marmots as the latent operator of Dudchenko’s epidemiological rationality.

The outlandish idea that marmots eat human corpses should be sought in a chance encounter between a doctor, a human bone, and a marmot hole. In 1900 Anatoly Podbel’sky organised a plague-finding expedition to Mongolia. There is no space here to do justice to the extensive and

elaborate report of the doctor, as published on December 30, 1901 in the *Russian Archive of Pathology, Clinical Medicine and Biology*. What is crucial to our story is that, at the same time as conducting field observations on the tarbagan, Podbel'sky made another, rather accidental, discovery, which would assume its own life in epidemiological literature in the years to come: 'three yards from the exit hole was found a yellowed human tibial bone. Its epiphysis seemed to be broken off. Seven yards from the entrance of the burrow the ground was littered with the frontal and occipital bones of a human skull. The rest of the skeleton bones were not found.'<sup>106</sup> Podbel'sky risked no explanation of this find; he merely mentioned, half jokingly, that marmots are playful animals and may have used the bones for sharpening their mighty incisors. On a more sombre note he also noted that one should not rule out the possibility of an infection from human corpses to Siberian marmots, in case the former had died of a 'contagious disease, for example plague'.<sup>107</sup>

This short passage would have been no more than a typical anecdotal entry in the chronicles of an expedition, and would have indeed remained a quaint footnote in the growing tarbagan literature, if it was not for Georg Sticker, who gave the story a whole new dimension. For in his book, the influential author reinvented the story, enriching it in a decisive way. According to Sticker, the corpse in question was a plague victim, something Podbel'sky never asserted.<sup>108</sup> This detail led to a captivating instance of epidemiological reasoning. Sticker noted that during sky burials, Mongols laid the bodies of the plague victims on the steppe where they were torn apart by carrion. As a result, he claimed, their blood was said to soil the steppe grass so that marmots, which habitually fed on it, were infected. Adding more spice to his imaginative rendition of the original paper, Sticker also added that tarbagan have the habit of dragging the bones of human plague victims into their nest, thus increasing the risk of infection.<sup>109</sup>

Seen from a critical anthropological perspective, we can say that the human tibial bone found by Podbel'sky near the marmot hole functioned as an epidemiological found-object, in the surrealist tradition of the term. Or, if I may venture a bit further, the bone in question should be approached as what the Rumanian surrealist poet and essayist Gherasim Luca called an 'objectively offered object': an object that exercises a force of catalytic encounter upon the subject that comes across it, radically transforming his or her perception of the ordinary into an extraordinary perception.<sup>110</sup> In the surrealist tradition, the objectively offered object allows different

narratives and imaginaries to come together in a plane of consistency that does not represent either a synthesis of their theses or an overcoming of their antitheses. On the contrary, the objectively offered object forces an anti-dialectical leap of faith: in our case, the belief in human-eating marmots, which, in the hands of Dudchenko, became the key to a new epidemiological reasoning about the trans-regionality of plague.

Hence Dudchenko instituted a transformed ethno-geographic imagination of plague. This rejected both Matignon's hypothesis that the disease was imported from South China, and the original Russian tarbagan hypothesis, which argued that marmots were the original source of the disease. Accepting Zabolotny's thesis that Eastern Mongolia was an endemic foyer of plague (but also adding Transbaikalia as a second endemic 'hearth'), this ethno-geographic epidemiological model envisioned the crucial route of plague-importation to run not from South China to Weichang, but in a circuit between Wutai, Weichang, Mongolia, and Transbaikalia. And, at the same time, it inverted the zoonotic link established by previous research, by claiming that it was humans who infected marmots rather than the other way around. Through a leap in epidemiological reasoning—the endorsement of the mytheme of man-eating marmots—Dudchenko turned Belivsky's tarbagan hypothesis on its head. The result was an apparently plausible if in fact surreal compromise between it, the endemic hypothesis of Zabolotny, and the trans-regional problematisation of plague transmission originally introduced by Matignon.

What had started in 1898, in the hands of Jean-Jacques Matignon, as a standard exercise in explaining plague outbreaks in a rural area of north-east China in terms of a trans-regional importation theory that pointed the finger at the alleged geographic source of the third plague pandemic (South China) and the usual suspects (Chinese coolies) was, within little more than a decade, transformed by Dudchenko into a complex outbreak narrative that bridged colonial medical problematisations of pilgrimage, Russian Orientalist fascination with Tibetan-Mongolian Buddhism, and research pointing at Siberian marmots as the zoonotic source of plague in the region.<sup>111</sup>

The three ethno-geographic epidemiological assemblages described in this chapter would prove a vital index of concepts come the Manchurian pneumonic plague outbreak of 1910–11. The Matignon-Zabolotny-Dudchenko trajectory would equip epidemiological reasoning regarding plague with a platform for negotiating the relation between zoonotic infection and contagion, leading by April 1911 to another major recon-

figuration of its three sites of problematisation: population movement, human-animal interaction, and endemicity. As we will see in the next chapter, in a stroke of epidemiological ingenuity, Wu Liande, the leader of Chinese anti-plague efforts in Manchuria, would recombine these elements, convincing the international medical community that plague was endemic in Transbaikalia, that it was a disease of the tarbagan transmitted to humans whilst hunting the animal, and, turning pneumonic and airborne, was thence carried south by Shandong coolies. Rather than signalling the demise of more adventurous explanations (including man-eating marmots) in epidemiological literature, Wu's model would become a canonical exegesis against which such elements, as well as new problematisations of the geographic and zoonotic aspects of plague in the region, would be evaluated.

## NOTES

1. Ch.-E. Amory Winslow, *The Conquest of Epidemic Disease: A Chapter in the History of Ideas* (Madison: University of Wisconsin Press, 1980); M. De Lacy, 'Nosology, Mortality and Disease Theory in the Eighteenth Century,' *Journal of the History of Medicine and Allied Sciences*, vol. 54 (1999): 261–184; V. Nutton, 'The Reception of Fracastoro's Theory of Contagion: The Seed that Fell among Thorns?,' *Osiris*, 2nd series vol. 6 (1990): 196–234; A. Zuckerman, 'Plague and Contagionism in Eighteenth-Century England: The Role of Richard Mead,' *Bulletin of the History of Medicine*, vol. 78, no. 2 (Summer 2004): 273–308.
2. D.G. Grigsby, 'Rumor, Contagion and Colonization in Gros's Plague-Stricken of Jaffa (1804),' *Representations*, no. 51 (Summer 1995): 1–46.
3. For a general critique of the contagionism-anti-contagionism dichotomy see Hamlin (2009).
4. Société de Médecins et des Chirurgiens, *Dictionnaire des sciences médicales* vol. 41 (Paris: Panckoucke, 1920), p. 74. This definition was consecutively endorsed by a series of doctors operating in areas of the Middle East that experienced eruptions of the disease in the mid-nineteenth century (see, for example, Castaldi 1875: 26).
5. Société de Médecins et des Chirurgiens, *Dictionnaire des sciences médicales*, p. 107. Such theories of plague did not evaporate with the discovery of the bacillus; James Cantlie, one of the most promi-

- nent researchers of plague in Hong Kong, developed his own miasmatic-contagion model in papers to *The British Medical Journal* and *The Lancet* (Cantlie 1894, 1897).
6. See, for example, Forbes (1840).
  7. M. Harrison, *Contagion: How Commerce Has Spread Disease* (New Haven: Yale University Press, 2013); on the subject of objects, infection, and quarantine, see Barnes (2014).
  8. Anon., 'The Japanese Soldier's Feet,' *The British Medical Journal*, vol. 1, no. 2263 (May 14 1904): 1150–1151; Anon., 'The Disinfection of an Army,' *The British Medical Journal*, vol. 2, no. 2391 (October 27 1906), p. 1148; Anon., 'A Short Way with Lepers,' *The British Medical Journal*, vol. 1, no. 2735 (May 31 1913), p. 1181; Jean-Jacques Matignon, 'De l'atriplicisme (Intoxication par l'arroche),' *Annales d'hygiène publique et de médecine légale*, série 3, no 37 (1897): 103–118; Jean-Jacques Matignon, 'La destruction des centres lepreux en Chine,' *La chronique medical*, vol. 20, no. 10 (May 15 1913): 289–291.
  9. J.-J. Matignon, *Superstition, Crime et Misère en Chine (Souvenirs de Biologie Sociale)* (Paris: Masson et cie, 1899).
  10. Anon., 'The Disinfection of an Army.'
  11. J.-J. Matignon, 'La Peste Bubonique en Mongolie,' *Annales d'hygiène publique et de médecine légale*, Serie 3e, 39 (January 1898): 227–256; J.-J. Matignon, 'La peste bubonique en Mongolie,' *Archives de médecine et de pharmacie militaires*, vol. 33 (1899): 463–486.
  12. Matignon, 'La peste bubonique en Mongolie,' (1899), p. 463.
  13. On this migratory pattern see Gottschang and Lary (2000).
  14. Matignon, 'La peste bubonique en Mongolie,' (1899), p. 464.
  15. Matignon, 'La peste bubonique en Mongolie,' (1899), p. 464.
  16. A. Proust, *La défense de l'Europe contre la peste et la conférence de Venise de 1897* (Paris: Masson et Cie, 1897), p. 119.
  17. Matignon, 'La peste bubonique en Mongolie,' (1899), p. 464.
  18. N. Smith, 'Hibernate No More! Winter, Health, and the Great Outdoors.' In B. Perrins & N. Smith (eds) *Interacting with the Environment: Explorations of Manchuria's Environmental History* (Vancouver: University of British Columbia Press, forthcoming).
  19. Matignon, 'La peste bubonique en Mongolie,' (1899), p. 465.
  20. Matignon, 'La peste bubonique en Mongolie,' (1899), p. 465.
  21. Matignon, 'La peste bubonique en Mongolie,' (1899), p. 465.

22. Matignon, 'La peste bubonique en Mongolie,' (1899), p. 465.
23. Matignon, 'La peste bubonique en Mongolie,' (1899), pp. 465–466.
24. On the issue of supposedly infected clothes in the 1894 Hong Kong outbreak see Hong Kong Government Gazette [GA 1895 no.146]; see also Simpson (1905).
25. On a discussion of continuous body dumping crises during plague outbreaks in Hong Kong see Hong Kong Public Records Office [HKRS203-1-24-33].
26. This was a lasting concern regarding plague burials. For a discussion on the danger posed by rain in the case of Hong Kong see Hong Kong Public Records Office [HKRS203-1-25-6].
27. Matignon, 'La peste bubonique en Mongolie,' (1899), p. 466.
28. Matignon, 'La peste bubonique en Mongolie,' (1899), p. 466.
29. Central to the bibliography on the subject are the five volumes of the Indian Plague Commission Report (especially House of Commons Parliamentary Papers [Cd.140], [Cd.141]), and J. Cantlie, 'The Spread of Plague,' *The Lancet*, vol. 149, no. 3828 (January 9 1897): 85–91.
30. Matignon, 'La peste bubonique en Mongolie,' (1899), p. 466.
31. Matignon, 'La peste bubonique en Mongolie,' (1899), p. 466.
32. Matignon, 'La peste bubonique en Mongolie,' (1899), p. 467.
33. On a board historical discussion of connecting trade to epidemics see Harrison (2013).
34. Matignon, 'La peste bubonique en Mongolie,' (1899), p. 467. Shandong was spelled 'Chan-toung' and 'Chang-tong' by Matignon, following conventions in French and English at the time. I have here retained Shandong throughout for avoidance of confusion.
35. M.E. Hanson, *Speaking of Epidemics in Chinese Medicine: Disease and Geographic Imagination in Late Imperial China* (London: Routledge, 2011).
36. M.E. Hanson, 'Visualizing the Geography of Diseases in China, 1870s–1920s' (quoted by permission of the author); Tom Koch, *Disease Maps: Epidemics on the Ground* (Chicago: University of Chicago Press, 2011).
37. Hanson, 'Visualizing the Geography of Diseases in China'; all quotes below from the same paper.
38. D. Wood, 'The Anthropology of Cartography.' In Les Roberts (ed.) *Mapping Cultures: Place, Practice, Performance*, pp. 280–303, (Basingstoke & New York: Palgrave Macmillan), p. 286.

39. Wood, 'The Anthropology of Cartography,' p. 289.
40. Matignon, 'La peste bubonique en Mongolie,' (1899), p. 470.
41. P. Manson, 'Dr. Manson's Report on the Health of Amoy for the Half-year ended 31st March 1878,' *Customs Gazette, Medical Reports*, no. 2 (January-March 1878): 25–27; E. Rocher, *La Province Chinoise du Yunnan* (Paris: Lerous, 1879).
42. This trajectory is more pronounced in the original hand-drawn map, reproduced here, than in the printed version (Archives de l'Institut Pasteur, Cote: IND.A1—Lieu: 4/151–153).
43. For further discussion of Matignon's maps and mapping disease in China in general see Hanson (2012).
44. Matignon, 'La peste bubonique en Mongolie,' (1899), p. 472.
45. Matignon, 'La peste bubonique en Mongolie,' (1899), p. 473.
46. Matignon, 'La peste bubonique en Mongolie,' (1899), p. 482.
47. Matignon, 'La peste bubonique en Mongolie,' (1899), p. 481.
48. Matignon, 'La peste bubonique en Mongolie,' (1899), p. 481.
49. M.J. Echenberg, *Plague Ports: The Global Urban Impact of Bubonic Plague, 1894–1901* (New York: New York University Press, 2007).
50. House of Commons Parliamentary Papers [C.9445], pp. 199–258.
51. R. Peckham, 'Infective Economies: Empire, Panic and the Business of Disease,' *The Journal of Imperial and Commonwealth History*, vol. 41, no. 2 (April 2013), p. 214. Peckham notes W. J. Simpson's claim that the pathogen spread along the 'most frequented trade routes' (in Peckham 2013: 221). Hence, he argues, Hong Kong was seen as a 'disease hub' and plague as a 'pseudo-commodity' (Peckham 2013: 223).
52. House of Commons Parliamentary Papers [C.9445], p. 214.
53. In this case too the openness of the harbour was seen as both a source of wealth and death, with a special focus on the coolie labour force. However, in the case of Newchwang (Yingkou) the connection between Shandong coolies and the Boxers complicated matters further, making this a political as well as economic threat (Padlevsky 1903, Poletika 1904).
54. On the notion of imperial interconnectedness, see Peckham (2013). See also Harrison (2013).
55. Anon., 'The Bubonic Plague in Manchuria,' *The British Medical Journal*, vol. 1, no. 2735 (May 14 1898), p. 1286; Anon., 'Chinese Imperial Maritime Customs,' *The British Medical Journal*, vol. 2, no. 2068 (August 18 1900), p. 454.



56. Fr. G. Clemow, 'Plague in Siberia and Mongolia, and the Tarbagan (*Arctomys bobac*),' *The Journal of Tropical Medicine*, vol. 3 (February 1900): 169–176.
57. Clemow, 'Plague in Siberia and Mongolia, and the Tarbagan (*Arctomys bobac*),' p. 173. See Chap. 2 for introduction to this work.
58. Clemow, 'Plague in Siberia and Mongolia, and the Tarbagan (*Arctomys bobac*),' p. 173–4.
59. Anon., 'The Plague, Progress of the Disease: Siberia,' *The British Medical Journal*, vol. 1, no. 2044 (March 3 1900), p. 540.
60. L.M. Boucher, 'La peste à la fin du XIX et au XX siècle,' *Précis Analytique des Travaux de l'Académie des Sciences, Belles-Lettres et Arts de Rouen Pendant l'Année 1900–1901* (Rouen: Caniard), p. 136.
61. Boucher, 'La peste à la fin du XIX et au XX siècle,' p. 152.
62. M. Laveran, 'Unititled,' *Bulletin de l'Académie de Médecine*, serie 3, vol. 43 (Séance du 20 Février 1900), p. 155.
63. Pr. Wald, *Contagious: Cultures, Carriers, and the Outbreak Narrative* (Durham: Duke University Press, 2008).
64. House of Commons Parliamentary Papers [C.9445], p. 363; Low refers to Laveran's report as made by Matignon, adding to the confusion.
65. Stekoulis, 'Contribution à l'Étude de la peste de Mongolie,' *Janus, Archives internationales de l'histoire de la médecine et la géographie médicale*, vol. 4 (November 1899): 617–618.
66. Stekoulis, 'Contribution à l'Étude de la peste de Mongolie,' p. 617.
67. Clemow, 'Plague in Siberia and Mongolia, and the Tarbagan (*Arctomys bobac*),' p. 173.
68. On Alexander Petrovich of Oldenburg (1844–1932) and his contribution to plague research in the Russian Empire see Supotnitsky and Supotnitskaya (2006) and Golikov and Sapronov (2010).
69. I.I. Bilai, *Zhizn', otdannaya lyudyam: K 100 letiyu so dnya rozhdeniya D. K. Zabolotnogo* (Kiev: Izdatel'stvo Naukova Dumka, 1966).
70. On the Weichang imperial hunting grounds see Hou and Pirazzoli (1979).
71. D.K. Zabolotny, 'Pustuleznaya forma chumui,' *Russkii arkhiv patologii, klinicheskoi meditsiny i bakteriologii* (September 30 1899), p. 239; D.K. Zabolotny, *Éndemicheskie ochagi chumui na zemnom share i prichinui eya rasprostraneniya* (Rikker: St Petersburg, 1899). D.K. Zabolotny, 'La Peste en Mongolie Orientale,' *Annales*

- de l'Institut Pasteur*, vol. 13, no. 11 (November 1899): 833–840. Bimbaev (1899) also provided his own account of the expedition.
72. Zabolotny, 'La Peste en Mongolie Orientale,' p. 833.
  73. Zabolotny, 'La Peste en Mongolie Orientale,' p. 833.
  74. Zabolotny, 'La Peste en Mongolie Orientale,' p. 833.
  75. D.K. Zabolotny, 'Izsledovaniya po chumê; stat'ya pervaya,' *Arkhiv biologicheskikh nauk*, vol. 8, no. 1 (1901), p. 65.
  76. Zabolotny, 'La Peste en Mongolie Orientale,' p. 834.
  77. Zabolotny, 'Izsledovaniya po chumê,' p. 66. For a discussion of term *wenyi* as a generic term on epidemics, and notions of contagion in Imperial China see Hanson (2011), Leung (2011).
  78. Zabolotny, 'La Peste en Mongolie Orientale,' p. 835. From a total of sixteen cases observed, six were pneumonic, eight bubonic, and one septicaemic.
  79. Zabolotny, 'Izsledovaniya po chumê,' p. 68. Two more patients were similarly diagnosed whilst three were found to suffer from bubonic plague. It must be noted here that Zabolotny claimed it was easy to procure samples through injection of buboes due to the above-mentioned pinching custom. For a critical discussion of popular perceptions of injection in the region see Rogaski (2011).
  80. Zabolotny, 'La Peste en Mongolie Orientale,' p. 836.
  81. Zabolotny, 'La Peste en Mongolie Orientale,' p. 837.
  82. Zabolotny, 'La Peste en Mongolie Orientale,' p. 837, emphasis in the original.
  83. Zabolotny, 'La Peste en Mongolie Orientale,' p. 837.
  84. Zabolotny, *Éndemicheskie ochagi chumui na zemnom share i prichinui eya rasprostraneniya*, p. 2. The map is reproduced in Zabolotny's 1907 monograph on plague (Zabolotny 1907: 6). I would like to thank Susan D. Jones for bringing to my attention that *ochag* was rendered as 'nidus' in the context of Eugene N. Pavlovsky's work on plague (1966). For a discussion of Pavlovsky's 'landscape epidemiology' see Meade and Emch (2010).
  85. In the Zabolotny (1907) version of this map, Mesopotamia and Asir were hand-marked with a question mark, whereas Yunnan and Weichang with an exclamation mark; a handwritten note under the map read: 'Note! Designated foyers where plague is bacteriological ascertained.' As regards Weichang, however, this remained a much-contested claim, illustrating the ambiguity of bacteriological proof.

86. D.K. Zabolotny, *Chuma (Pestis bubonica): épidemiologiya, patogenez i profilaktika* (St Petersburg: Interior Ministry Press, 1907), p. 7.
87. Dudchenko-Kolbasenko, 'Ob izslédovanií chumnuikh zabolêvanií v Zabaikal'skoí oblasti v 1908 godu v svyazi s tarabagan'eí chumoí,' *Vestnik obshchestvennoí gigeniui, sudebnó i prakticheskó meditsinui*, vol. 45, no. 7 (July 1909), pp. 1048–1049.
88. I.S. Dudchenko-Kolbasenko, 'K voprosu o "tarabagan'eí chumê,"' *Vestnik obshchestvennoí gigeniui, sudebnó i prakticheskó meditsinui*, vol. 45, no. 11 (November 1909), p. 1698.
89. Dudchenko-Kolbasenko, 'K voprosu o "tarabagan'eí chumê,"' p. 1699.
90. Dudchenko-Kolbasenko, 'Ob izslédovanií chumnuikh zabolêvanií v Zabaikal'skoí oblasti v 1908 godu v svyazi s tarabagan'eí chumoí,' p. 1051.
91. Dudchenko-Kolbasenko, 'Ob izslédovanií chumnuikh zabolêvanií v Zabaikal'skoí oblasti v 1908 godu v svyazi s tarabagan'eí chumoí,' p. 1078.
92. Dudchenko-Kolbasenko, 'Ob izslédovanií chumnuikh zabolêvanií v Zabaikal'skoí oblasti v 1908 godu v svyazi s tarabagan'eí chumoí,' p. 1077.
93. I. Charleux, 'Mongol Pilgrimages to Wutai Shan in the Late Qing Dynasty,' *Journal of the International Association of Tibetan Studies*, vol. 6 (December 2011): 275–326. It is interesting to note here that there appears to be no medical record of a plague outbreak in the monastic complex. Whether there are records of epidemics in the monastic archives themselves lies beyond the expertise of the author and comprises a promising topic of research. It should also be noted that, later on, Parry (1918) would implicate Tibetan Buddhist pilgrimage in a plague outbreak in the Gansu corridor.
94. Proust, *La défense de l'Europe contre la peste*. For an example of pre-1894 problematisations of Shiite pilgrimage to Kerbala in relation to plague see House of Commons Parliamentary Papers [C.2262], p. 33.
95. J.M. Eager, *The Present Pandemic of Plague* (Washington: Government Printing Office 1908), p. 5; for discussion of the question of pilgrimage and quarantine see Bulmus (2012), Harrison (1999), Nair (2009), and Tagliacozzo (2013). For contemporary accounts of plague, pilgrimage, and quarantine in Arabia see Cozzonis (1899) and Anon. (1909).
96. On the notion of nosology in the case of China see Hanson (2012).

97. For a discussion on pilgrimage, quarantine, and epidemics in the nineteenth century see Dutta (2008), Low (2007), Huber (2006), Mishra (2008), and Sriraman (2013).
98. Fr. G. Clemow, *The Geography of Disease* (Cambridge: Cambridge University Press), p. 326. On the Anzob outbreak see Levin (1899), Clemow (1899), and Basello and Ognibene (2013).
99. Clemow, *The Geography of Disease*, p. 326.
100. Yu. D. Tal'ko-Gryntsevich, 'O chumnuikh zabolêvaniyakh v Mongolii,' *Trudui Troitskosavsko-Kyakhtinskago otdelêniya Priamurskago otdêla Imperatorskago Russkago Geograficheskago Obshchestva*, vol. 1, no. 1–2 (1899): p. 100.
101. Tal'ko-Gryntsevich, Yu. D., 'O chumnuikh zabolêvaniyakh v Mongolii,' p. 101.
102. F. Skrzhivan, 'Nashi svêdênîya o tarabagan'eï chumê,' *Russkïi arkhiv patologii, klinicheskoi meditsinui i bakteriologii* (July 30 190);, p. 609.
103. Wu L.-T., *Treatise on Pneumonic Plague* (Geneva: League of Nations, 1926).
104. Dudchenko-Kolbasenko, 'Ob izslêdovanii chumnuikh zabolêvaniï v Zabaïkal'skoï oblasti v 1908 godu v svyazi s tarabagan'eï chumoï,' p. 1072.
105. Dudchenko-Kolbasenko, 'Ob izslêdovanii chumnuikh zabolêvaniï v Zabaïkal'skoï oblasti v 1908 godu v svyazi s tarabagan'eï chumoï,' p. 1076.
106. A.I. Podbel'sky, 'Nablyudenîya nad tarabaganami v Mongolii,' *Russkïi arkhiv patologii, klinicheskoi meditsinui i bakteriologii* (December 30 1901), p. 261.
107. Podbel'sky, 'Nablyudenîya nad tarabaganami v Mongolii,' p. 261.
108. G. Sticker, *Abhandlungen aus der Seuchengeschichte und Seuchenlehre*, 2 vols. (Gießen: Töpelmann, 1908), vol. 1 (Die Pest als Seuche und als Plage), p. 123.
109. Sticker, *Abhandlungen aus der Seuchengeschichte und Seuchenlehre*, vol. 1 (Die Pest als Seuche und als Plage), p. 123. This idea was also soon echoed by Preble (1912).
110. Gh. Luca, *The Passive Vampire* (Prague: Twisted Spoon, 2009).
111. For a critical approach to Russian Orientalism see Schimmelpenninck van der Oye (2010).

## Anthropological Types and Clustered Hypotheses

In the autumn of 1910 what had been imagined but had so far failed to materialise came into being. And in so doing it added a crucial episode in the history of modern plague, contributing significantly to medical knowledge and debates about the disease. The great Manchurian epidemic of 1910–11 was the first instance of the pneumonic type of plague to attract widespread international medical attention.<sup>1</sup> The disease first appeared in mid-October 1910 in the Manchurian border town of Manzhouli.<sup>2</sup> It would quickly spread along the rail tracks to the great cities of Harbin (October 27) and Changchun (December 31), reaching Beijing on January 12 and the provinces of Jilin and Shandong by February 1911.<sup>3</sup> After killing approximately 60,000 people, the epidemic waned in April 1911, marking an unprecedented mortality rate of 100%. As authors like Carl Nathan, Cheng Hu, Mark Gamsa, and William Summers have demonstrated, the epidemic fed into a major geopolitical crisis in the region, with the Chinese, Japanese, and Russian Empires using it as a lever for consolidating and expanding control over Manchuria.<sup>4</sup>

These aspects of the epidemic have been examined in detail by historians of late Qing China. As Luesink, Liew, Knab, and Lei have shown, the outbreak provided a privileged field for an international medical debate over plague, its causes, and its modes of transmission, as well as over ways of controlling and preventing epidemics.<sup>5</sup> What has been left out of this discussion is the utilisation of ethnographic data in the course of the outbreak. In this chapter I will examine the application of ethnographic approaches of plague during and after the great Manchurian epidemic, focusing on

the work of Wu Liande, China's leading plague expert and founder of the North Manchurian Plague Prevention Service. In so doing I will focus on two major operations: the constitution of an anthropological typology based on the binary between knowledgeable and skilful natives and ignorant and inept 'coolies', and the forging of a cluster between the tarbagan hypothesis and the native knowledge hypothesis, which had crucial consequences in the development of epidemiological reasoning regarding plague not only in the region but also on a global scale.

### WU LIANDE'S ETHNOGRAPHIC NARRATIVE

On April 1911 the First International Plague Conference was held in Mukden in order to discuss the epidemic that was ravaging the North-East Provinces (Dongbei) of the Qing Empire. This meeting of some of the world's leading plague experts provided a platform for contesting perspectives regarding the causes of the outbreak. Held only a few weeks after the sudden and largely unexplained end of the epidemic, the conference was composed of medical luminaries from eleven countries around the world, including Richard P. Strong, Kitasato Shibasaburō, Danilo Kirilovich Zabolotny, Arthur Stanley, Paul B. Haffkine, Oscar Teague, Reginald Farrar, Erich Martini, and Dugald Christie. The minutes of the conference would be published, with some omissions, a year later in Manila under the editorship of Strong, covering 500 pages of dense reports and discussion.<sup>6</sup>

The conference was chaired by Wu Liande, who had been placed by the Qing court in charge of China's anti-plague efforts in the region six months earlier. As has been shown by historians of the epidemic, Wu was faced with direct and often belligerent opposition by the administration and medical experts of the Japanese-run South Manchurian Railway and the Kwantung Leased Territory.<sup>7</sup> On the level of epidemiology this antagonism was translated in Japanese plague experts insisting on the role of the rat in the epidemic. Wu's confident opening address to the conference rebutted this hypothesis and declared that the zoonotic source of the epidemic was the tarbagan. Wu was also careful to underline the role of Russian experts in the identification of the latter as a plague-carrying animal, hence pleasing the delegates of the third imperial force contending for Manchuria at the time.<sup>8</sup> Treading carefully on the paucity of actual evidence, he presented delegates with information gathered by explorers and doctors over the previous decades. It is safe to assume that most of the delegates were already aware of this literature through reading the

international medical press. Wu would, however, prove far more creative when it came to ethnographic aspects of the outbreak. In his opening speech to the First International Plague Conference, he informed delegates that his assistant, Dr Ch'uan Shao Ching, had visited Manzhouli where he gathered vital information about the disease. Wu hence revealed that 'the local people have long been familiar with this disease, both in men and in animals' and that important data were collected '[f]rom the actual marmot hunters themselves' which 'will exercise an important bearing on our knowledge of the plague'.<sup>9</sup> These data were of a very striking nature indeed. The way in which Wu opened his speech at the Plague Conference is revealing and worth quoting at length:

Nature is very rich in coincidences, and perhaps as scientists more than any other class of men you are prepared for such, but who would have dreamed that the healthy marmot, basking, as it loves to do, in the warm sunshine, utters a cry resembling the sound of '*bu pa, bu pa*' which in Chinese language, at any rate, means 'don't be afraid', or 'no harm'. Sickness renders it mute, so that in the light of present knowledge it would seem that when the marmot is not crying 'no harm, no harm', there is very real harm indeed. The sickness in the *tarabagan*, which we presume is the forerunner of the plague, in this case is characterised by an unsteady gait, inability to run or to cry when chased, and when caught, the physical signs are seen to consist principally of enlargement of the glands. When noticing the above signs, the experienced hunter leaves his quarry severely alone and betakes himself to more distant sphere.<sup>10</sup>

It is worth pausing here to consider the semiotic and evolutionary implications of this reported signalling trait—a call transmitted by an animal which advertised its availability and edibility to all potential predators. Moreover, a signal produced by this unfortunate animal not in its own tongue, but, in a mysterious turn of fate, in Chinese. This however was not just a passing if brilliant moment in scientific fiction, but a crucial part of Wu's epidemiological reasoning. For, it was claimed, native hunters did not only translate the marmot's Chinese as an invitation to dinner, but also interpreted its silence as a sign of lethal danger.

What was the source of this paradoxical complex of zoosemiotics? The unsteady gait of ill marmots was, as we have already seen, first noted by Beliaevsky and Reshetnikov in their 1895 articles and then adapted and reproduced in various international medical publications. On the other hand, nothing of the sort applies for Wu's *bu pa* information. Beliaevsky

only noted that sick marmots do not ‘bark’; never that their barking has semantic content, let alone in Chinese, regarding their healthy and edible state. As there is no mention of the supposed linguistic dexterity of marmots in other sources, one is tacitly led to assume that this impressive new datum must have had its roots in Dr Ch’uan’s expedition to Manzhouli. This, however, presents us with a pressing ethnographic question.

Ch’uan never mentions venturing to the steppes outside Manzhouli. Even if he had, however, by the time of his visit (late autumn) marmots would be lying in hibernation in their burrows. Thus all access to first-hand information about the tarbagan’s signalling behaviour, and about hunting the animal in general, was barred to him. As a result, his data could but derive from interviews conducted in Manzhouli. In fact, in his own report to the conference (April 5, 1911) Ch’uan dispelled any ambiguity regarding the situation by revealing the source of his data: ‘I called upon Dr. Brisemsky, the Russian railway doctor at Manzhouli, who kindly supplied me with the following information.’<sup>11</sup> Derived not from native informants but from the lips of a Russian medical officer, this comprised in the endemic nature of plague amongst marmots in the area. Ch’uan explicitly attributed this to Zabolotny’s 1898 observations. He further claimed that the Russian railway doctor informed him about the seasonal habits of the tarbagan, and the possibility of cross-infection of new generations of marmots through the use of old infected burrows, a theory that had been the staple of Russian research on the disease in the region. And yet, this was not the only source of information Ch’uan managed to approach during his visit to Manzhouli.

Departing from his encounter with Brisemsky, he continued his report by claiming that he had several talks with marmot hunters in town. Ch’uan asked his indigenous informants whether they knew of the occurrence of any cases of sickness, such as blood spitting, or of sudden death during their hunting season on the steppes. The answer was striking: ‘they replied that they had never known of such cases either on the hills or on their return journey, and that only at Dawoolya, Manchouli, or other towns did the plague attack the marmot hunters and others.’<sup>12</sup> How then, we are moved to ask, did Ch’uan come to the conclusion that ‘according to hunters’ accounts [...] when they [the marmots] are affected with plague they become deaf, their eyes red and partially blind, their paws bloodless, and they creep along slowly and unsteadily’?<sup>13</sup> And what led him to conclude that ‘the hunters profess to diagnose the disease in the freshly killed marmots by making a slit in the paws’?<sup>14</sup> Given that he could not



have observed any of this himself, as marmots were hibernating at the time of his expedition, and that he admitted that the hunters he interviewed denied any knowledge of illness in the hills, the source of the data was either Brisemsky or the medical publications examined in the previous chapters. Given its absence in the latter, the idea that ‘the marmot when in a healthy condition is difficult to catch, rushing about vigorously and uttering a cry *‘bu pa’*, which in the Chinese language means “don’t fear”’ must have derived either from Manzhouli’s Russian railway doctor or another, unacknowledged source.<sup>15</sup>

I would like here to return to my ethnomethodological argument. Like Beliaevsky before them, Wu and Ch’uan appear to have performed a mystifying montage of data, only this time field-based data were mingled with unquoted bibliographical information. Ethnographic data gathered but not observed by Ch’uan in the field merged with information derived from medical journals in a unified narrative that obscured ethnomethodological peculiarities and discrepancies. Most importantly, this was a narrative that did not allow its audience to perceive that none of the described ethnographic data had been directly observed by the authors. On the basis of this mystifying systematisation, Wu proposed a native ‘folk model’ that was grounded in a two-phase process of reconnaissance: (a) the ability to recognise ill marmots from afar, according to their wobbly gait and their inability to cry out, and thus avoid such animals; (b) the ability to distinguish plague-stricken tarbagan, following their capture, by means of an incision on their paw.

This was a model that effectively barred native involvement in the spread of plague from marmots to humans. Who was then to blame for the devastating outbreak? Wu’s verdict here was crucial to the establishment of the aetiology of the epidemic: the human culprit of the outbreak was identified as no other than the old, and repeatedly vilified, figure of Shandong ‘coolies’ who were said to have surged in great numbers to Manzhouli to procure fur for the international market.<sup>16</sup> Wu explicitly identified the alleged greed and hunting malpractices of these migrant workers as the cause of the outbreak: ‘the inexperienced hunters nearly always dig out the marmots from their holes and thus run more risks than the ordinary Mongol, who generally hunts the marmot in the open, or traps it near its abode, thus coming into contact with and catching only healthy animals.’<sup>17</sup> This ‘transitory hand-to-mouth multitude’, as Wu called them, supposedly failed to recognise the signs of plague so open-handedly offered by sick tarbagans. They thus contracted plague whilst cutting down and skinning

the ill animal, although the precise mechanism of this infection pathway remained rather vague in Wu's conference speech.

At the same time as problematising the ignorance and ineptness of 'coolies' as the source of infection, Wu blamed the living conditions of the latter as the source of human-to-human contagion. In this, Wu sought to explain the apparent fact that despite their alleged ineptness in hunting marmots, migrant workers mysteriously failed to perish in the hunting fields. Instead the disease only appeared 'when they are gathered together in the late autumn at the market places'; there they were said to 'crowd into very poor hovels inns, where, with piles of raw pelts, there may often be found from twenty to forty in number, sleeping and eating, in the smallest of most badly ventilated rooms, wherein the conditions are ideal for the encouragement of an epidemic.'<sup>18</sup>

Wu sought to explain this by identifying 'coolie hovels' as amplifiers of pneumonic plague. Providing damning photographs of the crowded hunters' inns, and elaborating on Ch'uan's testimony, Wu would later give a detailed account of the dimensions and structure of the former, noting that therein '[t]he odour was indescribable, being made up of a mixture of foul breath, the vapors of old dirty fur garments and decomposing pelts which were lying alongside the men'.<sup>19</sup> This image drew on a long tradition of representing Chinese working-class forms of dwelling as unsanitary sources of infection and pestilence, especially amplified in the preceding years of the third plague pandemic.<sup>20</sup> This was an image equally applicable to 'coolie hovels' in the steppes, and to urban forms of migrant worker habitation, such as the Harbin slum of Fujiadian, which Wu derided as 'closely packed and built on a low-lying, swampy plain, with narrow streets, inhabited principally by coolies'.<sup>21</sup>

## INTERNATIONAL RECEPTION

In the course of the First International Plague Conference in Mukden, Wu devised a binary that counter-posed two anthropological types: knowledgeable and skilful natives on the one hand and ignorant and inept coolies on the other. This was a binary that internalised and reproduced pivotal colonial class and racial prejudices, and was readily adopted by a wide spectrum of international conference delegates.<sup>22</sup> The list is long and the arguments repetitive but perhaps one case is worth stressing only so as to underline not just the breadth of consensus, but also the scientific weight added to this by grace of the institutional backing of its author. I am referring to the opinion of the Lister Institute's delegate, Petrie, who explicitly tackled the issue of 'the influence of coolie migration on the

spread of the epidemic', explaining how the 'influx of labor' in response to the growing 'tarbagan hunting industry' accounted for a vast expansion of the population of North-West Manchuria.<sup>23</sup> In his insistence on the importance of the railways in 'streaming' the pestiferous 'migratory coolies' south and east, Petrie was supported by Germany's delegate, Martini, who went so far as to allege that 'Shantung coolies travelled first class when the order was given that otherwise prevented them from getting home for the New Year'.<sup>24</sup>

These expert opinions on the pestilential influence of coolies were reflected not only within the closed doors of the conference, but also in the treaty-port press at the time. *The North-China Herald* painted a bleak picture of migrant workers as 'ex-gamblers and opium smokers' constituting 'excellent subjects for plague', whilst preserving an image of pristine wisdom for the native population of the steppes, claiming that 'Mongols usually leave these animals and their haunts severely alone'.<sup>25</sup> Letters by expats seconding this image with claims that 'the trappers are Chinese of the lowest class' were readily reproduced.<sup>26</sup> The attribution of the spread of the disease to 'coolies' hence soon became a light motif in the treaty-port press, which reserved particular scorn towards the fact that the migrant workers ate marmot meat.

Equally well received was the Chinese theory of 'coolie hovels' as amplifiers of the epidemic, with *The Lancet* playing a central role in propagating this outbreak narrative. The journal painted coolies in the darkest colours, claiming they were so averse to washing, or even changing clothes to sleep, that in winter a coat of dirt covered the latter. Quoting Dr Farrar, *The Lancet's* anonymous reporter fully adopted the pathologisation of *kang*-heated dormitories, claiming that 'coolies' sleep 'like herrings in a barrel, in a confined and artificially warmed space, all fresh air being carefully excluded on account of the keenness of the cold'.<sup>27</sup> These 'wretched hovels', as *The North-China Herald* described them, were construed as the ultimate image of unhygienic living conditions of the Chinese working classes—an image quite similar to Taipingshan during the Hong Kong outbreak of 1894 or to the Honolulu and San Francisco Chinatowns during the respective plague outbreaks of 1899 and 1900.<sup>28</sup>

The 'wandering coolies' were linked to unregulated ways of inhabiting space.<sup>29</sup> Wherever they settled, they were seen as set on constructing crammed slums: breeding grounds of pestilence and degeneracy and opium addiction, a cause célèbre of public health reformers.<sup>30</sup> The following passage on Harbin's working-class neighbourhood, Fujiadian, is worth quoting at length as it underlines the construction of a discursive

interphase between the anthropological type of the 'coolie' and pestilent forms of spatiality:

Having increased during the fourteen years the railway has existed from a dozen to three thousand houses, Fuchiatien shelters half of the army of coolies coming to Harbin. Almost all the buildings of this town represent inns, eating houses, opium dens, low-roofed, dirty, half-tumbled-down dwellings swarming with insects and parasites. The quality of food corresponds with the conditions of life. Famine among the unemployed coolies appears regularly every year. Although opium smoking is prohibited both by the Russian and the Chinese authorities, there are a great many secret opium dens in Fuchiatien as well as in Harbin. During the night these dens are invaded by a large number of coolies, who fill every available space in them. If by chance a plague-infected person gets into such a crowded house the disease is most readily conveyed to the others (as has been proved several times by the night patrols), especially by means of the opium pipe, which passes from mouth to mouth.<sup>31</sup>

Adopting long-established colonial ideas about the relationship between race, class, space, and disease, Wu ordered his anti-plague staff to torch down entire blocks in Fujiadian. This was a measure of great symbolic efficacy, as the London Fire of 1666 was supposed to have put an end to plague in the city (a medical myth still prevailing today). But whereas in the case of Hong Kong the mythic element of this solution had been openly exposed by the Director of Public Works, Francis A. Cooper, by 1910 torching had acquired a new, scientific aura of efficiency due to the perceived success of the 1899 Honolulu Chinatown fire in stamping plague from the Hawaiian city.<sup>32</sup> The photographic album composed by Wu and presented to the delegates of the First International Plague Conference visualised this causal link between 'coolie space' and the proliferation of plague, as well as the ability of the Chinese authorities not simply to interrupt it but also eradicate it.<sup>33</sup> The album begins with bird's-eye views of Fujiadian, with the camera thereafter plunging to ground level with an ever more engaging close-up gaze upon its streets and alleys, evincing conditions that supposedly led to the spread of the disease: dirty alleyways, run-down houses, faulty structures, and muddy roads. It then proceeds by depicting the presence and work of Wu's anti-plague staff on the ground (search parties, disinfection teams, doctors, and staff clad in the latest anti-plague uniforms), concluding with victorious vistas of burnt-down shantytown blocks.<sup>34</sup>

In order to fully understand this mode of problematisation of the ‘coolie’ as an anthropological type and its interrelation with ideas about pestilent space, we must not simply refer back to colonial ways of representing working-class and indigenous modes of habitation.<sup>35</sup> Instead we should pay close attention to the term used by Wu to refer to them, a ‘floating population’, and to the way it was embedded in Confucian ethics.<sup>36</sup>

### FLOATING POPULATION

The problematisation of migrant workers during the Manchurian plague outbreak of 1910–11 was grounded on Confucian understandings of order and disorder as resulting from different qualities of flow. Li Zhang helps us understand the significance of this ethical binary through an etymological examination of the term *floating population*, which according to the anthropologist related to a double sense of *liudong* or floating: ‘one is to be lively and unencumbered; the other is to be rootless, unstable, and dangerous.’<sup>37</sup> Whilst he recognises the ambiguity of this term, Li nonetheless stresses its ideological function, claiming that dominant, state-sponsored Confucian discourse ‘tends to invoke and overamplify the negative meanings by emphasising their relationship with related residual terms such as *liumin* (vagrants, homeless people), *liukou* (roving bandits), *liumang* (hooligans), *liucuan* (to flee), *liudu* (pernicious influence), *liuwang* or *liufang* (exiles), and *mangliu* (an unregulated flow of people, which is a transposition of the sounds in the derogatory term *liumang*-hooligans)’.<sup>38</sup>

This binary opposition between harmony/rootedness and chaos/rootlessness played a central role in Confucian ethics, which invested much interest in the question of flow. A problem rather than a property, flow remains to this day a central aspect of Chinese medicine and cosmology.<sup>39</sup> It also forms one of the major baselines of what Li has called the ‘metaphysics of sedentarism’: ‘Earth-bound sentiments are clearly expressed in a widely accepted Confucian saying, *antu zhongqian* (to be attached to one’s native land and unwilling to leave it). Rootedness (not spatial mobility) is taken as the normal state of being in mainstream Confucian culture.’<sup>40</sup>

Both a medical and an ethical category, then, rootedness constituted the gravitational ground for the pairing of bodily and spatial categories. Not only did it bring these categories together in a common problematisation, it actually rendered them thinkable only in terms of one another, or rather in terms of their dynamic relation. What allowed certain qualities

of flow to be considered disorderly or chaotic was this process of thinking bodies via space and space via bodies, mediated as it was by the ultimate value of the 'root'. In accordance to this paradigm, 'coolies' were imagined as a disorderly, mobile source of pestilence that ravages its environs, reflecting the old Chinese proverb 'a rabbit never eats the grass around its nest'—a saying which, according to Li Zhang, implies that 'since migrants are not true members of any urban community, they are most likely to take advantage of it by committing crimes'.<sup>41</sup>

Having rendered 'coolies' a floating population whose very existence was in opposition to harmony as both the aim and guarantor of benevolence/humanness (*ren*), Wu's epidemiological exegesis also operated on a less familiar register, which concerned the supposed lack of skill and knowledge on the part of migrant workers employed as marmot hunters in Northern Manchuria.

#### IGNORANT AND INEPT

The pathologisation of coolies in terms of their disorderly flow and unsanitary dwelling habits was performed vis-à-vis both Confucian values and the image of what Ruth Rogaski has called hygienic modernity.<sup>42</sup> At the same time, their pathologisation in terms of their ineptness and ignorance as regards marmot hunting was performed vis-à-vis the imagined traditional skill and knowledge of native Mongol and Buryat hunters in the region. The question arising here is whether this binary concerns the classic distinction between a native 'natural' relation to nature and a non-native 'cultural' relation to nature.<sup>43</sup> Following Descola and Palsson's discussion, the former is described (and often experienced) as characterised by balance, reciprocity, respect, and innate knowledge. On the contrary, the latter is seen as a utility-oriented realm of estrangement and alienation, destruction, extraction, and exploitation. Useful as this anthropological critique of the nature-culture dichotomy may be in general, its application to the native-coolie binary obscures rather than illuminates the emic aspect of Chinese explanations of the outbreak.

For the ethical epicentre of the native-coolie binary was not an opposition between nature and culture, but one between skill and knowledge, on the one hand, and ignorance and ineptness on the other. Shandong 'coolies' were vilified not as utilitarian or alienated (all traits of a loss via exposure to 'culture') but as innately unskilled and ignorant: 'the newcomers were usually raw and ignorant migrants from the villages of Shandong

who had never seen a tarabagan before and perhaps had never heard of the plague. Hence they caught the animals indiscriminately with snares and even congratulated themselves when they saw a sluggish one.<sup>44</sup>

It is characteristic of this outbreak narrative that plague was conceived as a force of nature, an almost conscious or at least intelligent agent that bided its time and waited for the right human vector to arrive and fall into its trap: an easy prey, an ill, staggering marmot. Such was the alleged idiocy (in the classical sense of the term) of these ‘coolies’ that they were portrayed as gloating over capturing a marmot too ill to escape. ‘One sick animal could provide the spark for the epidemic’, claimed Wu, underlining that ‘inexperience and overcrowding in underground inns, to which the hunters returned from the fields, would supply the necessary fuel for its spread’.<sup>45</sup> Hunting ill tarbagans was hence depicted as an activity befitting dogs and children.<sup>46</sup> Seen as opportunistic ignoramuses, migrant workers were portrayed as unable to acknowledge danger even when this was clearly marked by native hunters. Thus, Wu Liande claimed that when in the summer of 1910 Mongol hunters wisely abandoned the marmot-rich area around the Uganor Lake, following the first signs of epizootic, Chinese ‘coolies’ rushed in the area to take advantage of the relative lack of competition.<sup>47</sup>

The supposed ignorance, ineptness, and greed of ‘coolies’ was epitomised in their practice of digging out hibernating marmots.<sup>48</sup> Although, as eyewitnesses like Cherkassov and Loukashkin testify, digging out, smoking out, and flooding out marmots was in fact standard practice among Mongols and Buryats, a native mode of ‘harvesting’ the animal much more common than trapping or hunting it, Wu claimed it was taboo to Mongol and Buryat hunters.<sup>49</sup> Farrar seconded this, blaming Chinese migrant workers for engaging in the supposedly forbidden practice: ‘the new hands, however, mostly Shantung coolies, who were not such good hunters, preferred digging down into the burrow of the animal and hauling it out by means of a wire noose round its neck, the idea being to avoid injuring the skin.’<sup>50</sup>

A crucial component of the ethnographic configuration of plague in response to the Manchurian outbreak of 1910–11, the skill and knowledge binary opposition between Mongol and Buryat native hunters and Shandong ‘coolies’ invested the former with an authentic, ancient wisdom and the latter with an irresponsible propensity for immediate profit. In this respect, the typological binary did reflect the anthropological nature-culture dichotomy described by Descola and Palsson. But rather than being

its cause, this dichotomy was the product of a pre-existing opposition; hence it lacked characteristics of ‘culturedness’ typical of cases described by the two anthropologists. If we are to use the scheme, we should note that, from the point of view of the binary typology, ‘coolies’ belonged *neither* to culture *nor* to nature: they were banished from both realms of value and meaning, abandoned in a zone of pure indeterminacy.<sup>51</sup>

It is evident that in the course of the First International Plague Conference the native knowledge hypothesis regarding plague assumed new forms and aspects that far surpassed both the ethnographic data on which it was based, and the ways these had already been employed in the preceding fifteen years. The major product of this was the production of Wu Liande’s binary anthropological typology. I have explored elsewhere the biopolitical significance of this in the context of the last months of Qing rule.<sup>52</sup> Here I would like to remain within the epistemological focus of this book, and explore the consequences of this bonding between the native knowledge hypothesis and the tarbagan hypothesis in the years following the Manchurian outbreak of 1910–11.

### THE TWO HYPOTHESES

Going back to the original work of Beliaevsky and Reshetnikov, we should here pay close attention to a crucial side effect of the novel ways in which Wu and his colleagues employed ethnographic data regarding plague in order to foster their explanation of the 1910–11 Manchurian plague epidemic: the peculiar bonding between the two distinct hypotheses, originally proposed by Russian plague researchers in 1895, into a single and unbreakable epidemiological doctrine. The two hypotheses consisted in (a) the assumption that plague in the region was harboured not by rats but by marmots—this was the ‘tarbagan hypothesis’; and (b) the assumption that plague, as a zoonotic disease, and the infection pathway from marmots to humans, was known to native hunters of the animal - this was the ‘native knowledge hypothesis’.

These two hypotheses co-existed and interacted in plague research and literature for fifteen years before Wu adopted them in 1911. Yet what transpired in the course of this adaptation was the forging of a logical bridge between the two hypotheses, so that if one was found to be not true, the other had to be admitted as false too. This peculiar form of epidemiological reasoning is evident in the conclusions Wu Liande drew from the joint Russian-Chinese expedition in the summer of 1911, to which I will now turn my attention.



## THE SUMMER 1911 EXPEDITION

By mid-June 1911, less than three months after the end of the great plague epidemic, rumours of large marmot epizootics north of the Chinese-Russian border were rife.<sup>53</sup> Then, on June 24, Leonid Michailovich Isaev, Zabolotny's protégé, managed to procure from the area a plague-infected tarbagan. The story narrated in his memoirs relates how he, whilst scouring the steppes for diseased Siberian marmots, saw what must have been a terminally ill animal in the open fields: 'I noticed a tarbagan, with no coordination of movement in the full sense of the word crazy, which not only did not run away from me, but approached me.'<sup>54</sup> Whilst on horseback, Isaev threw his raincoat over the animal and took it back to the laboratory station at Borzya, where it tested positive for plague. Cultures obtained from the dead animal's lungs and spleen were injected to the right leg of a healthy tarbagan, which soon perished of septicaemia, bearing 'two large buboes in the right groin only'.<sup>55</sup> Back in his Harbin laboratory, where he had been stationed since the great outbreak, Zabolotny demonstrated the preserved plague-marked organs of the animal, sent over the border by his colleague, to Wu Liande, and invited him to a joint plague-finding expedition.<sup>56</sup>



Illustration 5 Exploring a new burrow. Note the depth

Led by Zabolotny, the Russian expedition party was thus joined by a Chinese medical team led by Wu. The Chinese team arrived at Manzhouli on July 21, proceeding to cross the border into Transbaikalia in search of plague.

Due to the hunting ban in place since the epidemic, the expedition issued permits for 'skilled Russian hunters' to procure ill and healthy animals, offering one rouble for the former and five for the latter.<sup>57</sup> In this manner, eighty marmots were examined in total, of which, to the grave disappointment of the investigators, none was found to be plague positive. Equally fruitless was the examination of marmots in Arabulak and Borzya. Having parted ways with Zabolotny on July 29, the Chinese team moved into Mongolian territory six days later. There, joined by a large auxiliary force, including mounted police, an interpreter, and a Finn tarbagan hunter, Wu and his colleagues camped with a Mongol family in Charbada, sixty-three li south-west of Manzhouli. The natives were described as eating 'besides mutton, a good deal of Tarbagan flesh which is only half roasted before the primitive fire'.<sup>58</sup> Upon enquiry, they claimed that 'there had never been any outbreak of disease resembling human plague in their midst'; disheartened, Wu complained that he could not 'obtain any word of disease among the Tarbagans: the country in which they had lived for many years past abounds in Tarbagans, yet they had never noticed the animals dying'.<sup>59</sup>

Neither there, nor by the shores of Lake Kulun See, where the expedition camped in the following days, was evidence of sick marmots or humans procured. Still, limited as it was, the stay amongst local Mongols did provide Wu with ethnographic data as regards native ways of hunting and skinning marmots.<sup>60</sup> In his report on the expedition Wu offered extensive information on these practices, including technical details, such as how to break the neck of a captured marmot, ways of skinning the animal, the use of double loops, the cost of snares, and the price of marmot skins. What is striking, however, is that Wu's ethnography made no mention of any of the fabled plague-preventative methods employed during marmot hunting. The doctor refrained from commenting on this. Instead he noted a detail that he thought could explain the appearance of the disease not in the hills but in towns: rather than being performed on the spot, skinning was reserved for a time when individual hunters and trappers returned to their common camp after several days in the steppe.<sup>61</sup>

Returning to Harbin on August 25, Wu had found not a single plague-infected marmot and not one individual who could attest to the existence of plague in the region. The only testimony he managed to record, regarding

a potential victim of plague and a doctor who, after examining him, had also died, came from Wu's carriage-driver: 'unfortunately for the veracity of his account, our *isvotschick* was very fond of vodka as we experienced after leaving Tschintansk—he drove very recklessly and eventually dashing up a high bank and upsetting the carriage on the top of us!'<sup>62</sup>

The first trace of the impact of these disappointing findings came a few months later from the director of the Institute of Hygiene in Beijing, Lin Boon Keng in his capacity as China's envoy to the International Sanitary Conference in Paris (November 1911–January 1912). In the minutes of the conference Lin appears upset by Zabolotny's defence of the 'hypothesis that tarabagans are propagators of plague' and that 'the natives (Mongols, Buryats) know very well the symptoms and avoid the hunt of ill animals'.<sup>63</sup> To this Lin retorted that, in his opinion, if human plague derived from marmots, then this could only be via insects and, in that case, it should be bubonic, which was not the case in the Manchurian epidemic. Lin thus concluded that it was humans who infected tarbagan with plague and not the other way around.<sup>64</sup> Whether this harked back to Dudchenko-Kolbasenko's theory of marmots eating human corpses is not clear, although Lin mentioned that it was specifically via the latter that marmots got ill. Going even further in challenging the verdicts of the Mukden conference, Lin argued that the epidemic was imported into Manchuria from South China, via Shanghai and Canton—a tacit nod to Matignon's theory of importation.<sup>65</sup> Given his international prestige, it was easy for Zabolotny to brush away these largely syncretic arguments, noting that the cases observed on the Russian-Mongolian frontier were in fact bubonic, with both bubonic and pneumonic plague spreading to humans by means of direct contact with the skin and the mucous membranes of the animal.<sup>66</sup>

Was Lin Boon Keng's anti-marmot discourse the result of Wu Liande's disenchantment with the tarbagan hypothesis? Or was it an innovative attempt to forge a new hermeneutic bridge between Matignon and Dudchenko's origin hypotheses? What is certain is that the results of the summer 1911 expedition had a decisive effect on Wu Liande, who after a twenty-four-month silence finally aired his scepticism in a forward denouncement of the tarbagan hypothesis. In the conclusion of his October 1913 *Journal of Hygiene* article, which gave the medical community a full review of the Russian-Chinese Expedition, Wu openly mocked the attribution of plague outbreaks to marmot hunting. 'Comment is needless!' he scoffed at a Russian news-clip report about an outbreak near

Chita in December 1912, which claimed that the Cossacks in question had been hunting and eating tarbagan.<sup>67</sup> Wu explained that he could himself attest, on the basis of his 1911 expedition, that there was no evidence of human plague in the region being connected to marmots. Not only, he claimed, did the expeditions not find any plague-infected marmots, but what had been for so long believed to be an ethnographic fact, that natives know of the disease, had also been proven to be incorrect: ‘enquiries made by us directly from the hunters showed that they knew nothing of the alleged epidemic.’<sup>68</sup>

Wu was eager to elaborate on this ethnographic datum, which he apparently held to be of prime importance: ‘in the experience of these hunters not only had no epidemic ever occurred among the Tarbagans but they had never even seen sick ones.’<sup>69</sup> The lack of information on marmot-related plague was not attributed to faulty research methods, native reluctance to respond, or even to some local cultural peculiarity. It was rather taken at face value, as sufficient proof that the ‘folk model’ in place had to be abandoned. Lest there be no doubt about the scientific validity of his argument, Wu was careful to defend his method of obtaining information. He claimed that it ‘was sought by us direct from hunters who were experienced men and who had spent their lives living and hunting in the country where the Tarbagan abounds’, adding that two of the mounted policemen escorting the expedition had been tarbagan hunters in the past. Even the Finn hunter accompanying the expedition, a regular marmot-hunter over the past five years, was said to be able ‘to recall no case of Tarbagan disease’.<sup>70</sup>

For Wu this was sufficient evidence not only insofar as to convince him that native hunters knew nothing of plague, but that, in fact, there had never been such a thing as tarbagan plague in the first place: ‘to conclude that a man whose occupation is that of a Tarbagan hunter and who takes plague has been infected from a Tarbagan is comparable to concluding that a man who sells rice and who develops plague has been infected from rice.’<sup>71</sup>

This is a very striking instance of epidemiological reasoning. For it is one thing to conclude, on the basis of one’s ethnographic data, no matter how these were acquired, that the idea that the inhabitants of a region are aware of a disease and its animal hosts is false; it is another thing to conclude that if the former know nothing about such disease amongst a given animal, then the latter is not a host of the disease, or that the disease does not exist in the area. This was moreover an instance of epidemiological reasoning that explicitly went against conventional ways of treating

the paucity or lack of information regarding plague on the part of native subjects. Examples of doctors accusing the latter of lying or concealing information about plague abound in colonial medical literature, with this attitude being often attributed to the impact of anti-plague measures on these communities. For example, in 1878, Dr Convill's note that in and around Baghdad 'the aversion of the people [...] not only to discuss, but even to mention or acknowledge the presence of the disease, is extraordinary' was treated not as proof that Baghdadis knew nothing of the disease, even less as proof that no such disease exists in the region, but simply as 'one of the necessary and most unfortunate results of the vexatious restrictions to which they are apt to be subjected with the view of arresting the spread of the disease'.<sup>72</sup> Yet Wu's perplexing, and epidemiologically mistaken, conclusion was not a personal lapse, but rather a structural result of the bonding of the tarbagan hypothesis and the native knowledge hypothesis.

The extent and impact of this bonding is evident in the fact that (with exception of Russian plague experts) the international medical community embraced Wu's verdict that the marmot's 'direct relationship to human plague may be considered negligible'.<sup>73</sup> Following the foundation of the North Manchurian Plague Prevention Service, which he chaired, Wu sought to propagate his newly acquired epiphany abroad, where he found a very eager audience. In August 1913 he presented the results of the summer 1911 expedition at the tropical section of the Seventeenth International Congress of Medicine (London), attended by 8000 delegates. The denouncement of the tarbagan hypothesis and of the native knowledge of the disease in the region was consequently widely endorsed in the international medical press.<sup>74</sup> Characteristically, *The British Medical Journal* summary of the proceedings of the Congress concluded that 'Dr. Wu dissipated the belief once so firmly held that the tarbagan (*Arctomys bobac*) was the alternative host of the plague bacillus, and that in fact in Manchuria the marmot took the place of the rat as the carrier of plague'.<sup>75</sup>

It is here, in this neglected but crucial episode in the history of plague epistemology, that the ethnographic configuration of plague assumed its most controversial role. No longer a way of explaining outbreaks and epidemics, or of speculating about the geographic distribution and transmission of the disease, nor even a way for constructing anthropological types, this was now employed as a method of asserting, validating, or invalidating biological evidence.



Illustration 6 Regulation outfit

It would take another ten years for Wu to finally concede that marmots were in fact the source of human plague in the region.<sup>76</sup> The breakthrough came in the aftermath of the second Manchurian pneumonic plague outbreak (1920–21) and, in particular, in the context of a new marmot epizootic in Suktui in late spring 1923.<sup>77</sup> Troubled by the recurrent incidences of plague in the region, Wu, and his colleagues, Pollitzer and Kwan, moved across the border in the hope of establishing once and for all if there was any truth in the connection between marmot epizootics and human plague. They arrived in Suktui on June 10 where they were greeted by the Russian Plague Detachment and Vladimir Vsevolodovich Suknev, who informed them that the Soviets had already bacteriologically confirmed plague in the case of one marmot corpse.<sup>78</sup> Further examinations followed, with the results proving positive. Consequently, on June 18, 1923 the *North China Daily News of Shanghai* received a most urgent telegram from Wu Liande:

The Chinese Research Expedition, working in Siberia with the Soviet doctors, under my leadership, has finally traced the origin of the Manchurian plague epidemics to Siberian marmots. Numbers of sick and dead animals have recently been found, from which arose two fatal cases among the Russian hunters. Our relations with the Soviet [sic] are most cordial.<sup>79</sup>

Wu presented the international medical community with the results of the Chinese-Soviet Expedition during the Far Eastern Congress of Tropical Medicine, held in Singapore on September 1923. Two months later, the summary of the Eleventh Annual General Report of the North Manchurian Plague Prevention Service, November 30, 1923, provided the Chinese Minister of Foreign Affairs with the inevitable conclusion that marmots were in fact the source of human plague in the region.<sup>80</sup> What is however striking, yet also very telling of the epidemiological reasoning in place, is that the resurrection of the tarbagan hypothesis brought another epidemiological skeleton out of the closet: the native knowledge hypothesis. Without any new ethnographic data gathering deemed necessary, after ten years of neglect, Buryats and Mongols were restored to their role of all-knowing native subjects regarding marmot plague and its transmission to humans via hunting. Wu simply resumed the ‘folk model’ he had so skilfully weaved back in 1911. He claimed that Buryats and Mongols ‘possessed ancient traditions and long experience, which taught them to kill the animals by shooting, so that they were able to judge whether their

prey was healthy or not', furthermore embroidering this with unproven ethnographic data, stating that '[p]arties notified one another of the infected spots so that they might avoid them'.<sup>81</sup> At the antipodes of this pristine indigenous knowledge, 'coolies' were also restored to their less enviable role as the culprits of plague.<sup>82</sup> Wu's binary anthropological typology had hence come full circle, with the international medical press fully embracing its ethnographic narrative.<sup>83</sup>

### EPIDEMIOLOGICAL REASONING

We should be careful here not to assume that this double palinode on the tarbagan and the native knowledge hypotheses was the result of some whim on behalf of Wu Liande. There is no doubt that Wu was an accomplished medical scientist, who for a time even held hopes of a Nobel Prize. What led to this epistemological reflux was a peculiar but powerful inferential operation through which Wu found himself entangled in his own epidemiological reasoning. Put simply, once he accepted that the only explanation for the apparent absence of plague epidemics in North Manchuria prior to 1910 was an immaculate traditional native knowledge of the disease, the lack of empirical ethnographic evidence of such knowledge led Wu to refute the very idea that marmots carried the disease. Yet once he was no longer able to deny the latter, he did not draw the conclusion that the concurrent existence of plague among marmots and absence of epidemics in the regions did not necessitate a native knowledge of the disease. Instead he moved to disregard his own empirical evidence, and without any further research on the matter, readopted the native knowledge hypothesis. If back in 1913 Wu had drawn biological conclusions based on ethnographic data, in 1923 he drew ethnographic conclusions based on biological data. Hence, rather than challenging the epistemological bond between the tarbagan hypothesis and the native knowledge hypothesis, Wu chose to maintain the epidemiological cluster he had forged during the Mukden conference, regardless of the fact that this had in the first place led him astray, in concluding that marmots did not carry plague.<sup>84</sup>

In the years following the rehabilitation of the two hypotheses Wu continued to study plague, so as to produce much valuable research on the disease as well as an authoritative book on pneumonic plague published by the League of Nations in 1926. In these publications Wu would often return to the native knowledge hypothesis. Weaving more ethnographic data into his model, he would integrate to it narratives such as Skrzhivan's



Padmasambhava story, hence amplifying the former's aura of authenticity.<sup>85</sup> Yet, at the same time, Wu would also recognise some of the most striking epidemiological mistakes of the hypothesis, admitting, for example, that a wobbly pace was not a reliable indicator of plague amongst marmots, or that the pinkish axillary gland believed to be avoided by native hunters as a means of plague prevention was actually a physiological rather than pathological trait of the animal.<sup>86</sup> Still, whilst these concessions struck at the very heart of the native knowledge hypothesis (and of armchair medicalisations of native mythology), Wu never took the step to reflect upon the validity of this mode of reasoning about plague. Even though he found and accepted as false some of the cornerstone data of the ethnographic configuration of plague on the Chinese-Russian frontier, he never ventured to challenge the artifice itself.

This operation should be read as a structural trait of epidemiology, as a science on the one hand faced with urgent public health problems, whilst on the other hand moving inexorably towards a condition where the collection of ever more data and the inclusion of ever more factor-levels in understanding and reasoning about a given disease leads not to a fuller, more confident, and more actionable knowledge, but to the realisation that this disease, in our case plague, and how it actually operates on the ground, cannot be fully known.<sup>87</sup> Could, by contrast, native lifestyles have appeared empirically knowable, 'out there', and tangible? Could they have seemed a reliable source of information that could be unambiguously objectified in terms of ethnographic studies and surveys? We are thus tempted to ask whether ethnographic information may have appeared to provide some sort of solid ground in a sea of ever-changing scientific methods and soaring data about plague. This, I want to argue in the concluding chapter of the book, is a question that relates not just to Wu Liande, but to the wider ethnographic configuration of plague during the third pandemic.

## NOTES

1. Pneumonic plague cases had been observed in other outbreaks, with the Indian Plague Commission in particular paying attention to this form of plague. Other major pneumonic plague epidemics during the third pandemic include the Manchurian plague outbreak of 1920–21, the 1920s plague epidemics in Madagascar, and the 1924 plague outbreak in Los Angeles, California.

2. Putting the start date earlier, Sihm Kyu-hwan (2009: 168) claims that ‘on September 16, 1910, the first plague patient was identified among the Chinese carpenters working near the Russian border’.
3. These dates are approximate, and follow Nathan (1967), as reports are conflicting. For example, during the First International Plague Conference Wu Liande claimed that plague reached Harbin on November 7, only to provide a table claiming it arrived in the same city on December 4, 1910. Wu (1912: 20) noted that he could not vouch ‘for the accuracy of the dates’.
4. C.F. Nathan, *Plague Prevention and Politics in Manchuria 1910–1931* (Cambridge MA: Harvard East Asian Monographs, 1967). W.C. Summers, *The Great Manchurian Plague 1910–1911* (New Haven: Yale University Press, 2012); M. Gamsa, ‘The Epidemic of Pneumonic Plague in Manchuria 1910–1911,’ *Past and Present*, vol. 90 (2006): 147–184; Ch. Hu, ‘Quarantine Sovereignty during the Pneumonic Plague in Northeast China (November 1910–April 1911),’ *Frontier History of China*, vol. 5, no. 2 (2010), pp. 294–295.
5. C. Knab, ‘Plague Times: Scientific Internationalism and the Manchurian Plague of 1910/1911,’ *Itinerario*, vol. 35, no. 3 (December 2011): 87–105. D. Luesink, ‘The History of Chinese Medicine: Empires, Transnationalism and Medicine in China (1908–1937).’ In I. Borowy (ed.) *Uneasy Encounters: The Politics of Medicine and Health in China 1900–1937*, pp. 149–176 (Frankfurt am Main: Peter Lang, 2009); S. Hs.-L. Lei, ‘Sovereignty and the Microscope: Constituting Notifiable Infectious Disease and Containing the Manchurian Plague (1910–11).’ In A.K. Ch. Leung & C. Furth (eds.), *Health and Hygiene in Chinese East Asia: Policies and Publics in the Long Twentieth Century*, pp. 73–106 (Durham: Duke University Press 2011); Liew K.K., ‘(Re)Claiming Sovereignty: The Manchurian Plague Prevention Services (1912–31).’ In I. Borowy (ed.), *Uneasy Encounters: The Politics of Medicine and Health in China, 1900–1937*, pp. 125–148 (Frankfurt: Peter Lang, 2009).
6. R.J. Perrins. 2006. ‘Debating Disease: The History of the Manchurian International Plague Conference of April 1911’ <http://www.helsinki.fi/iehc2006/papers2/Perrins.pdf>.
7. Nathan, *Plague Prevention and Politics in Manchuria 1910–1931*.
8. For two important contemporary overviews of Russian research and policy during the Manchurian outbreak of 1910–11 see Bogutsky (1911) and Yasensky (1912).
9. Wu L.-T., ‘Address of the Chairman.’ In R.P. Strong (ed.) *Report of the International Plague Conference (held at Mukden in April 1911)*, pp. 18–25 (Manila: Bureau of Printing, 1912), p. 19.
10. Wu, ‘Address of the Chairman,’ pp. 19–20.

11. Ch'uan Sh. Ch., 'Some Observations on the Origin of the Plague in Manchouli.' In *R.P. Strong (ed.) Report of the International Plague Conference (held at Mukden in April 1911)*, pp. 27–30 (Manila: Bureau of Printing, 1912), p. 30. On the role of Russian Chinese Eastern Railway doctors in the fight against plague see Hsu (2006) and Ratmanov (2009).
12. Ch'uan, 'Some Observations on the Origin of the Plague in Manchouli,' p. 29.
13. Ch'uan, 'Some Observations on the Origin of the Plague in Manchouli,' p. 29.
14. Ch'uan, 'Some Observations on the Origin of the Plague in Manchouli,' pp. 29–30.
15. Ch'uan, 'Some Observations on the Origin of the Plague in Manchouli,' pp. 29–30.
16. For an examination of the economic consequences of the outbreak on migrants see Li and Li (2013).
17. Wu, 'Address of the Chairman,' p. 20.
18. Wu, 'Address of the Chairman,' p. 20.
19. Wu L.-T., & The Hulun Taotai, 'First Report of the North Manchurian Plague Prevention Service,' *The Journal of Hygiene*, vol. 13, no. 3 (October 1913), pp. 248–249.
20. On Hong Kong's Taipingshan and accusations of Chinese working-class forms of habitation as responsible for the plague see Sutphen (1997a). On plague and Honolulu's and San Francisco's Chinatowns see Mohr (2005), Shah (2001), and Risse (2012). On plague and urban planning in West Africa see Bigon (2015).
21. Wu, 'Address of the Chairman,' p. 23.
22. It should be noted here that another bias that worked to the advantage of Wu's binary was the Western colonial idea that the Chinese were inadequate hunters, who placed more emphasis on deceit than sportsmanship (Peckham 2014).
23. G.F. Petrie, 'An Epidemiological Review of the Epidemic of Pneumonic Plague in Northern China 1910 to 1911,' in *R.P. Strong (ed.), Report of the International Plague Conference (held at Mukden in April 1911)*, pp. 409–427 (Manila: Bureau of Printing, 1912), p. 418.
24. Collective, 'Session XIII, April 18, Discussion of Programme.' In *R.P. Strong (ed.) Report of the International Plague Conference (held at Mukden in April 1911)*, pp. 267–282 (Manila: Bureau of Printing, 1912), p. 312.
25. Anon., 'Plague in Manchuria,' *The North-China Herald* (January 20 1911), pp. 124 and 133.
26. W.J. Stanton Pyper, 'Untitled Letter,' *The North-China Herald* (January 27 1911), p. 193. The letter was a reprint from *North-China Daily News* (undated).

27. Anon., 'Propagation of Plague without Aid of Infected Rats,' *The Lancet*, vol. 178, no. 4607 (December 16, 1911), p. 1714. The only dispute regarding the coolie hypothesis seemed to revolve around the precise mode of transmission, with Paul-Louis Simond holding that 'coolies' infected each other via fleas, whilst Reginald Farrar insisted on the unmediated form of contagion. For further discussion see Anon. (1914).
28. Anon., 'The Plague in Manchuria,' *The North-China Herald* (January 20, 1911), p. 133; on these outbreaks see note no. 19.
29. Anon., 'The Plague' (Mukden March 15, 1911), *The North-China Herald* (April 1, 1911), p. 26.
30. Fr. Dikötter, L. Laaman & Zhou Xun, *Narcotic Culture; a History of Drugs in China* (London: Hurst & Co, 2004).
31. W. Bogucki, 'The Plague Epidemic at Harbin.' In R.P. Strong (ed.) *Report of the International Plague Conference (held at Mukden in April 1911)*, pp. 200–207 (Manila: Bureau of Printing, 1912), pp. 201–202.
32. Hong Kong Government Gazette, [GA 1895 no.117], p. 264. Cooper insisted that it was sanitary measures and not the fire that eradicated plague in London. On the Honolulu fire disaster-turned-success see Mohr (2005).
33. Wu L.-T., *Views from Harbin (Fuchiatien) Taken During the Plague Epidemic (December 1910-March 1911)* (Shanghai: n. p., 1911).
34. This anti-plague method was not confined to Fujiadian. In Changchun, too, coolie shanties were burnt down as an anti-plague measure (Putnam Weale 1911).
35. P. Stallybrass & A. White, 'The City: The Sewer, the Gaze and the Contaminating Touch.' In J. Farquhar & M. Lock (eds.), *Beyond the Body Proper: Reading the Anthropology of Material Life*, pp. 266–285 (Durham: Duke University Press, 2007).
36. Wu L.-T., *A Treatise on Pneumonic Plague* (Geneva: League of Nations, 1926).
37. Li Zhang, *Strangers in the City; Reconfiguration of Space, Power, and Social Networks within China's Floating Population* (Stanford: Stanford University Press, 2001), p. 33.
38. Li, *Strangers in the City*, p. 33.
39. On its application to the SARS outbreak see Lynteris (2013b).
40. Li, *Strangers in the City*, p. 33.
41. Li, *Strangers in the City*, p. 141.
42. R. Rogaski, *Hygienic Modernity; Meanings of Health and Disease in Treaty-Port China* (Berkeley: University of California Press, 2004).
43. Ph. Descola & G. Pálsson, 'Introduction.' In Ph. Descola & G. Pálsson (eds.), *Nature and Society: Anthropological Perspectives*, pp. 1–22 (London: Routledge, 1996), p. 3.

44. Wu L.-T., 'Hosts and Carriers.' In Wu L.-T., J.W.H. Chun, R. Pollitzer & C.Y. Wu (eds.), *Plague, A Manual for Medical and Public Health Workers* (Shanghai: National Quarantine Service, Shanghai, 1936), p. 32.
45. Wu, 'Hosts and Carriers,' p. 32.
46. Wu L.-T., *A Treatise on Pneumonic Plague* (Geneva: League of Nations, 1926), p. 145. Although later Loukashkin (1937a: 2107) would claim that native hunters' dogs were able to recognise ill marmots.
47. Wu, *A Treatise on Pneumonic Plague*, p. 76.
48. The attribution of greed and sometimes lust to coolies was perennial to British colonial views of them as 'a menace to the Colony' (Tsai 1993: 147).
49. Wu, 'Address of a Chairman,' p. 20; A.A. Cherkassov, *Notes of an East Siberian Hunter* (Bloomington: Authorhouse, 2012); A.S. Loukashkin, 'The Tarbagan or the Siberian Marmot and its Economic Value.' In *Comptes rendus de XXIe Congrès International de Zoologie, Lisbonne 1935*, pp. 2233–2293 (Lisbon: Casa Portuguesa, 1937).
50. R. Farrar, 'Plague in Manchuria,' *Proceedings of the Royal Society of Medicine*, vol. 5 (Section of Epidemiology and State Medicine) (October 1911), p. 20.
51. For further discussion see Lynteris (2013b).
52. C. Lynteris, 'Skilled Natives, Inept Coolies: Marmot Hunting and the Great Manchurian Pneumonic Plague (1910–1911),' *History and Anthropology*, vol. 24, no. 3 (2013): 303–321. For a discussion on the politics of control of coolies across the Chinese-Korean border see Kim (2014).
53. Wu L.-T., (G. L. Tuck), 'Investigations into the Relationship of the Tarbagan (Mongolian Marmot) to Plague,' *North Manchurian Plague Prevention Service Report*, vol. 1 (1911–1913), pp. 11–12. This article was also published in *The Journal of Hygiene* (October 1913) under the title 'First Report of the North Manchurian Plague Prevention Service'.
54. Quoted in Ya. K. Gimmel'farb & K.M. Grodsky, *D. K. Zabolotnyi (1866–1929)* (Moscow: Gosudarstvennoe Izdatel'stvo Meditsinskoï Literaturui, 1958), p. 66. Leonid Mikhaïlovich Isaev (1886–1964); not to be confused with the other plague expert, Vasily Isaevich Isaev (1854–1911). Wu Liande spells Isaev, Issaief.
55. Wu, 'Investigations into the Relationship of the Tarbagan (Mongolian Marmot) to Plague,' p. 26. The experiment was conducted by Zabolotny—four more dead tarbagan suspected of being plague-infected tested negative.
56. Wu, 'Investigations into the Relationship of the Tarbagan (Mongolian Marmot) to Plague,' p. 12.
57. Wu, 'Investigations into the Relationship of the Tarbagan (Mongolian Marmot) to Plague,' p. 27.

58. Wu, 'Investigations into the Relationship of the Tarbagan (Mongolian Marmot) to Plague,' p. 31.
59. Wu, 'Investigations into the Relationship of the Tarbagan (Mongolian Marmot) to Plague,' p. 30.
60. Wu was not a foreigner to ethnography. In the first volume of the North Manchurian Plague Prevention Service Reports we find a short paper by Wu (1913b) on the Fishskin Tatars of Lahasusu. Referring to the Nanai (Goldi), in accordance to their use of salmon skin for the construction for garments and shoes, Wu gave a brief sketch of anthropometric, habitation, and occupational aspects of the particular group, as well as of its suffering from periodic attacks of smallpox. In the following years, doctors of the North Manchurian Plague Prevention Service would return to study the Nanai (Jettmar 1929).
61. Wu, 'Investigations into the Relationship of the Tarbagan (Mongolian Marmot) to Plague,' p. 38.
62. Wu, 'Investigations into the Relationship of the Tarbagan (Mongolian Marmot) to Plague,' p. 27.
63. D.K. Zabolotny, 'Communication relative aux recherches de la Commission Scientifique Russe Envoyée en Mandchourie à l'occasion de l'épidémie de peste pneumonique.' In Ministère des Affaires Étrangères (ed.), *Conférence Sanitaire Internationale de Paris, 7 Novembre 1911-Janvier 1912*, pp. 487–491 (Paris: Imprimerie Nationale, 1912), p. 490.
64. Zabolotny, 'Communication relative aux recherches de la Commission Scientifique Russe Envoyée en Mandchourie à l'occasion de l'épidémie de peste pneumonique,' p. 505.
65. Zabolotny, 'Communication relative aux recherches de la Commission Scientifique Russe Envoyée en Mandchourie à l'occasion de l'épidémie de peste pneumonique,' p. 505.
66. D.K. Zabolotny, 'Observations,' in Ministère des Affaires Étrangères (ed.), *Conférence Sanitaire Internationale de Paris, 7 Novembre 1911-Janvier 1912*, p. 508 (Paris: Imprimerie Nationale, 1912), p. 508.
67. Wu, 'Investigations into the Relationship of the Tarbagan (Mongolian Marmot) to Plague,' p. 44.
68. Wu, 'Investigations into the Relationship of the Tarbagan (Mongolian Marmot) to Plague,' p. 45.
69. Wu, 'Investigations into the Relationship of the Tarbagan (Mongolian Marmot) to Plague,' p. 45.
70. Wu, 'Investigations into the Relationship of the Tarbagan (Mongolian Marmot) to Plague,' p. 45.
71. Wu, 'Investigations into the Relationship of the Tarbagan (Mongolian Marmot) to Plague,' p. 47.
72. House of Commons Parliamentary Papers [C.2262], p. 30.

73. Wu, 'Investigations into the Relationship of the Tarbagan (Mongolian Marmot) to Plague,' p. 47. It should be noted, however, that it took some time for the news to trickle down, as we still find endorsements of the previous paradigm as late as two years later in the European medical press (e.g. Vincent 1915).
74. Anon., 'Notes From China (From our Own Correspondent), Peking June 10th,' *The Lancet*, vol. 182, no. 4689 (July 12 1913) p. 112; G. Lambert, 'Marmotte et peste de Mandchourie,' *Annales d'hygiène et de médecine coloniales*, vol. 17 (1914): 691–692.
75. Anon., 'Summary of Proceedings of 17th International Medical Congress: Tropical Hygiene and Hygiene,' *The British Medical Journal*, vol. 2, no. 372 (August 16 1913), p. 413.
76. In the meantime, and in spite of extremely challenging political conditions in the region, Wu would continue to experiment with marmots, without however recognising their role as sources of plague amongst humans (see Ebersson et al. 1917; see also Wu's papers in the *North Manchurian Plague Prevention Reports*).
77. Wu L.-T., 'Plague in Wild Rodents Including Latest Investigations into the Role Played by the Tarabagan (Read at the Far Eastern Congress of Tropical Medicine, Singapore, September 1923),' *North Manchurian Plague Prevention Service Reports*, vol. 4 (1923–1924), pp. 128–129.
78. Suknev (1924) was a prominent Soviet plague-expert and author of an influential book on the endemicity of the disease in the region.
79. Anon., 'Pneumonic Plague,' *The China Medical Journal*, vol. 37, no. 7 (November 1923), p. 602.
80. J.W.C. Chun, 'Summary of Eleventh Report, 1923,' *The North Manchuria Plague Prevention Service Reports* (1923–1924), p. 280.
81. Wu L.-T., J.W.H. Chun & R. Pollitzer, 'A Record of Pneumonic Plague Outbreaks Throughout the World from the Earliest Times,' *North Manchurian Plague Prevention Service Reports* (1923–1924), p. 77.
82. Wu, Chun & Pollitzer, 'A Record of Pneumonic Plague Outbreaks Throughout the World from the Earliest Times,' p. 78.
83. Anon., 'Annotation: An Arraignment of the Bobac,' *The Lancet*, vol. 204, no. 5268 (August 16 1924), p. 330. It should be noted here that the uncritical re-endorsement by the international medical press, and the lionisation of Wu as the discoverer of the tarbagan zoonotic link, came under attack by Clemow (1924) in an open letter to *The Lancet*. At the same time, the ethnographic configuration of plague would continue in the region both under Soviet and under Japanese auspices, with diverse impacts on plague research under the two 'empires'.

84. This would be further defended and elaborated upon in Wu's consecutive work, reaching its apex, in terms of systematicity, in his *Treatise on Pneumonic Plague* (1926).
85. See Chap. 4.
86. Wu, 'Plague in Wild Rodents Including Latest Investigations into the Role Played by the Tarabagan.'
87. M. Kosoy, 'Deepening the Conception of Functional Information in the Description of Zoonotic Infectious Diseases,' *Entropy*, vol. 15 (2013): 1929–1962.



## Conclusion

### Ethnographic Data as Medical Evidence

This book has not been intended as a history of plague or plague research on the Chinese-Russian border in the line of inquiry inaugurated by Carl Nathan.<sup>1</sup> Nor has it, however, been an effort to reconstruct native understandings or responses to the disease in the spirit of Carol Benedict and Florence Bretelle-Estabet.<sup>2</sup> Rather, it has aimed to draw a critical anthropological analysis of an important epidemiological practice: the ethnographic configuration of plague. As we have seen, this followed different pathways and directions, each elliptical and at the same time open-ended, which came under a unified rubric and formed a unitary outbreak narrative only under the urgency of a devastating plague epidemic.

The central question raised by my examination of the ethnographic configuration of plague concerns what Sokhieng Au first coined in her study of French colonial medicine in Indochina as epidemiological reasoning.<sup>3</sup> After July 1894, scientific problematisations of plague took as their starting point the bacteriological identification of the disease. Yet, at the same time, the reach and scope of questions posed around the third plague pandemic far surpassed the laboratory and its epistemic reach. Whilst bacteriology established the identity of the pathogen, in other words *what plague was*, it was unable to determine *what plague did* (and how it did it): its zoonotic, climatological, geographic, and social ecological profile. In this sense, bacteriology's power was limited to establishing the causative agent of plague. What remained an open question was how plague operated within and between human and non-human animal populations in

different physical and social contexts. In other words, whilst bacteriology ascertained the identity of plague's pathogen, it did not and could not by itself establish an understanding of plague *as a disease*.

It is true that this limitation applies to all infectious diseases, but it is perhaps especially relevant to plague. For its pathogen, *Yersinia pestis*, is an organism that assumes an extraordinary spectrum of animal hosts (over 203 rodent hosts, and several other mammals and birds) and an impressive range of parasitic vectors (primarily but not exclusively flea species). Plague as a disease is defined by three clinical forms (septicaemic, pneumonic, and bubonic), several transmission pathways, and a very complex enzootic and epizootic epidemiology, which, 120 years after the discovery of the causative bacillus, scientists are still only beginning to understand. The result of research on different aspects of plague has been an enormous amount of data, as well as a range of methodologies and epistemological frameworks through which these have been examined.<sup>4</sup> In his recent work on what he calls epistemological entropy, Michael Kosoy, a leading plague researcher at the US Centers for Disease Control and Prevention, has problematised this plethora of data in a way that underlines an information paradox. Kosoy notes an 'increasing disconnection between the accruing body of information about infectious agents, infected organisms, influence of environmental factors on epidemic processes, and our limited understanding of infectious processes'.<sup>5</sup> This has led him to formulate the hypothesis that as more data is gathered about a disease such as plague, the more we dwell in a realm of uncertainty or entropy as regards 'the description of all components of host-pathogen systems at the population and community level'.<sup>6</sup> This is a paradox in the sense that we are generally trained to believe that more information leads to more certainty, more accurate predictions, and an overall more clear and confident understanding of a given phenomenon.

As regards plague, I would, however, like to argue that epistemological entropy is not a recent phenomenon. Since the first major outbreak of the third plague pandemic in 1894 in Hong Kong, studies of plague witnessed a qualitative and quantitative explosion, with a rough bibliographical review procuring more than 1000 papers in scientific journals between that year and 1934. Only in 1897, there were 109 scientific publications on the subject in Russian alone.<sup>7</sup> To give an idea of the complexity faced by medical scientists in the course of the pandemic, it suffices to note that between 1899 and 1901 Eager lists over 100 plague outbreaks across the globe, each generating its own data that swiftly found their way to scientific

publications.<sup>8</sup> The global dissemination of these data was such that we find discussions of the 1910–11 Manchurian epidemic in Argentine treatises on plague, or anxious reporting of plague statistics in India in the California daily press.<sup>9</sup> Data gathered and published locally or nationally during the peak of the pandemic thus became part of a global circulation and exchange of plague-related evidence. But most importantly, they were entangled in international debates about scientific methods and theories regarding their interpretation and the extrapolation of knowledge about plague both as a global disease and as a disease particular to specific locations, landscapes, and urban environments. At different historical points, authors such as W.J. Simpson and Wu Liande assumed the task of summing up existing data and approaches in the form of authoritative treatises on plague.<sup>10</sup> Yet these were by no means universally accepted systematisations of plague-related data and theories, and were, in the majority of aspects discussed, very soon surpassed, challenged, or invalidated by new evidence or approaches to the disease.

With this condition of complexity in mind, the question I posed at the end of the last chapter needs to be taken up. Can the ethnographic configuration of plague be linked to this phenomenon of epistemological entropy? Could it be that in some cases medical scientists responded to the uncertainties raised by ever-changing scientific methods and soaring evidence on plague, by anchoring their epidemiological reasoning on ethnographic data?

### ETHNOGRAPHIC OBJECTIFICATIONS

Though it can be read as part of a much longer heritage of sociological thinking that has implicated anthropologists in long debates about ‘social facts’, the concept of ethnographic evidence is one that has only recently come into focus in anthropological theory.<sup>11</sup> In his introduction to a special issue of the *Journal of the Royal Anthropological Institute* on the subject, Matthew Engelke draws on R.G. Collingwood’s classic essay on historical evidence in asserting the disciplinary specificity of evidence. Yet, at the same time, he stressed, following Chandler et al., that ‘in attending to the disciplinary specificity of evidentiary protocols, we need to be aware that evidence is defined not only by questions but also by competing pressures and regimes’.<sup>12</sup> In the case of plague during the third pandemic generally, but also on the Chinese-Russian frontier in particular, the questions asked of ethnography principally had to do with two aspects of plague: its

epidemicity and its endemicity. At the time, questions regarding the epidemicity of a disease conventionally concerned aspects such as transmission pathways (airborne, waterborne, etc.) and virulence, as well as population and individual immunity to the disease: in other words, all aspects pertaining to the spread of disease amongst and between human and non-human populations. In the same epidemiological framework, questions of endemicity related to the disease's ability to persist in a given area via hosts functioning as reservoirs of the pathogen, and hosts functioning as in-betweens amongst different susceptible species. As we have seen in the course of this book, these dynamic notions were of the utmost importance to scientists trying to make sense of plague in the action of both studying and containing it, with the link between the endemic and epidemic states of the disease forming a key in problematisations of plague. As a result, the primary questions asked of ethnographic data concerned both plague's patterns of transmission from non-human animals to humans and amongst human subjects, and plague's patterns of persistence amongst non-human animal populations.

Such questions were posed, developed, and explored within a context of imperial and scientific antagonisms over plague-related knowledge: a knowledge constitutive of administrative measures and policies against the proliferation of the disease on a global scale. This was an institutional antagonism between agents and agencies eager to 'possess' plague in all its symbolic efficacy.<sup>13</sup> What was at stake in the course of this agonistic, imperial quest for plague was not simply the identification of the disease's epidemic and endemic patterns. Equally important was the configuration of these patterns in relation to specific biopolitical and geopolitical problems and opportunities in the regions under scrutiny and on a much larger, global imperial scale.

Why then ethnography? In what way did ethnography contribute to epidemiological reasoning regarding plague? What was it that made ethnographic data a desirable resource to which scientists would return, in their effort to provide answers to different questions, but also so as to raise new ones, about the disease? If, following Engelke, 'it is what we do with facts—not only what questions we ask from them, but how we justify them to be "facts" in the first place—that makes ethnography important', then we need to ask how the evidential facticity of ethnographic data functioned within the wider epistemological conundrum of plague science at the time.<sup>14</sup> In other words, how were ethnographic data configured or 'harnessed' as evidence about plague in relation to dynamic, and often conflicting, epistemological frameworks during the third pandemic?<sup>15</sup> Following Carrithers's

analysis of Raymond Firth's classic work on the Tikopia, Engelke argues that 'when human patterns emerge out of ethnographic ones, confirmed as such by a community of critical readers, and in a sense independent of the intentions of an author, they gain shape as ethnographic evidence'.<sup>16</sup> When approached critically, this emergent 'robustness and independence that confirms [the] reliability' of a given datum may, however, be read as a phantom objectivity that allows the former's application in the explanation of phenomena similar to or altogether different than those intended by its original collector or systematiser.<sup>17</sup> We have already seen how in the course of the third pandemic ethnographic data collected on the Chinese-Russian frontier were rendered into such free-floating signifiers of plague, functioning as what Webb Keane has called 'portable objectifications'.<sup>18</sup> A prime example of this was the alimentary avoidance of marmot axillary glands on the part of Mongols.<sup>19</sup> Whilst this was initially collected and published by Gustav Radde as an ethnographic datum unrelated to plague, it was later rendered into ethnographic evidence of plague amongst the aforementioned animals, and of the knowledge of the zoonotic source of the disease amongst indigenous groups in the region.

Of course, ethnography was not the sole source of such portable objectifications, but rather functioned within a much wider economy of evidence derived, amongst other disciplines, from photography, cartography, and bacteriology. This was, for example, the function of photomicrographic plates of plague bacteria, of nosological maps and climatological charts, as well as of clinical photographs portraying men, women, and children exposing axillary or cervical buboes in identical poses that functioned as prototypes of plague patients.<sup>20</sup> The role of ethnographic data within this evidential economy was more pronounced in certain epidemic and biopolitical contexts, such as the one examined in this book, whilst less in others. In each of these contexts it operated in relation to other evidential regimes and within concrete medical and biopolitical conditions arising from the demographic character of the given outbreak as well as from the power relations in place between administrators, scientists, and the general population. Hence it would be altogether mistaken to analytically assert a universal function of ethnography as regards plague research, beyond the simple parameter that, in the context of the aforementioned phenomenon of epistemological entropy, it functioned as a potential anchor for epidemiological reasoning. What is more promising, analytically speaking, is to examine the transformation of ethnographic data into plague evidence in specific epidemiological contexts.

As we have seen in the course of this book, as regards the case of the plague research on the Chinese-Russian frontier the operation of configuring ethnographic data into epidemiological evidence was neither homogeneous nor underlined by a unitary research or interpretive culture. Different researchers took recourse to ethnographic data in different contexts with the aim to elucidate different epidemiological phenomena. In this process they rendered the former into evidence about a disease that itself had no stable identity but whose understanding changed as rapidly as new outbreaks stroked different parts of the globe. For Beliaevsky, it appears that this was an operation influenced by the ethnological spirit of the great geographic expeditions of late Imperial Russia. In this sense, we can say that he wrote his short but important report in implicit dialogue with explorers such as Gustav Radde, who was the first to scientifically describe the animal identified by Beliaevsky as the principal host of plague in the region. Writing twenty years later, Dudchenko seems to have operated under similar ideological conditions. Unlike the more cautious (or perhaps less ambitious) Beliaevsky, however, he ventured not only to employ ethnographic data he collected in the field, but also to speculate about practices such as Buddhist pilgrimage. By contrast, for Maignon ethnographic data were part of a more specific interest in what we would today call ethnomedicine, but what for him was a mixture of useful traditional remedies and detrimental superstitions. His Urga expedition, his book on 'Chinese superstition', and the numerous pieces he wrote on various exotica attest to this interpretation. By comparison, Zabolotny appeared more reserved in his ethnographic descriptions, though he had ample opportunity to collect such data not only in Weichang but also in India, Arabia, and in other areas he visited as a prominent member of the Russian Plague Commission. As for Wu Liande, he initially sought to gather first-hand ethnographic data and relate them to plague in the context of a major epidemiological crisis, finding henceforth himself entangled, if we are allowed the Geertzian metaphor, in the 'webs of significance' he had spun.

Similarly, each of these paragons of plague research in the region had a different repertoire of concerns or questions about plague as a disease in relation to which he employed ethnographic data. Beliaevsky's concern was immediate: to explain and contain the 1894 outbreak in Suktui. He does not appear to have written another piece on plague before or after this incident.<sup>21</sup> His collection of ethnographic information on the disease hence seems to be solely concerned with supporting the tarbagan hypothesis, Beliaevsky's revolutionary idea of a zoonotic origin of plague related

to Siberian marmots. Matignon was similarly concerned with explaining a specific and limited outbreak of plague, which involved identifying its geographic origins and the means by which it was introduced or, in his terms, ‘imported’ into the region from South China. Zabolotny, working in the footsteps of Matignon, shared the same concerns, with the crucial difference that he turned his attention not to the importation of the pathogen but to its perseverance in the area under study. Hence if Matignon employed ethnographic data as evidence regarding plague’s epidemicity, Zabolotny used such data to problematise the disease’s endemicity. Dudchenko by turn took a more synthetic and adventurous approach in employing ethnographic data. He did this in the process of crafting his peculiar model of plague as an anthroponotic disease with a complex epidemic and endemic pattern, asserting that plague spread from humans to marmots and then back to humans via native pilgrimage and burial rites. Finally, Wu exhibited the most complex, if ultimately palindomic, employment of and engagement with ethnographic data. Initially, he took recourse to ethnography so as to defend the tarbagan origins of the first Manchurian plague epidemic (1910–11) and to explain the spread of the disease on the basis of an anthropological binary between skilled and knowledgeable native hunters and inept and ignorant migrant ‘coolies’. He then proceeded to employ newly collected ethnographic data (or, to be precise, the lack of such data) so as to challenge the tarbagan hypothesis, only to end up readopting the hitherto discredited native knowledge hypothesis on account of having to concede to the tarbagan hypothesis under the strain of new epidemics in the region.

We can thus say that in each of these cases ethnographic data were rendered into evidence about plague at strategic points of each researcher’s epidemiological reasoning as regards the formulation of different hypotheses on the origins and trajectory of the disease. For Beliaevsky, ethnographic data functioned as a support of his and Reshetnikov’s pioneering tarbagan hypothesis; for Matignon, as an aid to his importation hypothesis; for Zabolotny, as accessories of his endemic theory; and for Dudchenko, as a bolster of his anthroponotic model. In these cases the employment of ethnographic data was an intelligent, if often misleading, attempt to stave epistemological uncertainty, and to bridge existing gaps in biomedical evidence as regards the areas or outbreaks under examination. Where the rendering of ethnographic data into evidence about plague assumed a qualitatively different epistemological function was in the case of Wu Liande. In this instance we do not simply have an opera-

tion of epistemological support or evidential patching up, but rather a meticulous engineering of ethnographic data into epidemiological evidence, which generated an outbreak narrative encompassing and often overdetermining several important aspects of the disease. In this case, ethnographic data were employed not simply to uphold a given process of epidemiological reasoning, or to provide a shortcut out of some evidential deadlock. Rather, they were rendered into epidemiological evidence in a manner that transformed and even challenged or negated biomedical evidence about the disease. This epistemological transformation was most evident in the case of Wu rejecting the tarbagan hypothesis largely on account of not being able to procure ethnographic data in support of it, an unprecedented act of epidemiological reasoning.

Yet at the same time as the transformation of ethnographic data into epidemiological evidence configured plague as a disease with set characteristics of epidemicity and endemicity, the ethnographic configuration of plague on the Chinese-Russian frontier had another effect, this time not with regards to the disease it aimed to problematise but in relation to the ethnographic subjects whom the said data were supposed to derive from and describe.

### ETHNOGRAPHIC SUBJECTS AS SUBJECTS OF AND AGAINST PLAGUE

Conventionally, as Ann Kelly puts it, medical anthropologists are interested in ‘practices, experiences and understandings that medical knowledge excludes’.<sup>22</sup> Instead my study has turned its analytical lens on practices and experiences included and framed by medical knowledge. ‘Disease’, claims David Arnold, ‘was a potent factor in the European conceptualisation of indigenous society.’<sup>23</sup> As discussed in the Introduction of this book, this was primarily done through a negative representational strategy that depicted indigenous groups as essentially pestilential societies. It was moreover an operation usually predicated upon a description of a given group as incapable of perceiving the key factors contributing to the spread of an infectious disease. These may, according to each case and narrative, include the source of the infection, practices contributing to its spread, or general (un)sanitary conditions that give rise to it in given contexts. It was a narrative most often focused on the supposed inability of a group to identify these patterns even in and amongst



what, from a colonial perspective, formed its defining cultural, social, and economic practices. An example of this is evident in the description of a limited plague outbreak in Abu Ghraib in the autumn of 1875. Doctors from the Sanitary Administration of Baghdad who visited the so-called seat of the epidemic related that the disease had first struck the camels of nomadic Arabs who were camping in the area for pasturage. Rather than avoiding the afflicted animals, the said nomads proceeded to slaughter and eat the ill camels. As a result, forty of them fell sick and died, demonstrating fever and glandular swellings.<sup>24</sup> This type of narrative fostered a critique of indigenous groups as societies of ignorance: societies unable to draw knowledge or even ‘observation and common-sense inferences’, to use Evans-Pritchard’s famous phrase, from amongst their most fundamental hand-to-mouth, life-sustaining practices.<sup>25</sup> This negative representational strategy reached its zenith in cases where entire ethnic groups were identified with a particular disease. The best-known example of this is the Fore of Papua New Guinea and their association with kuru. As Warwick Anderson has painstakingly demonstrated, this was a long-term operation of medical and anthropological identification of the Fore as a society suffering from an array of phenomena classified initially as culture-contact shock and hysteria, then as an unidentified syndrome, and finally as an infectious disease caused by a ‘slow virus’ that was identified as a prion.<sup>26</sup> These consecutive problematisations of kuru led to two Nobel Prizes, but also to the progressive reduction of the Fore into ‘a mobile archive of signs and numbers’ related to the particular disease: ‘the bodies of the Fore and their social life were reframed in terms of kuru, the territory was being reconstructed along the lines of kuru, the census of the Fore was a kuru census, and the map of the Fore was a kuru map.’<sup>27</sup> Most recently similar representational strategies have been employed in the problematisation of what in current epidemiological practice are understood as ‘emerging’ zoonotic diseases, such as SARS and Ebola, often through the re-employment of the notion of ‘cultural vectors’, first developed in the 1980s by Paul Ewald.<sup>28</sup> Cultural vectors refer to ‘a set of characteristics that allow transmission from immobilized hosts to susceptible when at least one of the characteristics is some aspects of human cultures’.<sup>29</sup> What this model basically implies is that whilst a given host may be relatively non-mobile, a disease can still manifest itself in the form of enhanced virulence through socio-cultural practices.<sup>30</sup> In the case of SARS, the 2003 pandemic of a respiratory disease hitherto unnoticed amongst humans, led to widespread

accusatory practices targeted against South Chinese live animal markets. Described in scientific literature as well as in the daily press as the loci of SARS's spillover from civet cats to humans, these so-called 'wet markets' became central to the development of a new Yellow Peril discourse, which portrayed China and Chinese lifestyles as potential sources of 'the next pandemic'; an all-encompassing contagion event that one day soon will supposedly threaten humanity with extinction.<sup>31</sup> In the case of the recent Ebola outbreak (2014), what has come into the focus of both scientific and popular science discourses is the threat of infection posed by burials—a theme, as already discussed, with a long colonial heritage. As discussed by Umberto Pellecchia, this isolation of burial practices as cultural vectors of Ebola in West Africa not only ignores the complexity of funerals as social phenomena, but has also contributed to overlooking crucial economic and political aspects of the crisis.<sup>32</sup>

By contrast, the cases examined in the course of this book comprise a seemingly affirmative strategy of representation—a conceptualisation of indigenous groups on the Chinese-Russian frontier as societies-against-plague. Between 1894 and 1926 Mongols and Buryats were configured in medical and epidemiological literature as ethnographic subjects whose culture was largely a reaction to the menacing presence of plague in their physical environment. Key myths, rituals, and burial practices as well as modes of hunting were seen as little more than exotic cultural expressions of a baseline survival strategy against plague. Part of a naturalist ontological enclosure, this mode of representation is, nonetheless, distinct from what Byron Good has critiqued as anthropological strategies of interpreting cultural traits as 'unintended adaptive effects'.<sup>33</sup> Although in our case too culture is seen 'as a set of adaptive responses to diseases', the difference lies in the interpretative emphasis placed on native intentionality.<sup>34</sup> Whereas the neofunctionalist ecological paradigm has focused on the net effect of adaptation, independent of and in the absence of intention on the part of the 'adapted', the epidemiological narrative examined in this book put emphasis on native intentionality as key to survival vis-à-vis plague. Mongols and Buryats were portrayed not simply as having 'adapted' to the conspicuous presence of plague in their environment, but also as able to articulate this cultural adaptation in a proto-scientific manner, explicitly attributing, for example, specific hunting practices to their desire to prevent zoonotic infection. In other words, they were seen as having developed not only cultural immunity to plague, but also a consciousness of this trait.

## FUNCTIONAL UNREASON

The point that we should not overlook here is that this mode of conceptualising native societies did not simply valorise their culture as epidemiologically aware or intentional. It also, subtly but I would claim pervasively, represented this culture as hopelessly irrational. For if these groups did realise that plague was present in their prey (marmots), and if they did understand the mode of transmission from marmot to humans (direct fluid contact), then an immediate question was raised: why instead of employing complex mythological and ritual apparatuses did they not simply wear leather gloves so as to prevent infection from the potentially ill animals? This query is only tacitly raised in medical literature, yet it always hovers between the lines, much like an example of what George Steinmetz has coined the ‘epistemological unconscious’, in plague ethnographies of the region.<sup>35</sup> The conclusion the audience of this outbreak narrative is led to draw is that knowledge does not necessarily make up for the lack of reason, which was seen by plague researches as constitutive of native culture. Mongols and Buryats may know plague, in the sense that they understand its zoonotic source and mode of transmission, and they may even have developed an adaptive intentionality with regards to the disease, but they are unable to reason about it in a rational manner—this is the intended lesson of the native knowledge hypothesis across its many variations. Hence, if this was a seemingly affirmative representational strategy, in that it praised native knowledge of plague, it nonetheless asserted an even more radical form of othering than its negative representational antipodes: indigenous groups could know or not know certain aspects of an infectious disease, but in either case they were incapable of reasoning about it rationally.

There is a crucial difference between the phenomenon I am trying to underline here and what Byron Good, following the much broader analytics of experience developed by Rodney Needham, has described as the pervasive juxtaposition between belief and knowledge in colonial medical narratives.<sup>36</sup> An example of this juxtaposition as regards plague is evident in Baber’s report on the disease in Yunnan, a region often considered as the origin of the third pandemic. Baber gives in his account a dry description of the disease that he claimed bore ‘a resemblance to the plague of London described by Defoe’, focusing on the progression of symptoms suffered by the victim: glandular pustules, weakness, aches, delirium,

and pseudo-convalescence leading to death.<sup>37</sup> This list of symptoms is no more precise than similar descriptions from the Middle Ages, and yet the idiom in place conjures up an aura of scientific objectivity. This image of ‘knowledge’ gives way to one of ‘belief’ when it comes to discussing Chinese understandings of the outbreak. Baber reported that, following the description of a French missionary, ‘the native version includes all the above facts, but includes them in a cloud of superstitious accessories.’<sup>38</sup> These consisted in the saturation of the sick room by demons: ‘even the tables and mattresses writhe about and utter voices, and offer intelligible replies to any one who questions them.’<sup>39</sup> The contrast here, in Good’s analytical terms, is clear: on the one hand the epistemic clarity of a list of symptoms that somehow manage to count as knowledge, and on the other hand the superstitious haze of demonology. And yet, whereas the belief/knowledge dichotomy presumes that the indigenous group in question is eventually capable of knowing a disease in its immediate environment, the epidemiological reasoning I have examined institutes the imagined gap between the scientific ego and the non-scientific other as an unsurpassable ontological condition. For if all that natives lack is knowledge, then the gap in place is merely epistemological. And in that case one can always equip the former with ‘scientific knowledge’ through education, the assumption being that as evidence or ‘proof’ becomes available then they would shift from a mode of believing into a mode of knowing. This model of otherness then asserts an ontological continuum on the basis of which an epistemological paradigm shift (from them to us qua from past to present, in social evolutionary terms) can be achieved. In the case I have examined in this book, however, this ‘progression’ towards identity seems not to be an option. Instead Mongols and Buryats were presented as already (one is tempted to say always already) possessing knowledge of plague. More than that, they were presented as possessing this knowledge before scientists, and hence providing it to plague researchers. And yet, in spite of this empirical knowledge accumulated, the narrative goes, over centuries, native subjects still failed to reason about the disease in a rational way. Whereas in the case of the ‘progression towards identity’ model of otherness we have a classical dialectical operation, according to which the accumulation of quantity (knowledge) leads to a transformation of quality (reason), in the second case we are faced with an anti-dialectical cul-de-sac. Mongols and Buryats, though praised for their knowledge and

for containing plague in the region, are nonetheless condemned to a static ontological condition: that of functional unreason.

### TOWARDS A CRITICAL EPIDEMIOLOGY

In the course of the third pandemic, the ethnographic configuration of plague was tied to an epidemiological interpellation of ethnographic subjects. This was an apparatus of association between social forms of the native other and forms or processes of pathogeny, which found a plethora of biopolitical applications in the context of global epidemiology in the age of high colonialism. But it is also an apparatus that continues to inform our postcolonial world, and the problematisation of epidemics, from SARS in 2003 and the Haitian cholera outbreak in 2010 to the Ebola epidemic in West Africa in 2014. Whether seen as possessing traits of cultural immunity to or as being burdened by cultural vectors of infectious diseases, societies in the global south continue to be studied and evaluated in terms of their ability to survive, adapt to, and cope with what are usually described as pathogens arising out of or endemic to their natural or built environments. And at the same time, infectious diseases continue to be ethnographically configured, with scientists taking recourse to ‘beliefs’, ‘folk-models’, ‘tradition’, and, ultimately, ‘culture’ for their problematisation and understanding. These largely anthropologically redundant categories are more often than not treated as portable objectifications, so as to model, explain, and dramatise disease in the face of epistemological entropy forged by: the social, biological, and ecological complexity of infectious diseases, the irreducibility of multihostal zoonoses, and the syndemic entanglement of co-infection processes, economic inequality, environmental destruction, and structural violence. In this way, not only is the complexity of epidemic crises obscured, but, at the same time, ‘behaviour change’ becomes a ‘pragmatic’ priority over political-economic reform. Adopting a critical stance towards this approach to public health requires us to excavate the impact of colonial medicine and its epidemiological reasoning in the present predicament of global counter-epidemic interventions and global health as a whole. With the imperative of moving towards a relational understanding of disease in mind, and with the hindsight of the implications of the employment of ethnography in the understanding of plague in the course of the third pandemic, the historical and anthropological critique of epidemiology can become a key tool for forging a new critical epidemiology.<sup>40</sup>

## NOTES

1. C.F. Nathan, *Plague Prevention and Politics in Manchuria 1910–1931* (Cambridge MA: Harvard East Asian Monographs, 1967).
2. C.A. Benedict, *Bubonic Plague in Nineteenth-Century China* (Stanford: Stanford University Press, 1996); Fl. Bretelle-Estabet, 'Les épidémies en Chine à la croisée des savoirs et des imaginaires: le Grand Sud aux xviii<sup>e</sup> et xix<sup>e</sup> siècles,' *Extrême-Orient Extrême-Occident*, vol. 34 (2014): 21–60.
3. S. Au, *Mixed Medicines: Health and Culture in French Colonial Cambodia*. (Chicago: Chicago University Press, 2010)
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11. For a critical discussion of these debates and their relevance to the anthropological notion of culture see Herbert (1991).
12. M. Engelke, 'The Objects of Evidence,' *Journal of the Royal Anthropological Institute*, (N.S) vol. 14, s. 1, Issue Supplement: The Objects of Evidence: Anthropological Approaches to the Production of Knowledge (April 2008), p. 5; R.G. Collingwood, *The Idea of History* (Oxford: Oxford University Press, 1946); J. Chandler, A. Davidson & H. Harrotonian (eds) *Questions of Evidence: Proof, Practice and Persuasion Across the Disciplines* (Chicago: Chicago University Press, 1994).

13. I am here borrowing the notion of possessing a disease from Anderson (2000: 714).
14. Engelke, 'The Objects of Evidence,' p. 7.
15. I am here borrowing the notion of harnessing ethnographic data from Chua et al. (2008: 12), whose agonistic undertones do more justice to this process than the idiom of 'translation' employed by Latour in his 'circulating reference' thesis (1999).
16. Engelke, 'The Objects of Evidence,' p. 11.
17. Engelke, 'The Objects of Evidence,' p. 11.
18. W. Keane, 'Estrangement, Intimacy, and the Objects of Anthropology: Reflections on a Genealogy.' In G. Steinmetz (ed.) *The Politics of Method in the Human Sciences: Positivism and its Epistemological Others*, pp. 59–88 (Durham: Duke University Press, 2005).
19. See Chap. 3.
20. Whether these ever achieved some sort of 'optical consistency' (Latour 1986) as regards to their application in the study of plague remains an important question.
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26. W. Anderson, 'The Possession of Kuru: Medical Science and Biocolonial Exchange,' *Comparative Studies in Society and History*, vol. 42, no. 4 (October 2000): 713–744.
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31. For a discussion of the 'wet market' complex see Zhan (2005); on the mythic dimensions of the 'next pandemic' see Caduff (2014) and Lynteris (2016); for a critique of 'emergence' see King (2004).
32. U. Pellecchia, 'Do Traditions Spread Ebola?,' Ebola Response Anthropology Platform (2014). Available at: [http://www.ebola-anthropology.net/case\\_studies/do-traditions-spread-ebola/](http://www.ebola-anthropology.net/case_studies/do-traditions-spread-ebola/). For critical anthropological approaches to the association of 'traditional' funerals and Ebola see also Richards (2014) and Fairhead (2015).
33. B.J. Good, *Medicine, Rationality and Experience: An Anthropological Perspective* (Cambridge: Cambridge University Press, 1997), p. 41. Good here refers to the work of Foster and Anderson (1978), Alland (1970), and McElroy and Townsend (1985).
34. Good, *Medicine, Rationality and Experience*, p. 41.
35. G. Steinmetz, 'Introduction: Positivism and Its Others in the Social Sciences.' In G. Steinmetz (ed.) *The Politics of Method in the Human Sciences: Positivism and its Epistemological Others*, pp. 1–58 (Durham: Duke University Press, 2005). The question of gloves is raised in *The Lancet* after the rehabilitation of the tarbagan hypothesis, where the anonymous correspondent notes that 'hunters do not use gloves, their hands are often chipped, and during skinning operations the plague bacillus may readily gain entrance directly into the circulation through an open wound' (Anon 1924: 30).
36. Good, *Medicine, Rationality and Experience*; R. Needham, *Belief, Language, and Experience* (Oxford: Blackwell, 1972).
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38. House of Commons Parliamentary Papers [C. 2262], pp. 39–40.
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40. For a recent endorsement of a relational approach of infectious disease see Hinchliffe et al. (2013).



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