An Historical Framework for International Scientific Collaborations: The Case of Kitasato Shibasaburo

by

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BSc, University of Victoria, 2008

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Supervisory Committee

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Abstract

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The Japanese scientist Kitasato Shibasaburo (1853-1931) was one of the founders of microbiology. A devoted student of Robert Koch, his successful collaborations with European scientists resulted in anti-serums for tetanus and diphtheria, the discovery of the causative agent of the bubonic plague, and a number of other major contributions to both science and public health. He achieved this in spite of condescending attitudes on the part of many of his peers and even resistance from within his own government. Yet there remains a paucity of academic writing on Kitasato in the English language, particularly when compared to his eminent contemporaries. What does exist constructs a narrative of an historically weak Japanese scientific establishment. This work challenges that perspective, and will examine Kitasato's interactions with his fellow collaborators in the context of the considerable social, political, cultural, and linguistic pressures acting upon them in order to elucidate what made them so extraordinarily successful in surmounting these barriers. In so doing it aims to provide insight for the scientists of today – for whom international collaboration is the ever-increasing norm – as to how they have succeeded historically and can now successfully interact with both each other and the powers that organize them.

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Chapter 1 – Introduction

I: Introduction

It may seem unlikely that a samurai of relatively humble birth, born in a remote mountain village in southwestern Japan in the years immediately before the end of the feudal era and the emergence of Japan as a modern nation-state, would rise to prominence as one of the most influential minds – and personalities – of his day. But to students of Japanese history, Kitasato Shibasaburo (1853-1931) can be seen as part of a rising tide of Japanese intellectuals constrained by the limits placed on their studies by the Tokugawa shogunate, only to burst forth and flood the national scene when that government was overthrown by modernization-minded rebels. Yet very little is written in English on Kitasato compared to the other Meiji elite, even though his ideas would have influence far beyond the borders of Japan – even though every living person today is still directly and profoundly affected by them.

Kitasato, in collaboration with his more famous German contemporary Emil von Behring (1854-1917), were the founders of science of immunology. Together they discovered that higher organisms produce anti-toxins – what we now know as "antibodies" – to resist foreign invaders, that these anti-toxins are found in the blood serum, and that this serum can be extracted and injected into another organism to confer immunity to the invader upon it. It had been known for some time before this, of

course, that higher organisms somehow resisted disease, but both the mechanisms – not to mention any practical or curative applications those mechanisms might have – were unknown, and theories ranged from cells dedicated to hunting and destroying the invaders (this would turn out to be correct as well, though concrete evidence and medical applications for it would half a century in the future) to rest and simple hygienic improvements. In Kitasato's time, even the germ theory of disease itself was still highly controversial; hygienists, as opposed to germ theorists – germ theory having been first espoused by Kitasato's own mentor Robert Koch (1843-1910) – believed that disease was a result of cellular and chemical imbalances in the body caused by poor sanitation practices. They had as their evidence the observation that improved cleanliness lead to improved overall health, and ample ammunition with which to criticize the germ theorists: the tiny organisms they claimed were the pathogens of disease were found everywhere in nature, even on healthy persons. Discoveries by in Berlin by Koch and his pupils like Kitasato, as well as those of their fellow germ theorist rivals in the Pasteur Institute of France, would change the course of medical history forever. Kitasato's own discovery would result in the creation of antisera: the first effective curative (as opposed to preventative) therapy for the treatment of infectious disease produced by modern medicine. Kitasato and Behring had discovered that the blood serum of an animal exposed to a toxin contained "antitoxins" to neutralize its effects; this "antiserum" could be produced in mass quantities to treat human patients suffering from the effects of a bacterial toxin. Kitasato himself would also go on to participate in and preside over some of the most successful collaborations of the era, including the discovery of the

causative agent of the Black Plague and the first treatment for syphilis, to found and direct an influential research institute, and to shape the future of Japan's medical modernization and views on public health.

Why then is the material to be found on Kitasato so scarce, compared to that on the politicians, economists, translators, and even poets of the Restoration? A study of what materials do exist (Chapter 2) written in English, in comparison to German and Japanese, is illuminating: the influence of language, of nationalism, and of the divide between historians and scientist-historians was profound. More importantly, Kitasato's life history and accomplishments, his influence in politics, his numerous and productive international collaborations and those of his students, contrast sharply with a conventional narrative that dominates discussions of the history of science in Japan prior to the Second World War and American Occupation to this day: that of an historically weak, ineffectual scientific establishment crippled by feudal factionalism, controlled minutely by a militaristic government, and having lacked the same historical background of science and enlightenment seen in Europe, hopelessly backward. Nobel-nominated Kitasato, stepping directly out of a feudal country alleged to have no history of science to become the founder of a scientific discipline, is not a suitable protagonist for telling that particular story. This work presents an alternate narrative, both to the development of science in early modern Japan, and to the conventional narrative of scientist as solitary genius with technicians and assistants as minor players, and in so doing provide a collaboration-focused alternative to historical representations of scientific discovery as a zero-sum game.

So how does one rationalize the discoveries of Kitasato – among other influential Japanese medical scientists of his time – with the conventional narrative of pre-war Japanese weakness in science? Morris Fraser Low points to a number of historical tropes often employed to just such an end: the first being the "Butterflies and Frigates" fallacy – that Japan had no knowledge of "the West" or technology prior to 1868 – followed by that of the "Unique Imitator" – that Japanese persons had some special cultural or even ethnic characteristic that enabled them to acquire this learning at a faster rate than the rest of Asia – to explains any exception that might appear[1]. Instead, this work will emphasize the long tradition of science, particularly medical science, from which Kitasato and his peers benefitted. Moreover, it will use historical evidence to delineate the ways in which the Japanese scientific establishment, rather than lacking a "traditional" historical period of enlightened free inquiry altogether, had from the beginning possessed a "modern" bureaucratic structure. Consequently, it will assert that the collaborations that form the crux of the material under investigation closely resemble the modern variety.

Of primary concern will be the mechanisms by which information was exchanged and status negotiated between the various actors, with a particular eye to what was successful and what was unsuccessful and the implications for scientific collaboration both present and historically. To this end a theoretical framework constructed using the principles of Marcel Mauss' gift theory is employed[2]. The role of the scientist as an agent of the state he represents, particularly in international collaboration, as described by Bruno Latour in his *Pasteurization of France* does much to explain the decisions made

and actions taken by these scientists[3], but where Latour saw calculated exchanges between scientists in return for credibility as a direct parallel to exchanges of capital in a modern capitalist economy, other social historians and sociologists of science such as Warwick Anderson, Warren Hagstrom, and Lewis Hyde observed competitive gift exchange. Scientific information traded within the community is traded as a "gift" – that is, inalienable from the giver, which creates both a relationship between the giver and the receiver and an expectation that the gift will be at some point reciprocated. "Gift" in this sense serves as the opposite concept from "commodity" – an alienable object whose exchange neither creates a relationship between transacting parties, nor is an expectation of future transactions. While there is considerable evidence that scientific information is becoming increasingly commodified in the form of patents, gift theory more accurately describes the exchanges in Kitasato's time. But the gifts produced by Kitasato and his collaborators are not given freely: payment is expected in kind from their peers, and in the form of resources, recognition, and prestige from the state and the general public. Thus the two theories are part of the same whole: the scientist as knowledge producer and state legitimiser is made possible by ideas and observations interpreted and "freely" contributed to the public discourse. Who wins and who loses – or who is perceived to have won or lost – these competitive exchanges have ramifications for the prestige of any nation's scientific establishment, and effects how the history of science in that nation is perceived.

Under investigation are some of Kitasato's most famous and influential discoveries. As a student sent by the Japanese government to study under Robert Koch

at the University of Berlin (Chapter 4) where, in collaboration with Behring and his mentor, he discovered natural immunity and serum therapy in 1890; as a Professor and newly famous microbiologist sent again by the Japanese government to find the cause of the 1894 plague epidemic in Hong Kong (Chapter 3); and as the director of the national Institute of Infectious Diseases overseeing Shiga Kiyoshi (1871-1957), Hata Sahachirō (1873-1938), and Paul Ehrlich's (1854-1913) work on dysentery and syphilis, deeply enmeshed in both the politics of the international scientific community and the politics of the nation (Chapter 5). This will allow us to consider Kitasato as a scientist at very different levels of prestige, and how this influenced his decision-making.

Several elements are beyond the scope of the thesis: while the social, political, and linguistic aspects – as well as how all three are irrevocably intertwined – of these collaborations are considered in depth, the colonial and racial dimensions require a more thorough treatment than can be provided within the scope of the investigation; moreover, detailed investigations into the colonialist nature of the science conducted in Asia during Kitasato's era already exist within the literature – such as Michael Shiyung Liu's study of the research conducted by Kitasato's students in Imperial Japan's colonies[4] – and this work seeks to tread new ground. Similarly, while there is a dearth of information on Kitasato's life and role as a scientist in English, James Bartholomew's historical investigations describe his role as an administrator in the founding of the Japanese scientific establishment in detail[5], so this role will be treated only briefly in final chapter.

To summarize: the next section will be dedicated to a description of the theories employed in the construction of the schematic used to analyze them as well as a description of the schematic itself; namely, scientist as state agent and knowledge producer, and the scientist's part in his or her gift community, followed by a short history of Kitasato's life and works. The second chapter consists of a review of the literature available about him. The role of the conventional narrative of an historically weak Japanese scientific establishment will be investigated, as well as the role of historians and scientist-historians of various backgrounds in constructing this narrative. The final three chapters will follow Kitasato's career of international scientific collaborations, investigating them through the use of this framework.

II: Theoretical Framework

A Definition of Terms

In this investigation the following terms require definition: science, scientist, scientific community, scientific establishment, and scientific collaboration. These terms are not unambiguous and so the interpretation utilized within this thesis requires definition, as well as the theoretical basis from which these definitions are derived.

Throughout the work **science** will be defined as the study of the natural world by means of empirical observation. These empirical observations are then used to made deductions about the phenomena observed; the deductions, in turn, used to produce

and disseminate knowledge about nature. This work will not define science as a means by which knowledge about nature is derived directly from empirical observation, but rather will utilize Bruno Latour's interpretation: that scientists construct this knowledge and distribute it[6]. They do so within the framework of the prevailing theories of the time – what Thomas Kuhn has named a scientific "paradigm". A change in the dominant theoretical framework to one that better explains empirical observations is a "paradigm shift". Kitasato's career spanned such a shift: Germ Theorists, such as Kitasato and Koch, took a different view of the causative element of disease than Hygienists. To the former, disease was caused by discrete organisms; to the latter by means of cellular imbalances caused by poor living conditions. Both conclusions were derived from empirical evidence – namely, that discrete organisms could be harvested from diseased individuals, and that as sanitation improved so did public health, but the scientists interpreted these observations differently. The distinction between empirical data based on direct observation and the knowledge constructed from this data will be crucial in the following discussion, as each has different methods by which it is exchanged.

Scientists are individuals trained to make empirical observations about the natural world, generally sanctioned by an official body within their nation of origin, often in the form of a degree. In this sense, scientists are representatives of their nation of origin: considerable investment is often made by a governing body in their education, and they are recognized as such by various methods of institutional recognition, such as the Nobel Prize. In this thesis a scientist's national, ethnic, political, institutional, and

economic identity, as well as his identity *as a scientist* will all be considered valid lines of inquiry. In his studies of the interactions between individuals during the annexation of Okinawa to Japan and its subsequent occupation by American forces, Tomiyama Ichirō determined that no distinction can be made between these identities in any given individual – none of them can be safely ignored – when considering the "small" politics between persons and the "large" politics of nation-states[7]. Interactions between individual scientists in this thesis will be investigated in the context of Tomiyama's findings; that is, that all of these politics are relevant.

In this thesis the **scientific community** will differ slightly from the **scientific establishment**. The latter will be defined as the organizations within each nation that conduct the training of new scientists, organize the professional lives of established scientists, and interact with both the state and the scientific establishments of other states in an official capacity. The former will refer to scientists internationally; those engaged in the pursuit of scientific knowledge as a whole. Clearly there is some overlap between these concepts, yet the distinction is important, as individual scientists will interact differently with both the "establishment" and "community" of science, and each has different degrees of operational and ideological freedom. Additionally, this work takes a Foucaultian post-structuralist approach to describing the institutions of establishment, community, and state – i.e., that these structures are to some degree socially constructed, but fundamentally inseparable from their respective realities[8]. As Latour observes, the scientific establishment is of particular interest to nation-states because it is engaged in the production of new power; it is this power that scientists use

to negotiate with their states for resources and recognition in the form of prestige[9] (see below). Kitasato's use of the power created by his constructed knowledge serves as a dramatic example of this principle – his remarkable ability to negotiate his own individual terms with agents of Japanese state power can thus be explained[10]. State power, conversely, while constructed in the same way relies on concepts, notions, or mythology – *dispositifs*, according to Foucaultian theory – already embedded within the population[11]. The use of scientist as symbol – of the success of a "scientific" or "modern" worldview, or as an intellectual champion of a given nation-state – helps to explain why the myth of the solitary genius, explored in the preceding chapter, is so pervasive in the history of science. If an achievement is seen as a collaborative effort, this cultural capital must necessarily be shared – possibly with those from a different scientific establishment, or with values and beliefs greatly distinct from those upheld by his fellow collaborator. After all, those within the scientific *establishment* – as Kitasato, the director of government-funded laboratory certainly was – are also agents of state power. But this is not so for all of those within the scientific *community*, who may be agents of private financial interests, military interests, non-governmental organizations, or otherwise.

Science as Gift Exchange

Since the crux of this investigation will focus on exchanges – of information, observations, constructed knowledge, favours, or resources – an exchange theory must be established. While Latour views calculated exchanges between scientists in return for

credibility as a direct parallel to exchanges of capital in a modern capitalist economy, I concur with Warren Hagstrom that the competition he observed was instead a form of competitive gift exchange, and this thesis will approach the scientific community as a gift community[12]. Scientific information traded within the community is traded as a "gift" – that is, inalienable from the giver, which creates both a relationship between the giver and the receiver and an expectation that the gift will be at some point reciprocated. Gift in this sense serves as the opposite concept from "commodity" – an alienable object whose exchange neither creates a relationship between transacting parties, nor is an expectation of future transactions. While there is considerable evidence that scientific information is becoming increasingly commodified in the form of patents, in Kitasato's time most exchanges would have been inalienable. This notion of the scientific community as a gift community was first espoused by Warren Hagstrom in a sociological context, then by Lewis Hyde in a theoretical context; scientific information is received by the community as "contributions" for which the scientist is not paid, but expects to receive in kind, and those who remove themselves from the community to commodify their work for private interests are generally looked down upon by the group. Scientists' identities as members of the scientific establishment and agents of the state complicate this exchange relationship; however, scientific knowledge as economic exchange helps to explain the interactions of state scientists with colonized peoples. Kitasato's experience of sampling the cadavers of plague victims in Hong Kong without the consent of the people but rather the permission of the British Empire parallels both

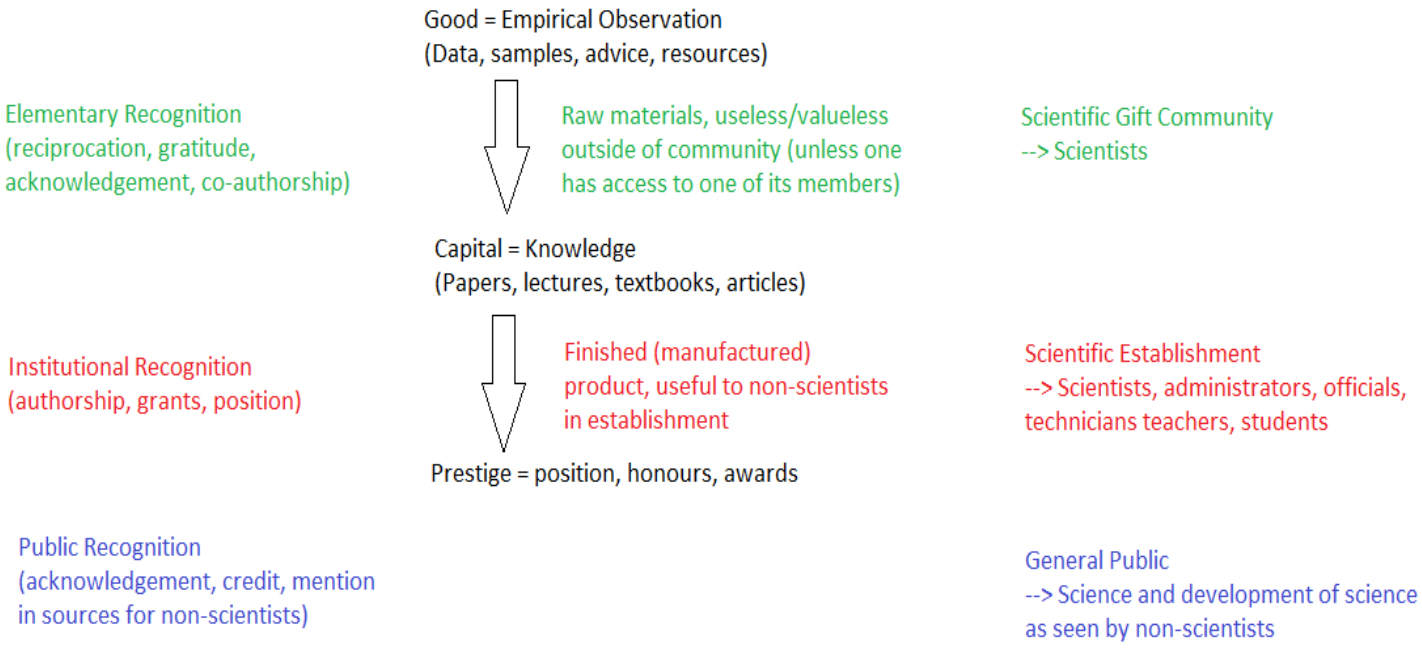
Anderson's observations about the treatment of Kuru victims in Papua New Guinea and the utilization of indigenous resources by colonizing powers[13].

Extrapolating the theoretical framework established thus far provides the definition of "scientific collaboration" used in this work. The exchange of information within the scientific community – which encompasses all scientists across the globe – takes the form of gift exchange, inalienable from the scientists providing the information. Thus, a scientific collaboration is the exchange of ideas and resources between two or more scientists, in which the knowledge produced by the exchange is inalienable from all parties. Furthermore, in their function as agents of the state, an international collaboration between scientists can be seen as a form of gift exchange between the nation-states involved. Once again, this interpretation is ideally suited to Kitasato's time: Mauss describes gift exchange as particularly prevalent, and necessary, between groups whose relationship is tenuous or uncertain because of the expectation of reciprocity, and this could well describe the way various colonial powers jockeyed for international position prior to the First World War[14].

This model, along with the gift and commodity theories of C.A. Gregory[15] – that exchanges become increasingly commodified toward the periphery of a gift community – is used to derive the following schema by which Kitasato's exchanges will be analyzed: firstly, that the basic unit or **good** of scientific exchange takes the form of information (data, raw materials, methods, technological improvements), which is used as described by Latour in the production of knowledge (journal and newspaper articles, scientific

papers, lectures) or **capital**. The choice of these terms is deliberate, for it is at the knowledge stage that exchanges begin with those outside the scientific gift community, and it is at this stage that these materials become alienable – e.g., a member of the community may not be credited. This capital may be cashed in to obtain **prestige**: an improvement in position within the scientific establishment, or formal recognition such as prizes, awards, grants, buildings, or commendations and honours conferred by officials. Lastly, prestige can be measured and compared by the scientist's rank within a home or equivalent institution – such as director, lead scientist of a laboratory or expedition, student, assistant, or outsider – as well as where the scientists ranks in the list of authors of a formal publication.

Figure 1. Exhange Schema for Scientific Collaboration.



For example, during their research on the causative agent of the bubonic plague, Lowson – then a junior medical official – provided the head of the Japanese research team Kitasato with goods in the form of research materials, laboratory resources, and his initial findings on the microbe. This created a reciprocal relation between the two scientists; even though Kitasato claimed sole authorship when this knowledge was exchanged with the general public, he repaid Lowson in the form of formal recognition from the Japanese government. Both scientists used this academic capital to obtain prestige: Kitasato was commended by an Imperial prince and given directorship of a research institute, Lowson selected as head of a plague commission to India[16].

A Brief History of Collaboration

According to our histories of science and social studies of the phenomena, from the inception of "Big Science" in the era prior to the Second World War to the present day, the incidence of scientific collaboration – in particular international scientific collaboration – is increasing. This is not a misapprehension or a distortion, but literally true: according to studies by Beaver and Rosen on scientific collaboration over the past three and a half centuries, the number of co-authored papers and the incidence of

collaborative work have been on the rise since the very first jointly-authored publication in 1665[17].

What Beaver and Rosen find fault with, however, is the recurring tendency of "historical blindness" in these works. Regardless of the publication date, scientific collaboration is presented as a modern phenomenon, its recency emphasized[18]. This emphasis imparts on the issue a sense of urgency, not unfounded: collaborative work now comprises not only the majority of scientific research conducted, but the research of highest impact as well[19]. Beaver and Rosen do not deny the importance of the phenomenon, but call for more historical perspective in studies of scientific collaboration; moreover, they reject the notion that the increase in incidence is due to increasing specialization among scientists, as specialization is a largely 20th century phenomenon and cannot account for the first two hundred years of collaborative work. Instead, they posit that the increase is due to the professionalization of science. They define professionalization as the ability to "define the rules, rights, and rites of access to the group, what holds the members of the group together, and what sets them apart from other individuals in the larger society"[20]. By this definition, in their view, the first region to have truly professional science was mid-19th century France. If their theory holds true, one would expect to see the majority of scientific collaboration in that era take place in France, with its occurrence in other nations increasing as the professionalization of their scientific community improved. Beaver and Rosen demonstrate that that was, in fact, the case.

They extend the same argument to the very first scientific collaborations in the 17th and 18th century, to explain why the most heavily represented field by far was astronomy[21]. The utility of astronomy, in navigation and calendar-making, had by this point been proven beyond a doubt to each state, and practitioners in the field acquired recognition, funding, and the ability to set standards. Moreover, Beaver and Rosen emphasize the close ties between 19th century French scientists and the government; when these ties, along with popular acclaim for their research, faded, French collaborative science suffered and the quantity and quality of British and German collaborative science improved as their state recognition and funding grew[22]. "Big Science" – that is, large institution-driven multi-national collaborations in science, mostly in physics – appeared as the result of a sharp increase in the social status of and funding for the sciences following the First World War[23]. A simpler explanation than professionalization or specialization seems evident: the increase in the incidence and quality of collaborative science follows from the increasing state recognition and support for the field. In his work, Bruno Latour has described extensively how modern nation-states rely heavily upon the knowledge production of the sciences for authority[24]. It is not surprising, then, that science has "modernized" to the extent that practitioners have become agents of the state.

Warren Hagstrom, in his comparison of "traditional" versus "modern" scientific collaboration, concurs. Modern collaboration is defined by bureaucratization, traditional collaboration by the free association of scientific peers and teachers and their pupils[25]. Modern bureaucratic collaboration involves the pooling of enormous resources, the

large-scale employment of non-scientist technicians, and the onus of the scientist to answer to a bureaucratic authority, either in government or in industry. Although Hagstrom targets large national nuclear physics collaborations as the worst offenders[26], modern studies of particle physics collaborations describe them as among the most free and democratic in the sciences; conversely, modern collaborations in biomedicine are hierarchical and factional – a structure that directly encourages inter-personal rivalry, inter-group competition, and priority disputes[27].

Biomedical Research in Tokugawa Japan: The Anatomy of a Scientific Revolution

It is a central assertion of this work that historical Japanese collaborations in biomedicine in many ways more closely reflect the modern form of scientific collaboration than the traditional. As such, a direct comparison to the modern mechanisms of negotiating these collaborations is not only relevant, but useful. To begin with, the line between state agent and scientist has been blurred in Japan since the first modern scientific texts arrived from Europe in the early Tokugawa period (1603-1868): these texts were primarily accessible to the samurai class, the administrators of the Tokugawa government[28]. Warriors in an era of unprecedented peace and stability, the samurai were encouraged to take up scholarship in addition of martial disciplines, and many of them did[29]. State oversight and the obligation of the scholar to answer to these authorities were absolute; failure to do otherwise could result in imprisonment. The historical period wherein science was dominated by the efforts of wealthy amateurs guided by their own curiosity never truly existed in Japan[30], and the bureaucratic nature

of their investigations, the stratification of student-teacher/state-subject relationships, and the high level of factionalization between schools of thought better resembles the modern biomedical collaborations described by Knorr-Cetina than the free associations of traditional collaboration described by Hagstrom.

A secondary assertion is that the state of biomedical research in Japan in the late 19th and early 20th century, while not comparable to major producers of scientific research such as Germany, France, and Britain, was at a sufficiently advanced state of development to be directly comparable to the minor European nations of the same era. One of the most common conventional narratives with regard to the history of science in Japan is the so-called "Butterflies and Frigates" view described by Morris Fraser Low: that science in Japan began when Commodore Perry arrived on its shores to forcibly open Japan to the West, and taught the local inhabitants the need for modern technology[31]. On the contrary, a thorough understanding of the power of modern technology, particularly its military applications, led to a ban on foreign scientific materials in chemistry and physics by the Japanese government of the Tokugawa period[32]. While fields such as astronomy, medicine, and mathematics largely escaped censure in comparison, Japanese scientific thought was sufficiently advanced to make the connection between basic research in movement, energetics, and chemicals to weaponization. The conventional narrative fails to take into account the hundreds of scientific texts imported and translated during the era prior to Perry's arrival: by 1848, 71 texts had been translated and circulated in the medical sciences alone[33]. It also fails to take into account the ready and willing class of intelligentsia pursuing these studies:

samurai comprised a full 6% of the population[34], and, as mentioned, were encouraged to take up scholastic pursuits. Justification of state authority through Confucian rationalism arose in a timeline roughly comparable to the European Enlightenment, discarding full reliance on myth structures and military power and appealing to the need for an orderly society[35] - in other words, a need was established in the same era in Japan for knowledge production. Traditional studies, too, such as Confucianism, Chinese astronomy, and traditional medicine were not so wholly dissimilar that they did not foster curiosity and analytical thinking beneficial to the pursuit of science: there existed a strong tendency for sons with fathers in these traditional professions to pursue careers in science after the Meiji Restoration(1868)[36].

In biomedicine, however, the foundation for Japanese scientific thought relied upon far more than a foundation in rationalism, objective curiosity, and a reading of translated texts. Its relative lack of potential for weaponization, unlike chemistry or physics, left it open for two and a half centuries as a line of free inquiry; in fact, during periods wherein all other Western studies were officially banned, the whole of Japanese scientific inquiry focused on biomedical research[37]. Original research was conducted in the fields of anatomy and botany, and subsequently published, distributed, and debated[38]. A government-sponsored testing facility to determine the efficacy of imported medicines was established in 1722[39]; several schools of Western medicine, with Dutch doctors as faculty members, were created in the 1750s[40], a full century before Perry arrived. Contrary to depictions of Japan as a vessel for absorbing Chinese, then Western, knowledge, this knowledge was critically examined and tested, the more

accurate knowledge absorbed – e.g. Western anatomy as compared to tradition Chinese descriptions of bodily vessels – and the less accurate discarded. Early Japanese anatomic researchers paid little heed to traditional prohibitions against defiling human bodies, and performed their own dissections to confirm what they had read[41]. In the view of some historians, such as Nakayama Shigeru, the adoption of Western anatomy in Japan constitutes a true scientific revolution, or Kuhnian paradigm shift[42]. Unlike Europe there was little early investment in the state's or religious governing body's descriptions of heavenly bodies as accurate, so Western astronomy was absorbed with comparatively little impact, but the adoption of Western anatomy required the wholesale abandonment of the Chinese view of the body as vessel for energy conduits and illnesses as a result of imbalances in *ch'i*. Even amongst those historians who caution against overstating the importance of early Japanese scientific experimentation and publication, and reject the notion of a true scientific revolution before the Meiji era, there is some agreement that biomedicine was the exception[43]. Finally, medicine as a discipline was by far the most open and competitive: non-samurai by birth could still study it, travel to attend medical schools, and expect meritocratic promotions to prestigious positions[44]. This meant that Western medical concepts could be disseminated throughout the nation at a much greater rate and on a larger scale than those fields that relied upon texts restricted to government officials. It is only in this context – that of a centuries' old biomedical establishment – that we can shed light the accomplishments of one bacteriologist Kitasato Shibasaburo, without resorting to historical fallacy.

III: Biography of Kitasato

Unlike many of the great microbiologists of his day – such as Koch, Pasteur, Yersin, and Behring – Kitasato requires introduction. His origins will be familiar to many scientists of the modern age: born well before the Meiji Restoration of 1868 when the feudal period of Japan ended and the island nation became a modern state, he was not one of the famous Meiji elite; instead, his parents were comfortably middle-class. While sending their son to higher education was not an undue burden, Kitasato worked a variety of part-time jobs to support himself while he pursued his doctorate – this was no silver-spoon genius[45]. He attended Kumamoto Medical Academy. While there his instructor, the Dutchman C.G. von Mansveldt, took note of his aptitude for science and encouraged him to apply to Tokyo Imperial University for post-graduate studies. In 1885 he was selected from amongst the promising students of his faculty for a prestigious internship under the German bacteriologist Robert Koch to study his then-controversial Germ Theory of Disease. There, under Koch's mentorship, and in collaboration with Emil von Behring, he made an epoch-making immunological discovery in 1890: antibodies in the blood which not only reacted to pathogens but could be used to counteract them. In 1894 he responded to the British Empire's call for assistance with the outbreak of the bubonic plague in Hong Kong and had within six days isolated – in collaboration with the English medical doctor James Lowson and in direct competition with the French

microbiologist Alexandre Yersin – *Yersina pestis*, the pathogen responsible for the plague[46].

Eschewing offers of tenure at European and North American institutions, he returned to Japan in 1891 and founded the Institute for the Study of Infectious Diseases. The Institute produced effective vaccines, serums for general public health in addition to hosting leading bacteriologists from across the globe. When the Institute was incorporated into Tokyo Imperial University by the government in 1914 in order to check Kitasato's influence – whose ideals of open, international science and the preeminence of public health research ran counter to increasingly totalitarian and isolationist policy – he founded the Kitasato Institute on his own coin, and that of his private supporters. The Kitasato Institute, to his opponents' chagrin continued to attract considerable talent in the field and enjoyed widespread support among the general public.

Kitasato's legacy impacts us today. He continued his work on anti-serums as well as his mentor Koch's work on tuberculosis; he traveled to China again in 1911 during another plague outbreak to help contain it and treat infected patients; through his persistent friendships he precipitated collaborations between his own students such as Shiga Kiyoshi and Hata Sahachirō with European scientists such as Behring and Paul Ehrlich, resulting in the discovery of and treatments for dysentery, syphilis, and autoimmune diseases. He founded what would become the Terumo Corporation – which continues medical research throughout the Asia-Pacific region to this day – in

order to fund his private institute, now Kitasato University. Yet, outside of his native Japan, Kitasato remains relegated to the footnotes of history.

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Chapter 2 – Literature Review

Introduction

A review of the literature published to date pertaining to Kitasato provides us with a number of observations, from which conclusions can be drawn about the historiography of collaborative science, science in Japan, and science in general. The first thing to notice is how perceptions of the same material change over time, by politics and by era. While it may be obvious how materials controversial when published become less so to future generations, the reverse is also true, in ways that are not always readily predictable: stripped of all context, the same words read differently. Secondly, to uncover the ways in which laudatory works intended to sing the praises of a particular scientist, or to single him out as an individual to criticize him independently of his affiliations, can still work to construct a narrative critical of his home establishment: as we shall see, many of these works refer to Kitasato as an eminent figure in his field – a "genius"[47] and a "giant"[48] – yet solidify the perception of the Japanese scientific establishment as insular and weak. Thirdly, to observe the roles played by historians of science and scientist-historians, respectively, in creating their parts of this historical narrative. Finally, the striking role played by language – when that language is not necessarily the language of that scientist's publication nor that of his home country – in how scientists and their accomplishments are perceived.

The distinction between historian of science and scientist-historian requires some comment. Historians of science are, first and foremost, historians. While they may – and often do – have some training in the sciences, it is not part of their profession to keep abreast of the latest developments in a given field or the technical minutiae of how experiments are performed. They may be employed by academic institutions or as journalists. Scientist-historians are, conversely, professional scientists or medical doctors engaged in research who have an interest in history and have been provided with the resources to investigate the history of a scientist or a discovery, usually one affiliated with the institution that employs them. While scientist-historians generally outstrip their historian peers in terms of funding and access to resources, this comes at a cost: they are close to the subject matter, personally and professionally, which leads most to avoid analysis – let alone criticism – and simply document what was done by when and by whom[49]. But historians of science are not immune to this problem either: working too closely with scientists in order to use their access and expertise often results in, as Japanese historian of science Nakayama Shigeru puts it, "triumphal, commemorative essays, or deluxe volumes of collected reminiscences"[50]. This proximity results in another problem as well, as we will soon discover – a form of historical territorialism. But for now it is important to bear in mind the distinction between these two groups.

The Influence of Language

To begin this section with one of the original assertions of this thesis: in spite of his accomplishments, as noted, there remains a paucity of English-language materials on the subject of Kitasato and his pupils, particularly when compared to Behring, Ehrlich, or Koch, with their numerous biographies. As one reference collection of historical works on microbiology puts it, with more than twice the number of materials on Behring than Kitasato, "much more is needed"[51]. When Kitasato – or his students – do appear, they tend to appear either as footnotes in histories of microbiology or as part of a supporting cast for the discoveries of a European scientist, their contributions relegated to the strictly technical. In Paul de Kruif's *Microbe Hunters*, Kitasato is never mentioned as having made any specific contribution to Behring's work on natural immunity at all, while Shiga's work on syphilis with Ehrlich is as a technician Ehrlich acquired to do repetitive work, as Ehrlich "got himself a Japanese doctor... to do the job, in short, that it takes the industry and patience of the Japanese to do"[52]. While Isabel Plesset goes into great detail about the accomplishments of Noguchi at the Rockefeller Institute, she describes Kitasato as Behring's "assistant" (Behring was Koch's assistant) and does not mention Hata's contribution to Ehrlich's discoveries at all[53].

One could attempt to explain this phenomenon through the assertion that while Kitasato and his students' contributions to the medical sciences were noteworthy, they were not *as* noteworthy as those of their European peers. This explanation does not hold in light of the fact that an inspection of general histories of microbiology reference

Kitasato increasingly as the presumed technical knowledge of the subject matter of their audience increases. For example, William Bulloch's general *History of Bacteriology* contains only a single reference to one of Kitasato's works and to six of Behring's[54], while Patrick Collard's *The Development of Microbiology*, intended as a text for aspiring microbiologists, contains as many references to Kitasato's work as Behring's[55]. Additionally, several historians from within Japan and without allege that Kitasato should have shared in Behring's Nobel Prize for natural immunology and serum therapy[56], and even Derek Linton – biographer of Behring and staunchly opposed to the idea – admits there is a great deal of evidence to the claim that, had they conducted their experiments in today's collaborative climate, a shared prize would have likely been awarded[57]. Even more striking is the number of scientific reviews on immunology, often from scientists of German origin, which mention Kitasato in passing, and for what: his "epoch-making"[58] discovery, which served as "the foundation of the science of immunology"[59], and who went on to "almost single-handedly build a research tradition in investigative medical research"[60]. In other words, while Kitasato may have been neglected by historians of science, he is a household name amongst his scientist-historian peers. This is a notable disparity, given the aforementioned proximity of the two – one would think one of the founders of a scientific discipline would incite some curiosity; so why hasn't it? Ultimately, their scientific accomplishments are indeed comparable, and another explanation is required.

One of the few historians of science to write on Kitasato directly is James Bartholomew, to whose research this thesis is greatly indebted. His subject matter is,

however, not Kitasato as a scientist, his accomplishments, discoveries, and collaborations, but rather his contributions as an administrator and political figure to the building of a scientific establishment in Japan. His contributions to the Japanese research tradition as director of the Institute for Infectious Diseases and later Dean of Medicine at Keio University were considerable, and Bartholomew discusses them at length. Kitasato's belief that matters of public health were best left to clinicians rather than bureaucrats, his ability to persuade both elected officials and the general public, and his conviction that bacteriology was an area of clinical and applied, rather than basic, research made him powerful enemies – and ultimately resulted in having his Institute stripped from him by officials who believed that academics had no business wielding political power. How and why this occurred is found in detail in Bartholomew's *The Formation of Science in Japan: Building a Research Tradition*, and so will only be touched on briefly and as necessary here[61]. But Kitasato as a scientist was surely as influential as Kitasato as an administrator; the lack of material on the former is curious, and revealing.

The bulk of the dedicated subject matter to be found in English on Kitasato outside of general histories of microbiology gives evidence of the most likely culprit: language. While it pales in comparison to his discovery of natural immunity in terms of scientific importance, his discovery of the plague bacillus took place in the British colony of Hong Kong, and was originally published in English – as were much of the secondary materials and many of the priority disputes, and thus easily accessible to English-speaking historians. Even these, however, are not dedicated to Kitasato, per se, but

rather “the plague controversy”, wherein Kitasato plays foil to the French scientist and co-discoverer Alexandre Yersin (1863-1943). The controversy can be summarized as follows: although Kitasato published a description of the plague bacillus several days ahead of Yersin, Yersin’s paper gave a more finely detailed description of the organism. Some historians have alleged that Kitasato’s description was a result of contamination with pneumococcus[62,63]; several reviews by microbiologists disagree, pointing out the definitive characteristics of the bacillus described by Kitasato and that in some ways his work was more thorough than Yersin’s own[64].

A Century of Scientific Controversy

It is instructive to provide a timeline of the controversy, as it might have been observed by an English-speaker, as it progressed. A few months before the start of the First Sino-Japanese War, in the summer of 1894, an epidemic of the bubonic plague broke out in the British colony of Hong Kong. A call went out to the scientists of all other friendly nations to discover the cause and cure: one Professor Kitasato Shibasaburo, the first foreigner to be awarded that title at the University of Berlin, famous for his discovery of tetanus antiserum, responded. According to some reports a French scientist had arrived as well to study the disease; nevertheless, shortly after his arrival, the Lancet published that they had been informed that Kitasato had discovered the cause[65], followed by an article later that summer describing the organism and its pathology[66]. The French scientist, Yersin – mistaken by some articles for Albert Calmette[67], director

of the Pasteur Institute at Saigon, mistaken by others as a Japanese[68] – published in French[69], with no differences sufficiently striking to warrant comment in international scientific circles. Yersin sent his samples to the Pasteur Institute in Paris, Kitasato his to the Institute for Infectious Diseases in Berlin.

If our hypothetical English-speakers could read German, as one very well might if they possessed an interest in clinical science at that time – prior to the Second World War German was the lingua franca of medical science, as English is now – in 1895 they might have read a letter to a German medical journal by Aoyama Tanemichi, one of the Japanese scientists who had accompanied Kitasato to Hong Kong, but while there was stricken with the plague[70]. In it Aoyama claimed that Kitasato had said in a lecture that his plague bacillus was different from the one discovered by Yersin. In response, Hugh Zettnow retrieved Kitasato's samples and compared them to samples he had received from Élie Metchnikoff in Paris, and published an article in early 1896 that included photomicrographs of the bacteria, and his findings: that they were most likely the same organism[71].

Yet the following year, in 1897, an article by Ogata Masanori, published in both English and German, again claimed that Kitasato had given a lecture on the plague and stated that the bacterium he had discovered was morphologically distinct from Yersin's own[72]. Ogata listed the "distinctive" characteristics of Kitasato's bacillus, although he had not, he admitted, seen it himself. Furthermore, during his researches on the plague in Taiwan, the bacterium he isolated resembled Yersin's description, not Kitasato's. Our

German-reading English-speaker would recall that this was the same Ogata who had written to another German medical journal to say that he had anticipated Behring and Kitasato in the discovery of antisera and that they had merely copied him – garnering no response from Kitasato and a terse, flat denial from Behring, who pointed out that Ogata's method, the one they had supposedly "copied", did not work[73]. Later that year Wilhelm Kolle tried to lay the matter to rest by comparing samples of what was then known as *Pasteurella pestis* from across the globe, including Kitasato's and Yersin's from Hong Kong, and determined that they were all the same species of bacterium[74].

Three years later in 1900 another article by a Japanese naval surgeon, Yabe Tatsusaburō, was published in French[75]. This was different from Aoyama and Ogata in that he claimed to have seen Kitasato's bacillus firsthand in his own Tokyo laboratory; however, like them, he found it to be different from Yersin's. Instead it was, in his view, a *Streptococcus* – a common secondary infection of the plague. Even so he noted that, oddly, a pure culture of this bacillus still caused the bubonic plague in test subjects. Our English-speaker would be understandably confused as to how a Streptococcus could cause the plague – in any case, that same year Kitasato himself would publish again on the matter, again in English, and, for the time being, put an end to it: his investigations during the 1899 plague epidemic in Kobe had "proved no doubt that the bacillus of the bubonic plague was identical with the Yersin bacillus"[76].

The following year marked the beginning of the Anglo-Japanese Alliance, a close relationship between the two Empires that would last until 1923. During this time

Kitasato presented papers on the plague at conferences in Lisbon (1906), Budapest (1909), and London (1913); he presided over the International Plague Conference in Mukden in 1911. At these *Pasteurella pestis* was referred to as the "Yersin-Kitasato bacillus" and no controversy arose in discussions of its characteristics or discovery for some three decades, until in 1926 a student of Yersin's, Emile Lagrange, published an article in English based on the publications by Aoyama and Ogata, which drew the conclusion that Yersin was the sole discoverer[77]. In response, a British doctor, Milliot Severn, wrote a letter to Kitasato to ask him to clarify: he responded, through his own student Miyajima Mikinosuke, while he had originally observed some differences between his bacillus and Yersin's, "further studies on these two discoveries proved that they belonged to the same species"[78]. Severn published this response in the English *Journal of Tropical Medicine and Hygiene* in 1927. When Kitasato died in 1931, nearly every obituary published at that time credited him with the co-discovery of the cause of the bubonic plague[79].

When the drama of the controversy resurfaced upon Yersin's death over a decade later, not only were the players in the first act gone, but the world had changed drastically. The Second World War was building to a climax in 1943, and this time Japan was not an ally, but a foe. When another of Yersin's peers gave his colleague sole credit for the discovery of *Bacillus pestis*, discrediting Kitasato, he found a much more receptive audience than had Lagrange[80]. Over the decades that followed the perception of Japanese science would change utterly, particularly of its history. A new narrative emerged during the Occupation years: Japanese science had been ineffectual,

backwards, and under the sway of a totalitarian state until the Americans arrived to modernize, democratize, and enlighten it. Thus, this claim went unchallenged until well after the war and even the American Occupation of Japan. The publication of Fabian Hirst's *The Conquest of the Plague* in 1953 was the most thorough account of the history of the disease, its discovery, and treatment to date[81]. In it he discussed Kitasato's original paper as well as the differences noted by Ogata, and came to the conclusion that, based on the examinations by "skilled European technicians"[82] – presumably Zettnow and Kolle – finding them identical, they both deserved credit for this discovery, and that any discrepancies were the result of Yersin being "a better technician"[83]. The book touched off a small debate in the *British Medical Journal*, which saw Ogata's son Norio decry this conclusion[84], and James A. F. Lowson – the son of the same James Lowson who had worked with Kitasato in Hong Kong – defend it[85].

Once more, the matter seemed to have been settled; but the two decades that passed before another historian would comment on the issue were but a lengthy intermission before the next act, this time on a new stage. By now eighty years had passed since the discovery, and even the youngest of the original observers would have been long gone. By 1973 the world was in the grip of the first Oil Crisis, and anti-Japanism, particularly in depths of the American recessions, was on the rise. American historian of science Norman Howard-Jones published a scathing review of the plague controversy[86], condemning Hirst's findings as "wishful thinking"[87] and accusing Kitasato of making a "circuitous face-shaving admission of error"[88] following the Kobe epidemic. He had found, in a rare American medical encyclopedia published in 1898 (though for

which submissions were collected in 1895), an entry by Kitasato in which he stated outright that, based on his observations, his bacillus was not the same as Yersin's, and urged the scientific community to come to a conclusion on the matter[89]. Howard-Jones held this up alongside all of the publications he could find by Japanese scientists in English, German, and French – in other words Yabe, Ogata, and Aoyama – as proof that Kitasato did not discover *Pasteurella pestis*, but rather a contaminating *Streptococcus*.

Prior to this the only other major work in English on Kitasato was Bartholomew's doctoral dissertation[90], but in the years between this and Howard-Jones' next publication on the matter, he would publish articles detailing the history of the Japanese medical science establishment and its factional and political rivalries[91]. On the one hand were Kitasato and his colleagues at the Institute of Infectious Diseases; on the other, the faculty of medicine at Tokyo Imperial University, including Ogata, Aoyama, and Yabe. Bartholomew contributed the origin of the schism – Kitasato had, on scientific grounds and at Koch's urging, criticized Ogata's assertion that he had found the microbial cause of beriberi (now known to be a result of vitamin B12 deficiency). This had caused a rift between the two faculties so deep that members of the university faction had created their own journal purely to publish allegations of misconduct and scientific failings on the part of the Kitasato faction – allegations which, when pressed, they revealed they knew to be untrue[92]. In light of this new information, Howard-Jones' second publication in 1975 hedged away from the claim that Japanese scientists were unanimous[93] in their disagreement with Kitasato, to the more diplomatic "virtually unanimous"[94]. It concludes with the ominous postscript that two British scientists had written to inform

him that, based on his 1894 publication, Kitasato had indeed described what are now known to be the characteristics of the plague pathogen. This did not stop Howard-Jones, however, from declaring the idea that Kitasato should share credit with Yersin for his 1894 discovery a "myth"[95].

In 1976 two microbiologists wrote their own paper in response: David Bibel and T.H. Chen of Stanford published in *Bacteriological Reviews* what is still the most thorough scientific analysis of the plague controversy to date[96]. The paper, lengthy and highly technical, was clearly intended for a scientific audience, though their conclusions are stated in plain language: the morphological and clinical characteristics described in the 1894 were distinctly those of the newly renamed *Yersinia pestis*, not *Streptococcus*, and that their analysis left "little doubt that he did isolate, study, and reasonably characterize the plague bacillus. Kitasato should not be denied this credit."[97] Though much more diplomatic in tone, it was clearly written to challenge Howard-Jones, as it responds to his incredulity that Kitasato could make errors that took at least a year to correct on what were to his knowledge simple identification techniques with a gentle reminder that the state of microbiology in the 1970s was not the state of microbiology in the 1890s[98].

No one has risen up to challenge Bibel and Chen's findings on scientific grounds in the years intervening, and our hypothetical English speaker – now a very different person than the original German-reading doctor who might have picked up the Lancet in 1894 – might have assumed that after one last furor the matter was at last concluded.

Indeed, a survey of the English-language materials published in the 1980s and early 1990s, such as Thomas Butler's *Plague and Other Yersinia Infections* in 1983[99] and Andrew Cunningham's chapter on the plague discovery in Hong Kong in *The Laboratory Revolution in Medicine* in 1992[100], shows that while they do discuss the controversy, they credit both Yersin and Kitasato as Bibel and Chen suggest.

That would all change in 1993. The newly-constructed Hong Kong Museum of Medical Sciences put out a call that year for contributions to its opening exhibition and it received a journal written by a British naval surgeon during the year of the plague epidemic, from the granddaughter of one James Lowson. Though it contained no new scientific information to add to the debate, it was rich in historical detail as to how the epidemic started, how the quarantine was carried out, and how the research progressed. These details were analyzed in publications by Dean of Medicine in Hong Kong, Gerald Choa[101], and by William Yule in the *Scottish Medical Journal*[102]; strikingly, neither of them cite Bibel and Chen, only Howard-Jones, and both accept his conclusions as the scientific consensus. British scientist-historian Tom Solomon would take this one step further in 1995 and 1997[103]; placing the "blame" on Lowson for urging Kitasato to "rush into publication"[104], which may have "cost him credit for the discovery[105]". Solomon does cite Bibel and Chen's paper – but as an endnote for a statement that states precisely the opposite of the conclusions to which they came regarding the issue, along with other perplexing citations, such as listing Severn and Butler as part of the "historical consensus" that Yersin, not Kitasato, deserved credit for the discovery[106].

Finally, in 2002, journalist-historian Edward Marriott would use Lowson's diary as the springboard for a semi-fictional documentary account of the 1894 plague epidemic in Hong Kong, *Plague: A Story of Science, Rivalry and the Scourge That Won't Go Away*, which received wide circulation[107]. Though he too includes Bibel and Chen – and even Bartholomew – in his list of references, the influence of their findings is nowhere to be found within; again, Howard-Jones is cited as the conclusive work on scientific matters[108], though Marriott adds to the list of accusations of deceit on Kitasato's part spying and bribery as well[109]. In 2012, should our hypothetical English-

Table 1. Timeline of the Plague Controversy.

Year	Event	Language of Publication
1894	Kitasato Shibasaburo and Alexandre Yersin publish their independent discoveries of *Bacillus pestis*	English; French
1895	Letter written by Aoyama Tanemichi of Tokyo University questioning Kitasato's claim	German
1896	Hugh Zettnow examines photomicrographs of both organisms, publishes results	German
1897	Ogata Masanori of Tokyo University: Kitasato's organism is "morphologically distinct", Yersin's found during epidemic	English; German
1897	Wilhem Kolle compares samples from outbreaks internationally, including Yersin and Kitasato's, concludes they are the same organism	German
1898	Medical encyclopedia containing a 1895 article by Kitasato suggesting his organism, KB, differs from Yersin's, YB, is published	English
1900	Yabe Tatsusaburō: "direct observation", concludes Kitasato's organism is *Streptoccus* , yet causes plague symptoms	French
1900	Sata Aihiko compares samples of Pasteurella pestis from a number of researchers, including Kitasato and Yersin, concurs with Kolle	Japanese
1900	Kitasato concludes, based on his investigations of the Kobe plague epidemic in 1899, that his and Yersin's organisms are "identical"	English; German
1926	Emil Lagrange, a student of Yersin's, asserts that based on Yabe, Aoyama, and Ogata's observations, Yersin deserves credit as sole discoverer	English; French
1927	Milliot Severn writes to Kitasato asking for clarification; receives response from Miyajima Mikinosuke that the organisms are the same	English
1931	Kitasato's death; universally credited with discovery	English; German; Japanese
1943	Yersin's death; also credited with discovery - obituary names him sole discoverer	English; French
1953	Fabian Hirst concludes that Yersin and Kitasato are co-discoverers; Yersin is "the better technician"	English
1954	James A. F. Lowson, James Lowson's son, asserts that Kitasato made the discovery first, cites Lowson's diary	English
1955	Ogata Norio, Ogata Masanori's son, asserts that Kitasato did not discover the same organism, cites his father's article	German
1959	Takano Rokurō publishes a biography of Kitasato, discusses controversy, cites Kolle's conclusion that both deserve credit	Japanese
1973	Norman Howard-Jones asserts that Kitasato did not discover (now) Pasteurella pestis, based on Ogata, Aoyama, and Yabe's articles, Kitasato's "KB"	English
1975	Howard-Jones reaffirms his position despite letters from microbiologists suggesting the contrary	English
1976	David Bibel and T.H. Chen examine Yersin and Kitasato's original findings - both *Pasteurella pestis* ; Kitasato's potentially contaminated after 1895	English
1989	Nagaki Daizō writes a biography of Kitasato and his students, cites Kolle and Sata, gives credit to both Yersin and Kitasato	Japanese
1993	Copy of Lowson's diary given to Hong Kong Museum of Medical Sciences	English
1993	Gerald Choa examines the diary, gives Yersin sole credit for the discovery	English
1995	William Yule comments on Lowson's role in the plague discovery, gives Yersin sole credit	English
1995	Nakase Yasukiyo writes an article commemorating the "controversy", credits both Kitasato and Yersin	Japanese
1997	Tom Solomon blames Lowson for Kitasato's "rush to publication", gives Yersin sole credit	English
2002	Edward Marriott writes a historical novel recounting the plague discovery, sharply critical of Kitasato and Lowson, gives Yersin sole credit	English
2003	Nakase writes an article on Kitasato's discoveries and inventions, gives credit to Kitasato and Yersin	Japanese

speakers choose to investigate the plague controversy the way they now almost certainly would, they would find Marriott and Solomon's works at the top of the results of their search engine.

Constructing a Narrative through Controversy: Part 1, Historians of Science

Examining a few of the most influential pieces of the controversy in detail reveals a number of common themes: omitting or outright misstating the research in which Japanese scientists were involved, and characterizing the Japanese scientific establishment as homogenous – presenting a unified front with no dissenting opinions or academic rivalries – and uninfluential.

To begin with, in Howard-Jones original 1973 submission we find both. As alluded to above, the article is a polemic: in it, the arguments of the opposing side are "ludicrous"[110]; the evidence they offer "noninformation"[111]; in 1975 the explanations they offer a "travesty of the truth"[112]; and they themselves are unthinking "believers – the strength of whose convictions would appear to be matched only by lack of familiarity with the relevant literature"[113]; he concludes with a dramatic poem about the fragility of truth[114]. Even so, he is careful to credit Kitasato as "one of the great pioneers of medical bacteriology"[115] and "one of the most experienced and skilled bacteriologists of his time"[116] – doing so, of course, serves the purposes of his argument – yet he never mentions why Kitasato was one of such an esteemed group, or what he discovered to join it. He mentions nothing about Kitasato at all outside of the plague controversy. This

seems inconsequential in and of itself as this was the topic at hand, and perhaps forty years ago these accomplishments had not faded as much from public memory, but it was as we shall see the first instance of many and varied retreads of the same theme.

The most egregious example of the aforementioned tropes to be found in Howard-Jones' original paper was his assumption of homogeneity on the part of all – not many, or most – but literally "all" Japanese scientists on the issue of Kitasato and the plague controversy[117]. (And in doing so overlooked numerous publications by members of the Institute for Infectious Diseases; later, Bartholomew's work may have brought these to his attention.) While he openly states he is using only those sources from Japanese authors that are available in English, French, and German[118] – this is clear enough from his list of references – and makes no attempt to investigate any of their claims in the original Japanese, he nevertheless paradoxically claims that these writings had been overlooked because they were written by Japanese authors. Why would this have been so if Kitasato, one of the "giants of microbiology" was himself Japanese? Ogata, too, was by this point a German-trained, world-renowned microbiologist published in a number of languages – his lasting contribution to plague research would be as the first to identify the role of fleas as the disease vector, while all others, Kitasato and Yersin included, thought it to be rats. If Ogata's writings were overlooked for any personal reason, it seems more likely to have been because he had made unfounded claims about Kitasato's credibility before, published and readily available in German. More importantly, his and Aoyama's second and third-hand "observations" directly contradicted the results of the studies published by Zettnow and Kolle on the organism

itself[119]. Moreover, by publishing these lectures on the plague in Japanese only for several years before his conclusion in German in 1899 that the organisms were "identical", Kitasato (Howard-Jones insinuates) was deliberately concealing his work; his Japanese peers, being able to read the language, would have "known better". What he does not mention is that the 1899 paper was a colossal work, the culmination of years of research on plague outbreaks, containment, and treatment, and was the most thorough of its kind up to that point, on which he was invited to speak at numerous conferences internationally[120]. Did Kitasato have an intellectual obligation to publish his preliminary results in a non-Japanese language before then? And was failing to do so inherently deceptive, as Howard-Jones implies?

This is not the only accusation of duplicity on Kitasato's part levelled by Howard-Jones for failing to adequately employ a European language: Kitasato's response to Severn was through his pupil Miyajima, as, Severn notes, Kitasato had "only the slightest acquaintance with the English language"[121]. To which Howard-Jones responds incredulously that Kitasato had published in English before, including his preliminary communication to the Lancet in 1894[122]. But this communication was originally in German, and translated into English by James Lowson[123]. The papers in English on the plague that followed were written by his students – Kitasato merely signed off on the results. And at the Institute he employed an interpreter, Noguchi, to communicate with English-speaking guests[124]. To all evidence Kitasato's grasp of English was imperfect in Hong Kong, and that was thirty years of disuse prior to his communication with Severn. Miyajima, on the other hand, would publish frequently and fluently in that language,

including novel-length memoirs[125]. Rather than employing his student as proxy to deceive his European peers, as Howard-Jones implies, Miyajima seems by far the more logical choice to ensure that they did not mistake his meaning.

Furthermore, in this paper Howard-Jones himself demonstrates an adequate understanding of how language can be employed to make questionable claims unquestioned. While German was the dominant language of medical science in Kitasato's time, it was not in Howard-Jones'. His audience was primarily English-speaking, meaning his quotes and paraphrased sections of German text are all in translation. Though he savages Hirst for failing to reveal the identity of the "anonymous, spaceless, and timeless technicians"[126] who had determined that Kitasato's bacterium was identical to Yersin's, Hirst did come to his own defence in the *British Medical Journal* debate of 1953 and provide his source[127]. This was English W.J. Simpson's *A Treatise on the Plague*[128]. G.S. Wilson, another Kitasato defender in the *British Medical Journal*, cites German scientist R. Otto[129], whose own source was, in turn, according to Howard-Jones, "just another of the many unsubstantiated ex cathedra statements asserting the identity of the two organisms"[130]. This source is never mentioned by name: it turns out to be Kolle, and August von Wasserman – a German scientist studying the plague at Kitasato's Institute for Infectious Diseases during the time of the controversy[131]. From these names it is then trivial to track down Kolle's original study of the two organisms, and discover the observations he had referenced. To refer to Kolle citing his own scientific work as an "unsubstantiated ex cathedra statement" seems incredible, as Howard-Jones does not shy away from citing himself, either[132].

Howard-Jones does cite Kolle and Zettnow's studies on the various strains of the plague, near the beginning of the paper. On Zettnow he writes that "he worked with a strain provided by Metchnikov, but mentioned toward the end of his paper that he also received a strain from Kitasato" and that "he did not specify whether or not there were differences between them"[133]. This is not an entirely accurate summary of Zettnow's findings. Firstly, Zettnow writes in the opening paragraph that he had received two strains: one from Metchnikov in Paris, and one from Koch in Berlin[134]. Shortly afterward he makes explicit that these are Yersin and Kitasato's strains, respectively. He observes that he did not find any major differences between them, based on photomicrographs taken of the bacilli, and many similarities[135]. Of Kolle's paper Howard-Jones writes that the results of his study were that "the organisms from all sources were the same"; but omits Kitasato's sample from this list, substituting "strains from Hong Kong (source not otherwise stated)"[136]. But Kolle does cite his source for this strain earlier in the paper: Kitasato, and Berlin.

Howard-Jones employs the language of his presumed audience to attack Kitasato's character in a similar fashion. In his 1899 paper, he paraphrases Kitasato's "only reference to Yersin" as being that "his antiplague serum was found to be therapeutically and prophylactically useless" – while at the same time others had employed the serum successfully[137]. Oddly, an examination of the rare medical encyclopedia published in 1898 that served as the impetus for Howard-Jones article is much more diplomatic on the subject, saying: "Yersin has also prepared a curative serum from his plague bacilli, and this serum has enjoyed extensive trial, particularly in

Bombay. It appears that in some cases the results have been very encouraging, while other statistics again do not give so favorable a showing. The question of the efficacy of this serum therefore awaits further researches."[138] Why would Kitasato's opinion of the antiserum and willingness to cite Yersin have changed so drastically in one year? It turns out that this too is a misleading summary of the text of the original: Kitasato does not discuss the discovery or morphology of the organism at all in this paper, and in the context of treatment, which he does discuss, he references Yersin several times. Of his antiserum he publishes his findings: that in animal testing Yersin's antiserum performed "exactly the same as our own, sometimes to greater effect, sometimes less", but that when it had been employed in human cases that had reached the pneumonic stage of the disease (that is, spread to the lungs from the bubos – severe swelling in the lymph nodes characteristic of the Black Plague) it had not improved the outcome of the patients, and all cases were still fatal – as his own efforts had been[139]. He would later experience some success with Yersin's antiserum, and attribute his earlier results with a lack of supply[140]. This is hardly the dismissive condemnation or glaring omission Howard-Jones, and later Marriott, imply.

These and other reproductions would be omitted from Howard-Jones' follow-up paper in 1975. References to Hirst, Kolle, and Zettnow are removed; Kitasato's defenders are represented solely by Severn and Miyajima and their personal correspondence. Still, their statements are hearsay and "'history'"[141]; Lagrange's review of Ogata and Aoyama's second- and third-hand information is "a scholarly contribution showing a thorough knowledge of the relevant literature"[142]. As mentioned, by now

scholarly articles based on works translated from the Japanese, such as Bartholomew's, were becoming available, causing Howard-Jones to retreat slightly from his position of speaking for "all" Japanese microbiologists to almost all. But this time not only does the Japanese scientific establishment come under attack for homogeneity and ineffectuality – Howard-Jones points out that Kitasato had come to be acknowledged as the discoverer of the plague bacillus in Japan as well even though they knew better: a failure on the part of Japanese historians of science[143]. And indeed they did know better, though not in the way he meant to imply: Japanese historians such as Rokurō Takano and scientist-historians such as Fujino Tsunesaburō knew about the factional rivalry of the time, had access to the lectures they quoted, and disagreed with him on evidentiary grounds – not because "error had proved to be invincible"[144], as Howard-Jones would have it. This would be the last Howard-Jones wrote on the subject.

Part 2: Scientist-historians Respond

Scientist-historians Bibel and Chen allude only briefly to the historical controversy; the main focus of their highly technical and specialist-oriented paper is the scientific details, not correspondence or conjecture[145]. They begin by "finding" Hirst's sources, Kolle and Zettnow, and describing the results of these papers accurately, in detail[146]. Using Kitasato's 1894 publication to the Lancet as a primary source, they come to the conclusion that the "similarities far outweighed the minor variation in observations"[147]; moreover, that "Kitasato's paper, being longer and more detailed,

presented a greater opportunity for finding fault"[148]. The major discrepancy between the morphology of *Yersinia pestis* today and what Kitasato described was that while the organism now known to be Gram negative, Kitasato could not say if it was Gram negative or Gram positive. As a means of classification bacteria are separated into two large groups based on structural differences in their cell walls: Gram positive bacterial cell walls have high peptidoglycan content and lack the outer membrane of Gram negative bacteria. They can be differentiated through means of a dye test, called "Gram staining"; Gram positive bacteria retain a violet dye and appear purple under a microscope, Gram negative bacteria do not, and are counterstained with a red dye. Yersin correctly describes it as Gram negative, but an examination of his preliminary data by Bibel and Chen reveals that he too had met with inconsistent results in Gram staining[149]. It was not then the simple procedure it was in the 1970s or is today; they did not have the same chemicals at their disposal nor the same thoroughly controlled environments, and most importantly, Gram's original method of decolourization of the dye involved absolute alcohol, any dilution of which could increase the process, making the timing of these observations critical – Howard-Jones's incredulity that it could have taken him so much time to get accurate results is, according to Bibel and Chen, baseless[150]. Hirst's supposition may have been correct after all – Yersin was "the better technician"[151]. They then describe the multitude of morphological and pathological differences between Kitasato's original description and Streptococcus, concluding that if he was indeed describing a contaminant between 1895-1897[152], that contamination

occurred well after his initial discovery, and that he "should not be denied credit"[153] for it.

If he had been describing a contaminant, as well, they provide a number of reasons he may not have published this fact explicitly – though "having determined them to be identical" in his 1899 paper does imply that he had once believed they were not – including the factional rivalry within the microbiologists of Japan and Kitasato's tenuous position among them. What both Howard-Jones and Bibel and Chen all overlook is that he did publish such a clarification, after receiving Kolle's results – in Japanese[154]. Did Kitasato, once again, possess an intellectual obligation to publish it in the dominant language of his discipline instead? Or would that have been beside the point, since by then Zettnow and Kolle's studies were widely disseminated? Or is it more ambiguous than that – since he had never published a scholarly research article claiming that Yersin and his bacillus were not the same organism, though may have said so in lectures and other commentary, did he possess an obligation to publish a retraction at all?

Language was to become an issue in another way in publications on the plague controversy to follow. Bibel and Chen's work was dense, highly technical, and published for the consumption of fellow scientists. Howard-Jones' was aimed at those with a general interest in the history of medicine. This may have been why, several decades later – while publications in the1980s and early 1990s, such as those by Butler and Cunningham, were careful to follow Bibel and Chen's recommendations – works began

to appear in the mid-90s that cited Howard-Jones alone. The first two works pertaining to Lowson's diary were perhaps examples of such, coming to Howard-Jones' conclusion that Yersin alone deserved credit.

But Tom Solomon's 1997 *Lancet* publication cannot be explained away so easily. Solomon is a scientist-historian himself, and the bulk of his work is in the field of microbiology. He cites Bibel and Chen's 1976 paper, and it seems unlikely that he could have misunderstood it. Yet he begins by saying that while Kitasato "was credited with the discovery for many years… in more recent years Yersin has received sole recognition"[155]. This is true – sole recognition from Yersin's students decades ago, Howard-Jones, and those who cite only Howard-Jones; but many recent texts still credit both[156]. He goes on to say that Kitasato may have rushed to publication too soon, and that "the consensus of the many people who have reviewed the question in detail over the past century is that Kitasato did exactly that"[157], for which he cites Howard-Jones, Lagrange, Severn, and Butler. Howard-Jones and Lagrange do not put the blame for inaccuracies on a rush to publication; they assert that he did not discover the plague bacillus at all. Butler advises dual credit[158]; Severn was a staunch Kitasato supporter[159]. Solomon goes on to say that "it seems likely that his first cultures were contaminated by (*Streptococcus*) pneumonocci, and although he may have subsequently isolated the plague bacillus, all his reports were confounded by the initial findings"[160] – for which he cites Howard-Jones, Lagrange, and Bibel and Chen. Once again, Howard-Jones and Lagrange do not support the idea that he isolated the true plague bacillus at all. More importantly, this conclusion is precisely the opposite of the one Bibel and Chen reach:

that while his first cultures were undoubtedly *Yersinia pestis*, later cultures may have been contaminated and resulted in the confusion between 1895 and 1897. This is an incredible error for a microbiologist to make, particularly when many non-scientist historians preceding him had correctly identified Bibel and Chen as supporters of Kitasato's initial claims.

This is not the only surprising inaccuracy in Solomon's report. Written to reflect the "new evidence" Lowson's diary provided, he furnishes many quotes from the original materials. A main thrust of his argument is that Lowson did not get on with Yersin and tried to disadvantage him, favouring Kitasato. To this end, Solomon writes that "he never once entertained Yersin, and would not even write his name in his diary, only ever referring to him as 'the Frenchman'"[161]. Lowson's diary reads otherwise. There is at least one mention of entertaining Yersin on June 16th[162], and the French scientist is indeed referred to by name[163]. (Lowson also refers to Kitasato and his research group frequently as the "the Japs".) In fact, Solomon had to edit a quote from the original to support his original assertion: from "Yersin got his bacillus" to "Frenchman got his bacillus" on the date of Yersin's discovery[164]. This isn't the only embellishment from the diary: he describes the last entries, on the subject of the controversy, as "furiously scribbled"[165], when this would do better to describe his other entries; these last were, by comparison precise and tidy[166].

Why would Solomon do this? The diary itself is rare – only three copies exist, in the library of Lowson's hometown of Forfar, in the Museum of Medical Sciences in Hong

Kong, and in the archives of Kitasato University in Tokyo – but it is by no means inaccessible, nor are the writings of Choa and Yule, who quote directly from the diary, in which Yersin's name is included. Solomon's argument is that Yersin alone deserves sole credit because Kitasato rushed to publication, and it was Lowson and his abiding dislike of Yersin that pressed him to do so. To this end he seems to stretch the evidence available: for example, he quotes Yersin's diary as saying that the Japanese had ignored the buboes of the patients, and had only begun to examine them after Lowson spied the French scientist doing so[167]. Solomon points out that Kitasato's "subsequent publications" claimed they had done so from the beginning – but the initial interview he points to that did not mention the buboes was given mere days after they had arrived[168]. All publications at this point were subsequent. And both Aoyama and Kitasato's notes describe examinations of the buboes from the very start[169]. To this end he also distorts later events between the three: Solomon writes that "even years afterwards, Lowson made embarrassing statements about the Frenchman's credibility, which had to be retracted subsequently"[170]. He cites a series of letters in the *British Medical Journal* in which a fellow doctor writes to say that he is sure Lowson has been misquoted in the condensed minutes of a conference as saying Yersin's work was suspicious – surely he was referring to Yersin's antiserum. Yersin's antiserum work, was, at the time questioned by sources that had little to do with Lowson: the German Commission at Bombay and the Indian Plague Commission had utilized his antiserum and obtained results that fell far short of Yersin's published values, and suspected that he had been selecting individuals with the best prognosis to administer his serum, while

using the most dire cases as controls[171]. Far from retracting this statement in his reply, Lowson published it in full – he was indeed referring to the antiserum and praises Yersin's commitment as a scientist and humanitarian otherwise[172]. Solomon ends by saying that "Kitasato, Yersin, and others involved in the 1894 epidemic received high honours, but Lowson never did" and that "after spending time in India and Australia, he returned to his native Forfar"[173]. Lowson was "spending time in India" after being selected to lead the Indian Plague Commission – after having been decorated by the Japanese government and rewarded with a gold medal by his own – and he "returned to his native Forfar" after contracting tuberculosis, where he eventually became a well-respected Provost of the town and served as an unpaid medical officer during the First World War[174]. If Solomon meant to use these to imply that Lowson suffered an ignoble fate for his part in the controversy, it does not match with his writings or his biography.

Why would Solomon present his sources this way; why would this be the thesis of his article in any case? The plague controversy by now was a century old. A possible explanation is the one provided by Nakayama in his description of the pitfalls of the scientist-historian: Solomon was at this time employed by the Centre for Tropical Diseases in Vietnam[175], where Yersin lived out his late career dedicating himself to advances in public health, and is still venerated as a hero: every year on March 1, the anniversary of his death, a ceremony is held at his grave site in Nha Trang[176]. As even Solomon admits, technically, Kitasato discovered the plague bacillus several days before Yersin did – if he is not discredited somehow, the national hero of his home institution comes in second place, and it would be far less controversial, perhaps, to Kitasato's

supporters to place the blame on a relatively unknown Scottish doctor than the man himself.

Part 3: Popular Science Interprets

Marriott takes Solomon's interpretation and crafts from the details his own narrative: "maverick" humanitarian scientist Yersin and his race to defeat "establishment" imperialist Kitasato[177] and his unscrupulous Scottish henchman Lowson[178]. Even without Solomon's article as a reputable, scientific basis for his account, Yersin in many ways unrelated to science makes a more attractive protagonist than Kitasato. To begin with, numerous materials on Yersin are available in French, a language more likely to be accessible to most English speakers than Japanese; many of these are also available in translation. Yersin was also, it can be – and frequently is – intimated, less complicit in the colonial nature of the science being conducted. While Kitasato was working hand in glove with British authorities and at the behest of his government, Yersin travelled to Hong Kong of his own volition to pursue discovery[179]. *Plague*, for example, assumes the reader is under the impression that Britain and Japan are already adversaries, and that it would be more natural for them to collaborate with the French, even though the period in question is far closer to the Anglo-Japanese Alliance of 1902-1921 than WWII[180]. Furthermore, Kitasato was at this point hailed the discoverer of natural immunity, a "Professor", and the head of his own laboratory, and

he quickly befriended the British officials. Therefore Yersin, younger and less well-known in comparison, can be portrayed as the plucky underdog.

But what was Kitasato well-known for, exactly? Once again Kitasato's – and Aoyama's – accomplishments are not described, though Yersin's are, in detail. For all he was the favoured contender, details about Kitasato in these chronicles of the controversy are scant compared to Yersin. Or, in the case of Marriott, even inaccurate: Marriott credits him, for example, for world renown on account of his discoveries regarding tuberculosis[181]. While Kitasato had indeed published twice on the subject, it was in relation to Koch's ill-fated tuberculin, which had generated its own storm of controversy for being of dubious use[182]. Elsewhere he refers to his isolation of tetanus and anti-tetanus "vaccination"[183]. Certainly his fame derived not from this, but from his discovery of natural immunity, a discovery Marriott does not discuss. Marriott also describes attacks on the legitimacy of Kitasato's discovery of the plague bacillus from his contemporaries in Japan as "the last thing he could have imagined", because with other Japanese bacteriologists "he would have expected his reputation to be safe"[184]. But wouldn't his own home country be precisely where one might expect his scientific rivals to be? Indeed, the bulk of these attacks came from Ogata Masanori – the lead scientist of the Tokyo Imperial University faction opposed to Kitasato's private institute – a scientist with whom Kitasato had had a long-standing personal feud[185]. Marriott makes no mention of this feud, in favour of the implication that Japanese science was a uniform, monolithic entity in which criticism was discouraged, denying it the same diversity and academic disputes possessed by its European counterparts.

Although Marriott does include Bartholomew and Bibel and Chen amongst his references, his major sources were almost certainly Solomon and Howard-Jones; his epilogue is a paraphrased version of their conclusions. With no background in the natural sciences, journalist-historian Marriott can hardly be faulted for presuming Solomon had referenced Bibel and Chen's document accurately; uncontradicted, Howard-Jones' evidence appears damning indeed. Like Howard-Jones, however, he does not thoroughly investigate his Japanese sources: a "never before published"[186] letter from Lowson to Kitasato is used to provide groundbreaking new details on their relationship, which had in fact been published in the *Japanese Journal of Bacteriology* seven years prior and discussed therein[187]. Nevertheless, he too insinuates that the lectures Kitasato gave in his own language were a way of hiding proof: scientific proof, as well as evidence that he disagreed with the way the Chinese residents were treated by the colonists during the quarantine[188]. Oddly, Marriott presumes we will be inclined to disagree with Kitasato on this point. He follows suit with Solomon in using the diary as damning evidence of Lowson's scorn for Yersin, though in journalistic thoroughness he corrects both of Solomon's inaccurate claims of "never" to "seldom"[189]; though he does refer to some of Lowson's statements about Yersin – that he refused to collaborate with Kitasato – as "spite and untruth"[190] that were, according to other historians, more or less accurate[191]. Once again we see the influence of language, and once again, the marked tendency to assume homogeneity on the part of the Japanese.

Part 4. Other Perspectives on the Controversy

But, as we've seen, at the time of the plague controversy Japanese clinicians and microbiologists were deeply divided. What a hypothetical Japanese speaker – who would, at this time, also likely be able to read some German – thought of it would probably have depended on his affiliation with either Tokyo University or the Institute of Infectious Diseases. If, as Howard-Jones puts it, "the legend that Kitasato was the co-discoverer of (*Y. pestis*) …eventually came to be accepted even in Japan"[192], how did this come to pass? An examination of the literature available on the subject in Japanese sheds light on a parallel, but wholly separate narrative.

Howard-Jones himself, though using primarily Japanese microbiologists as sources, provides no explanation, stating simply that "error prov(ed) to be almost invincible and truth vulnerable"[193]. As mentioned, he makes use of no Japanese language sources, yet is confident in what amounts to a sweeping condemnation of Japanese scientist-historians and historians of science alike: even though "all Japanese engaged in plague work… discredited Kitasato's discovery" [194]at the time it was made, not a single Japanese historian in half a century could uncover any of these writings and debunk this "myth".

Could this really have been the case? It certainly would have been, as Howard-Jones puts it, "extraordinary"[195] if it were. Writing in 1973, the most recent source used in his article was the letter published in 1955 by Ogata Norio, the son of Ogata Masanori, stating that "priority for the discovery of the plague pathogen cannot be

attributed to Kitasato"[196]. Though the original contributors to the controversy were by this time gone, it would seem that the distribution of credit remained divided along factional loyalties. By the time of Howard-Jones' writing, however, Japanese historians writing in English were largely unanimous in giving Kitasato credit.

Though it has never been cited by English-language publications on the plague controversy, Takano Rokurō's Japanese-language biography of Kitasato[197], first published in 1959, is likely the source of this shift. Kitasato's biographies up until this point, such as the one by his student Miyajima Mikinosuke, were largely laudatory eulogies – Nakayama's "deluxe volumes of collected reminiscences" – with little or nothing in the way of critique. Takano, in spite (or perhaps because) of his position as director of the Kitasato Institute provided an extensive examination of Kitasato's life and works, including criticism of both. In his section on the plague controversy, Takano references Kitasato's detractors, and points out the errors in Kitasato's description of the bacillus[198]. He comes to the conclusion that while his later cultures may have been contaminated by *Streptococcus*, leading to a false Gram positive result and the resulting confusion, the cultures that Kitasato sent directly from Hong Kong to Germany were according to Zettnow and Kolle identical, and so Kitasato deserves credit as co-discoverer[199]. In other words, precisely the same conclusion Bibel and Chen came to independently nearly two decades later after a thorough examination of the morphology and pathology Kitasato first described.

Takano's reasoning seems to have been accepted by most Japanese historians and forms the prevailing view. In his history of the Institute and its scientists, Nagaki Daizō also described the controversy[200]. In addition to an argument identical to Takano's, Nagaki provides another source never used in English language discussions of the controversy: Sata Aihiko, a bacteriologist studying in Germany with no affiliation with either the Institute or Tokyo University – he was, by contrast, a graduate of the Osaka Medical School – had performed experiments similar to those of Kolle. He collected samples from plague-stricken sites across the world, including Kitasato's from Hong Kong, and after a series of tests concluded, like Kolle, that they were the same organism. He published this research not in German, but in Japanese, in 1900[201]. This was the same year that Yabe published his article dismissing Kitasato's claims, and could well be the reason that Yabe was the last of Kitasato's contemporaries to do so.

By the time Yule, Choa, and Solomon were marking the centennial of the discovery and the uncovering of Lowson's diary in the 1990s, Kitasato's status as co-discoverer of the plague bacillus was in Japan uncontroversial. The aforementioned 1995 article by Nakase Yasukiyo in the *Japanese Journal of Bacteriology* regarding his plague discovery does cite Howard-Jones, but only in reference to a mistaken point of view[202]. A translation of Bibel and Chen's work appears alongside Takano's writings in the references regarding the "controversy", but the controversy is not discussed in detail and Kitasato's discovery is unchallenged. In a follow-up article by Nakase in 2003 regarding Kitasato's scientific discoveries, that includes information from Lowson's diary, his status as co-discoverer is accepted as fact and the controversy not discussed at

all[203]. Howard-Jones' assumption of homogeneity on the part of Japanese historians was accurate, but not for the reason he had supposed – rather, the opposite. They had access to more information than he had, not less.

Part 5. Uncontroversial Narratives.

The problematic tendencies found in English language materials, however, arise in articles about Kitasato completely unrelated to the plague controversy, as well. Although he falls into the same trap of misstating the accomplishments of his subjects – he credits Kitasato for a rabies vaccine, one of Louis Pasteur's most famous accomplishments – unlike Marriott and Howard-Jones, Michael Shiyung Liu discusses factional rivalries between Kitasato's Institute for Infectious Diseases and the biomedical researchers at Tokyo Imperial University, and by extension Japanese Home Ministry and Ministry of Education, in depth[204]. Liu notes that in 1916 the "university faction" was victorious: without Kitasato's permission and against his wishes, his institute was placed under the administration of Tokyo Imperial University. (This led to Kitasato's resignation, along with the resignation of his entire staff, and the creation of the privately-funded Kitasato Institute.) The result, according to Liu, was that a number of researchers of the "Kitasato faction" went abroad to practise medicine and public hygiene in Japan's colonies. An entirely plausible explanation, but Liu offers it in lieu of Basalla's "diffusion" model of colonial medicine: Japan did not, he claims, have sufficient medical or scientific resources to spill over into the colonies; another explanation is

necessary, namely that these scientists had been displaced. Once again, we see an appeal to an historically weak Japanese biomedical establishment. According to Liu, "Japan's own process of medical Westernization… dated only from 1874", there were a mere 40,215 qualified doctors by the end of the 1890s to serve a population of some forty million, and the "overwhelmingly adverse" conditions of the colonies would not have been attractive to qualified Japanese doctors[205]. As we have seen, Japan's process of medical Westernization began centuries before 1873; moreover, slightly over one doctor per thousand citizens is roughly comparable to the 1.56 doctors per thousand to be found in the United States in the same decade[206], and one could describe the conditions in Hong Kong as adverse for the same reasons – yet colonial scientists appeared there, too. More importantly, Liu relies on data from the 1890s to make inferences about the state of Japanese science in 1916, when enrollment in medicine had increased five-fold in the thirty years between his data and the "victory" of the university faction.[207] Competition for official and academic positions had increased from 2:1 qualified applicants per opening in 1897 to 6:1 in 1917[208]. In other words, these are *precisely* the conditions under which we might expect to see diffusion occur. Liu also emphasizes the random and personal nature of decisions to move abroad, that the spread of modern medicine from Japan to her colonies could be described as an "accident"[209] and had it not occurred, "a whole generation of elite Japanese doctors might not have left Japan"[210]. Yet Liu himself describes the targeted, as opposed to random, nature of colonial medicine: the decision to first move resources abroad to protect colonists, then their workers, then public health in general, under the diffusion

model[211]. Since the first priority of the Japanese scientists was to increase the "'healthy' zone for Japanese colonizers"[212], it is safe to say this was no accident. Had the dispute between the university faction and the Kitasato faction never occurred, it seems more likely that imperial authorities would simply have found other doctors to take their places.

Derek S. Linton – the biographer of Behring mentioned earlier – describes Kitasato over the course of his collaboration with Behring, and similar omissions and implications about Japanese science can be drawn from his work. In describing Behring as the only one who had done experiments related to antiserums in the collaboration, whereas Kitasato provided the technical expertise, he overlooks Kitsato's paper on antitoxic reactions to tetanus with Theodor Weyl, published well before Kitasato collaborated with Behring[213]. This is an historically relevant omission not just for Kitasato's career, but for the story of their discovery: it was this work with Weyl that led Koch to suggest he collaborate with Behring in the first place, and so history was made[214]. Linton, when discussing the 1901 Nobel Prize in Medicine, which was awarded to Behring, does not agree that it would have been natural to award it to Kitasato as well, as their European peers would have found it "somewhat odd and highly controversial to award the Nobel to both"[215]. This assertion seems dubious in light of the fact that Kitasato was also nominated for the Prize, with precisely that recommendation – a shared award[216]. He appeared on the very first list of nominees[217]; why would it have been "odd" to nominate the then-famous Professor Kitasato for his accomplishments? The implication was that the Japanese scientific community did not

have enough clout to call for it. While that may have been so in some cases, it was clearly not in all, and it was on political, rather than scientific grounds. Japanese scientists would be overlooked for political reasons for the same award several times in the pre-war era, including cases that are now broadly regarded as cases of mistaken credit[218].

Linton champions Behring in the same context that Solomon writes about Yersin: defending his primacy as chief discoverer. But why would he do this? Kitasato was Behring's coauthor, not his institutional rival. Not only did he contribute to the paper that won the Prize (there is some evidence, which even Linton is forced to admit is credible[219], that Kitasato was the primary author, and Behring's first authorship was based on his superior position in the laboratory), but Behring also had many non-scientific reasons to later play down Kitasato's contribution: while Kitasato turned his intellectual property rights for antiserum production over to the Japanese government for public use, Behring kept his, and took in steep profits as a result. What difference would it have made to acknowledge Kitasato's role thoroughly? Roux, Yersin, Metchnikov, Ehrlich, Weyl, Koch, and even Ogata were working in the same area chasing the same conclusion, though Behring and Kitasato were the first to reach it. There is no question they influenced each other, looked over their respective contributions and publications, and made improvements; let alone those scientists who collaborated directly. Kitasato, too, shared progress updates, resources, findings, and even slides of his bacillus with Yersin. What damage does it do to the credibility of either to

acknowledge that they both discovered *Yersinia pestis*? Is it accurate to portray the increasingly collaborative world of scientific research as zero-sum?

Linton is a historian, but it seems he has fall into the trap described by Nakayama, even though, unlike Solomon, he has no institutional affiliation to force his hand. As Bruno Latour puts it, "a cause is always the consequence of a long work of composition and a long struggle to attribute responsibility to some actors"[220]. In other words, those of James English in his elucidation of how controversy actually contributes to the credibility of awards and prizes in *The Economy of Prestige,* attacking or defending to whom recognition was awarded, and why, merely reinforces that there *is* an objective answer: someone who, even in artistic pursuits, is 'most deserving' of it[221]. In the case of science, controversy propagates and reinforces the notion that, even in collaboration, there is a scientist who is 'most deserving', whose ideas or contributions were objectively most important. This is why even historians of science keep trying to hammer the square peg of the solitary genius toiling away in his laboratory until a moment of brilliance strikes into the round hole of the modern, highly collaborative discipline, with all its multiplicity of contributing authors and attendant host of peers, students, and technicians. Not that scientist-historians do not also have a role to play in correcting historians of science, as the example of Bibel and Chen demonstrates so amply here: they might be beholden to the institution that funds them, but they can also provide great clarity by sticking to direct, scientific observations they understand intrinsically, in the international language of science, where an historian might be weighed down by competing, contradictory, second-hand claims.

This thesis will endeavour to find a more useful template for describing science in collaboration; the emphasis will not be on discrediting Behring and Yersin or even championing Kitasato beyond what has been said here as part of the historical narrative, but rather on how they interacted with each other and the politics that organized them – what they exchanged, what they hoped to gain from it, and what they did gain from it in the end. The champions of each scientist in the controversy certainly knew who they were defending and why they defended him. With a century of controversy laid out before us, we can finally see what this has done, and strive toward a different outcome.

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[159] Severn (1927) p. 209. "There are certain difficulties in the way of accepting either Yersin or Kitasato as the prior discoverer of the *Bacillus pestis*, but, in the opinion of this writer at least, the balance of the evidence is in favour of the Japanese."

[160] Solomon (1997) p. 61-62.

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[162] Lowson, James. Diary 1894. Puzzlingly, Solomon himself acknowledges this (p.61) though he writes that Lowson only did so "to maintain the appearance of impartiality". Solomon does not provide a source for his insight into Lowson's motives.

[163] Lowson (1894) pp. 4-120.

[164] Lowson (1894) p. 120. Also reproduced in Choa (1993) p. 137, Yule (1995) p. 185, and Marriott (2003) p. 278 with Yersin's name intact.

[165] Solomon (1997) p. 62.

[166] Lowson (1894) p. 120. Not only in this author's opinion, but that of Choa (1993) who describes them as "written in very clear, tidy, and easily legible handwriting".

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[173] Solomon (1997) p. 62.

[174] Choa (1993) p. 144, Yule (1995) p. 185.

[175] Solomon (1997) p. 59.

[176] Hawgood (2008) p. 171.

[177] Marriott (2003) p. 36. Interestingly, the term "maverick" is one that James Bartholomew, the foremost expert of Kitasato in English, has used to describe Kitasato (Bartholomew (1982) p. 331).

[178] Marriott (2003) refers to Lowson as an "irascible skew-eyed scot" (p. 87) "pawing the ground" before Kitasato (p. 62).

[179] Marriott (2003) pp. 2-9.

[180] *Ibid*., p. 37.

[181] *Ibid.*, p. 68.

[182] Klöppel, Ulrike. (2008) "Enacting Cultural Boundaries in French and German Diphtheria Serum Research," *Science in Context*, 21(2): 161-180, p. 168.

[183] *Ibid*., p. 10. As discussed in detail in Chapter 4, antiserums are vaccines, and Kitasato did not discover a vaccine for tetanus in any case.

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[194] *Ibid.,* p. 306.

[195] *Ibid.,* p. 307.

[196] Ogata (1955) p. 172.

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[210] *Ibid*., p. 51.

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[215] Linton (2005) p. 83.

[216] Bartholomew (1998) p. 250

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[218] Such as awarding the 1926 Nobel Prize in Medicine to Johannes Fibiger instead of Yamagiwa Katsusaburō; see Chapter 4.

[219] Linton (2005) p. 79.

[220] Latour, B. *The Pasteurization of France*. Sheridan, A. and Law, J. trans. Cambridge: Harvard University Press, 1988, p.258.

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Chapter 3 – Hong Kong, 1894

Introduction: A Cast of Scientists

To begin with, we must construct our own historical narrative; the setting for the laboratory and the cast of scientists. In 1892 Kitasato had returned from Europe to Japan, rejecting offers of professorships both abroad and at home in Tokyo Imperial University[222]. Beyond the bad blood that now existed between him and the University scientists, he desired an independent laboratory in which to work on clinical and public health issues over and above academic or pure research. When no funds were forthcoming, he reconsidered the offers of tenure overseas and was set to leave until a small but influential group of supporters, including Fukuzawa Yukichi – prominent political theorist, entrepreneur, and personal friend of Kitasato – convinced him to stay with a loan for the construction of a small laboratory[223]. For Fukuzawa, who viewed the strengthening of Japan both militarily and technologically as an absolute necessity to resist the influence of European colonizers, losing Japan's most prominent microbiologist to a foreign nation would have been an unforgivable waste. Kitasato set part of his new laboratory aside as a private clinic for tuberculosis patients, and gathered enough funds through treatment and the production of antiserums to repay Fukuzawa in full within a year.

By 1894, Kitasato was what Hagstrom categorizes as a "highly-involved leader": in a position of authority and mentorship, yet still pursuing and publishing his own research[224]. Later he would become more involved with the politics of institution building and oversee the research of his students, and earlier his network of collaborations were confined to Koch's school in Berlin, but now his influence reached well beyond institutions and borders. Even so, he wanted more: against Fukuzawa's better caution, a large, publically funded research institute that would oversee epidemics, quarantine, and the training of public health officials, as existed in many European nations at the time. Many critics saw no reason that this could not be adequately handled by the laboratories of Tokyo University – chief among them the microbiologists of the university themselves[225]. If Kitasato was to convince them he would have to prove himself; when the announcement arrived from British-occupied Hong Kong that there had been an outbreak of the bubonic plague, and a call for the scientists of the world to find a cause an a cure, it must have seemed the perfect opportunity.

The perfect opportunity not only for Kitasato, but for members of the university faction to increase their prestige, as well. Once again he found himself facing rival microbiologist Ogata Masanori for a place in the mission[226]. Competition was fierce, but in the end, Kitasato was chosen to study the bacteriology of the plague, and Aoyama Tanemichi – Tokyo Imperial University's Dean of Internal Medicine – its treatment. Aoyama and Kitasato could not have been of two more different temperaments: Aoyama was reserved and officious, and did not like socializing – particularly with

students[227]. He had come out strongly against Kitasato for publishing a critique of Ogata's work; he refused to allow his own students to criticize their superiors in academia at all[228]. According to some historians, he would be the driving force behind the government seizure of Kitasato's Institute for Infectious Disease in the decades to come[229]. With such antagonism within his own research team, it does not seem like a stretch to assume that Kitasato would have been looking for allies upon his arrival in Hong Kong.

He found one in James Lowson. At twenty-eight, the young Scottish naval surgeon was acting director of the colonial hospital, responsible for control of quarantine measures and the oversight of the scientists who arrived to study the plague[230]. Hot-headed and impetuous – his diary records his almost daily arguments with his superiors, his travels into plague-affected areas in disregard of their caution[231] – he somehow befriended Kitasato. Like most clinicians of his time, he was familiar with German, and with Kitasato's antiserum research[232]. The opportunity to co-operate with one of the most renowned scientists of his day must have been exciting – but Lowson had no qualms about expressing his dislike for other highly influential figures of his acquaintance[233]. Just as importantly, perhaps, he and Kitasato had a number of individual characteristics in common. Both were sportsmen: Kitasato practised judo and kendo well into the years of his doctorate[234], and Lowson was an "eminent" cricketer[235] who played for Hong Kong in the Interport League until the shipwreck of the S.S. Bokhara – of which Lowson was one of only twenty-three surviving passengers of a total of 148 aboard – damaged his lungs so badly one had to be removed.[236] Both men also

loved to socialize, something they would do often throughout Kitasato's stay in the colony[237]. Since he had done some microbiological research on the plague in what little time he could spare from his other duties, upon Kitasato's arrival in Hong Kong, Lowson gifted him with his slides, observations, and gathered knowledge.

Swiss-born French scientist Alexandre Yersin arrived several days later, a man as different from Lowson as Kitasato was from Aoyama. Yersin preferred solitude to company, and had few pursuits outside of the intellectual; having sworn off women altogether after a single romantic rejection, his closest confidant was his own mother[238]. Upon leaving the Pasteur Institute – the rival school to Koch's – he became and would continue to be what Hagstrom terms a "productive isolate" throughout his career: a scientist with few close connections within the community, but a respectable number of publications nevertheless[239]. In Hagstrom's research "non-productive isolates" were by far the norm[240]; productive isolates tended to be brilliant researchers in their own right, but socially marginalized for reasons of ethnicity, gender, or personality. Speaking neither English nor German well, it is possible Yersin was marginalized in Hong Kong for these reasons – later in his career, however, his isolation was likely his own personal preference. The conditions of his dispatch to Hong Kong could not have been more different from Kitasato's, either. Though he was sent under the auspices of the French government, he had personally asked to go; initially, they had intended to send no one[241]. Not only had Yersin – thirty-one years old at this time – yet to make his mark within the scientific community, but he had in collaboration with Emile Roux worked unsuccessfully on antiserum years prior, only to be anticipated by Kitasato and

Behring[242]. The discovery of and, hopefully, cure for the infamous Black Death would have been an unparalleled chance to make a name for himself and defeat a scientific rival in one stroke.

A Conflict of Interests

Solomon and Marriot make much of the animosity Yersin and Lowson held for each other. Based on their private remarks, there is little doubt of the fact: Yersin accused Lowson of spying for and taking bribes from the Japanese[243] (he did neither[244]); while Lowson wrote in an unofficial letter to Kitasato, with the same characteristic frustration that would cause him to describe members of the English colonial government in his diary as "damned cowards" and "bloody fools", that "if he can kill a man named Yersin, for God's sake do so"[245]. The most obvious foundation of their mutual contempt appears to be personality, in addition to the traditional Anglo-French rivalry and the language barrier. Marriot finds neither of these explanations sufficient, suggesting that their closeness in age made it difficult for Lowson to respect Yersin to the same degree he did Kitasato, as well as Yersin's relative lack of prestige[246].

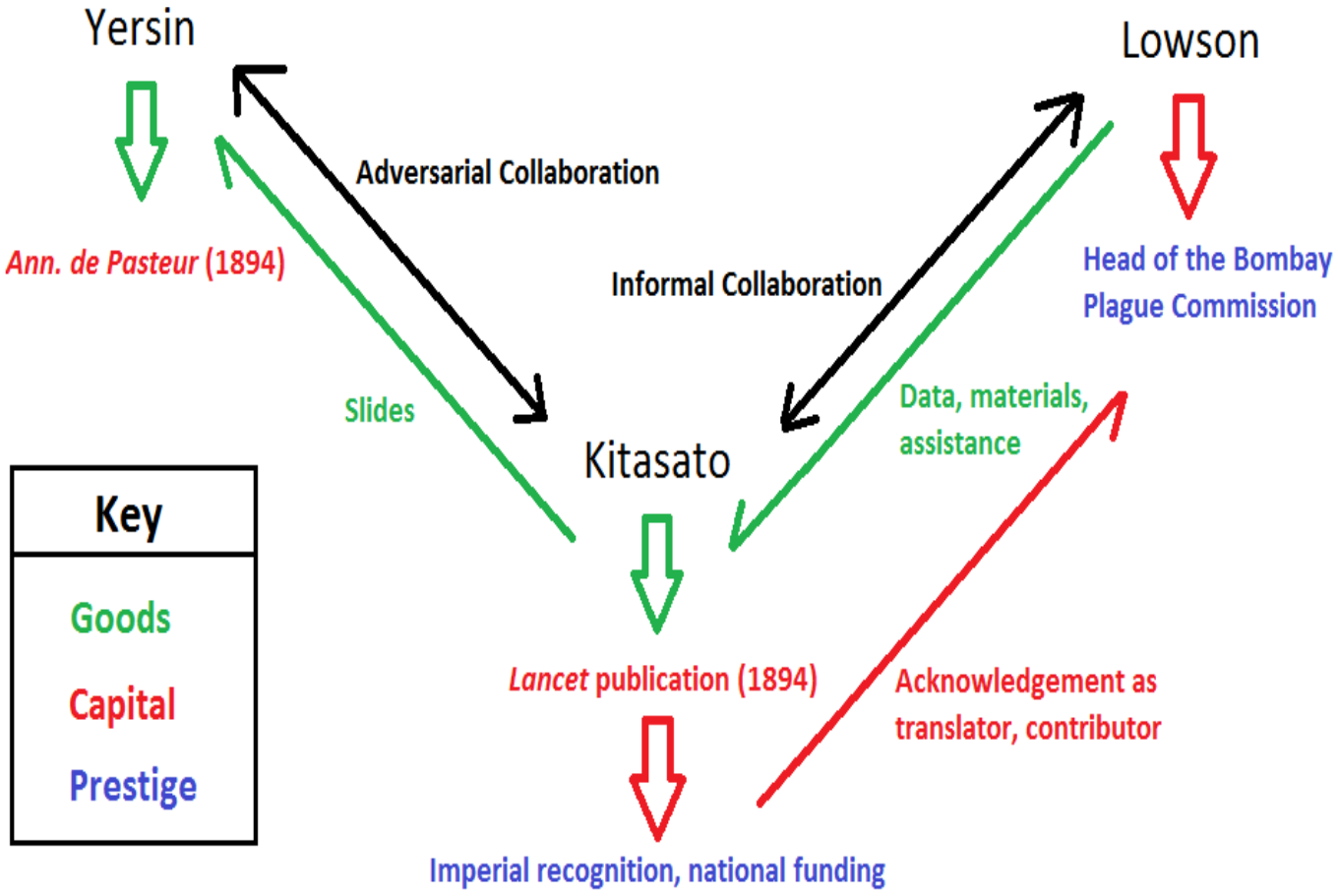
But what about more obvious causes? Lowson, in charge of the diagnosis, management, and treatment of plague victims, as well as the control of quarantine measures, was at the height of the epidemic working fourteen hours a day[247]. He writes of his frustration with the resistance of the colonized Chinese to his quarantine efforts, with the resistance of colonial bureaucrats to moving quickly and decisively during the

crisis, and with constant lack of sleep. Overseeing and procuring resources for Yersin's research was yet more work; they already had a team of scientists working on the plague when Yersin arrived, one headed by a much more eminent researcher. To Lowson this may have seemed like an unnecessary burden, and in addition to all other factors – personality, language – explains his reluctance to give Yersin the same degree of attention and assistance he did Kitasato, a personal friend.

Nevertheless, Marriott condemns this partisanship on Lowson's part and attributes the inaccuracies in Kitasato's first description of the plague bacillus to Lowson's youthful enthusiasm and a rush to publication[248] (to what he attributes the inaccuracies in Yersin's first description[249], he does not say). But as representatives of their respective states, Lowson had a number of reasons to be partisan that had little to do with his own personal feelings or Kitasato's scientific prestige. For one, in the summer of 1894 Japan was on the brink of the First Sino-Japanese War; their colonial ambitions in Taiwan and in the north matched those of the British Empire. The aforementioned Anglo-Japanese alliance, too, was in the offing. A politically savvy British official would have been well-inclined towards a Japanese state representative, as opposed to a French one who could do him few favours. In this context language, too, has other important implications unaddressed by other historians of the collaboration: Yersin's lack of English would make it unlikely that he would publish in that language, let alone give interviews or lectures as Kitasato did – and indeed Yersin did not. English was not at this time the prestige language of science, especially not of clinical science. Both researchers would have had more to gain from publication in French or German. It is

possible Lowson's friendship tipped the scales for Kitasato to submit his findings to the English-language Lancet[250]; in the terminology of gift theory, Lowson's contributions of data, resources, and information created an obligation for Kitasato to reciprocate. Since an exchange in kind would have been meaningless to a clinician, language was one of the many ways we shall find that Kitsato repaid his debt.

Figure 2. Reciprocal Relationships of the Plague Discoverers.



A Collaboration of Adversaries

The cast of scientists above seem primed for a fierce competition, a "Plague Race" as Marriott has it – so why refer to the 1894 discovery of *Yersinia pestis* as a collaboration? There was open collaboration between Kitasato and Lowson, certainly, but Lowson was a marginal scientist at best. The boundary between clinician and clinical researcher was in Kitasato's day not as wide as it is now: the former would have been expected to perform their own laboratory tests and the latter frequently took on patients. Lowson was an integral part of the management of those teams in the context of the larger operation of controlling the epidemic, but rarely do administrators and scientists "collaborate" so much as "work together". We will see, however, that in the case of Kitasato and Lowson such a fine distinction is impossible to make. The latter would not only be acknowledged by Kitasato in the same way he might a peer, but his scientific work would be published alongside Kitasato's findings.

But surely not Kitasato and Yersin? They are credited with "nearly simultaneous co-discovery" by most – at least, by most with a wider array of references than Howard-Jones – but none describes this as a collaborative effort. It can, however, be seen as one in the modern context: an adversarial collaboration, now known to be one of the most effective types of collaborative effort[251]. The organization of modern, bureaucratic efforts can impart an effectively non-collaborative structure – obviating the need for trust. Kitasato and Yersin were part of the same organized effort; they relied upon the British colonial administrators for access to space, and resources; they worked on the

same problem, within the same compound, if not in the same building. They were not two opposing forces working for opposing ends – the discovery of the bacillus and means of treatment, as we saw, did not preclude the other from reaching the same goal – but rather two parts of the same force working toward the same end.

In his research on scientific collaborations, Wesley Shrum found trust in collaborations involving administrators and technicians to be largely overrated[252]. The example he cites of two teams of physicists conducting the same research in the same large particle accelerator, with one side favoured by the administration, closely parallels the relationship between Kitasato, Yersin, and Lowson. The competition and even distrust between the two teams caused them not to sabotage each other's work, but rather to scrutinize it closely; when one side had a result, it caused the other not to abandon the effort, but rather to ensure that the discovery was legitimate and to produce a more refined effort of their own – ultimately resulting in a Nobel Prize[253]. There was conflict, as we saw between Yersin and Lowson, but Shrum points out that "most large projects are equipped to handle conflict… through the formal organizational features on which they depend for the acquisition of resources"[254]. Indeed, when Yersin felt his resources were inadequate compared to the Japanese team, he "complained to the Governor" who gave him "the same facilities they (had)"[255]. More importantly, perhaps, for the study of exchanges, is the fact that while exchanges of goods between Kitasato and Yersin, and Lowson and Yersin, were rare, they *did* take place.

A Chance for Co-operation

First and foremost of these exchanges between the adversarial collaborators are the slides Kitasato gave Yersin of his isolated bacterium. Kitasato had arrived the morning of June 12th, and discovered his bacillus on June 14th – the day before Yersin arrived in Hong Kong. He gave these slides to Yersin before either of them had published their work[256]; Yersin would take them with him when he returned to France, and even his French peers could not tell them apart from Yersin's own[257]. Kitasato had a headstart on his research, so why would he take an action that might help his competitor?

Lowson's diary may provide the answer: in it, he mentions that Kitasato had initially offered to cooperate with Yersin, but that Yersin rejected the offer. Marriott dismisses this claim as "untruth" on the part of a man who held an obvious personal dislike for Yersin[258], and neither Yersin nor Kitasato make mention of it[259], but at least one other source concurs with Lowson.[260] According to both, Yersin refused even to work in the same building as Kitasato – perhaps no surprise, so deep was his mistrust that he would later accuse the Japanese researcher of bribing the hospital staff to give him favourable access to specimens, and of sending Lowson out to spy on his research. Kitasato, then, can be seen as attempting to initiate a reciprocal, co-operative relationship by means of exchange.

If Kitasato did make such an offer, why might Yersin have rejected it? It may have seemed quite natural to Kitasato: he was accustomed to collaborating, as he had done with Behring and Weyl, and, as mentioned, he was looking for allies – if he could

gain the support of not on the administration in the form of Lowson, but in the form of another skilled microbiologist, his position with respect to Aoyama would have been much more secure. Since he and Behring had come first in their competition with Yersin and Roux, doubtless there was less ill-feeling about their previous work on Kitasato's part. More importantly, Kitasato was now unquestionably the more prestigious scientist in the collaboration – he would be taking on Yersin as a junior partner and likely second author. This, of course, may be why Yersin turned him down. He may have felt – and quite justifiably – that their collaboration, regardless of the facts of the matter, would likely be seen as yet another accomplishment on the part of the well-known Professor, and his role relegated to the footnotes of history. A look at Hagstrom's research on collaboration also reveals that many of the most common barriers were present in their case: a large difference in the status of the two scientists makes collaboration more difficult and less likely, as does – predictably – the lack of a shared language, and physical separation[261]. If Yersin had been forced, regardless of his wishes, to share the same building as Kitsato's team, they may have ended up cooperating after all.

If this was the true course of events, what happened next is also predicted by Hagstrom's theories: when he first arrived and for several days after, Kitasato described Yersin favourably, and Lowson was still giving advice to and socializing with the French scientist[262]. Within a week, their relationship had rapidly deteriorated. "Requests for collaboration may be rejected" notes Hagstrom, and based on his observations quite often "such rejection is taken as a personal affront"[263]. Lowson, with much more in common with Kitasato, clearly took his side in the falling out. Yersin's desire for secrecy,

too, is characteristic of the scientists studied by Hagstrom who had been anticipated in an important work in the past[264]. Though Kitasato was quite open about how his work had progressed with hospital officials, interested observers, newspapers, and even Yersin, Yersin's initial observations were contained within his personal journal and letters to his mother.

Repaying Friends and Rivals

The bulk of the exchanges, of course, took place between Kitasato and Lowson. And, at least initially, the bulk of the goods given were all from Lowson to Kitasato. Lowson's original work, his slides and his cultures, assistance, information and local knowledge, access to materials and resources – some of which Lowson, already greatly overworked, used his personal influence to acquire. Lowson was not, strictly speaking, a professional scientist and had no ambitions of becoming such – he was on the periphery of the community, and reciprocation in kind would have far less value to him than the worth of the goods given to Kitasato. Their friendship was already a mutual exchange; Kitasato needed something to give him professionally. He could not manufacture his finished goods from donated materials and sell them – i.e., produce and publish knowledge – without giving something back.

He could, and did, acknowledge Lowson's assistance[265], but that was only what was required of Lowson as the British official responsible for his care. Rather, Kitasato's solution to this dilemma was one that took into account the manifold respects in which

language plays a role in the production of knowledge: not only did he publish and give lectures in English, he let Lowson translate them. Though he was fluent in German, Kitasato's English, as mentioned, was conversational at best – the very first draft of his report on the plague begins in English, but he quickly switches to German[266]. Thus he not only asked Lowson to translate his first public lecture on the etiology of the plague, but invited him to be the one to deliver it, as well[267]. He had Lowson not only translate but submit his initial findings to *The Lancet*; though it describes the findings of one Professor Shibasaburo Kitasato, the first report on the discovery of the cause of the bubonic plague in history lists as its author one Dr. James A. Lowson[268]. Not only this, but his data, the slides that this marginal scientist gave to Kitasato and which are properly credited with in the article, are published on the same page as one of the most eminent bacteriologists of his time.

Ultimately these efforts produced considerable cultural capital for both researchers, capital it may have been impossible to obtain on their own. In the controversy that followed, many of Kitsato's most ardent defenders were British as well as German[269]. And Kitasato invited Lowson to Japan, in the autumn after his final publication in *The Lancet*, to share in his rewards: there he toured the leading hospitals of Tokyo as well as the Institute for Infectious Diseases, rubbed shoulders with the top health officials in Japan, and holidayed at the posh Hakone Fujiya Hotel[270]. Both men successfully converted this capital into prestige: within a few years, Kitasato's Institute received national funding and support within the Ministry of Home Affairs[271]; Lowson was selected to lead the Indian Plague Commission and act as Advisor to the Governor

of India, where another epidemic of the plague had broken out[272]. The two would continue their reciprocal relationship: Kitasato by acknowledging Lowson's assistance in committees on plague he would lead in the future, and Lowson by being sure to give Kitasato's plague discovery priority in his future letters and reports[273]. When the efficacy of Yersin's anti-plague serum was placed in doubt, Lowson brought these results to the attention of his colleagues[274] – though Kitasato acknowledged that it was somewhat effective[275]. For his part, after leaving Hong Kong, Yersin would not mention either of them again.

Where was Aoyama in all of this? He – and one of Kitasato's assistants – contracted the plague several weeks after their arrival, effectively knocking him out of the "race"[276]. In an interview with the *China Mail* on June 20th, six days after Kitasato's initial discovery, Aoyama agreed, with certainty, that Kitasato's bacillus was the cause of the plague[277]. From his writings in those first weeks, it is clear that he expected to share recognition with Kitasato when the results were eventually published, but shortly thereafter fell into a fever-induced coma for days and was too weak to do any research for weeks. When Kitasato did publish, Aoyama was still severely ill, and it was only with his name – and Lowson's – attached. Aoyama would return to Japan and later question the accuracy of Kitasato's claims[278]. Given Kitasato's inability to correctly identify the Gram staining of the bacterium for some time following the discovery, and the potential contamination of his samples[279], this is understandable; however, in the article, Aoyama would contradict his statements given in Hong Kong, now claiming that he had suspected that Kitasato had not discovered the true cause all along.

This would be difficult to understand outside the context of the tensions between the growing Kitasato or "Institute" faction and the "University" faction of microbiologists that existed at the time. Kitasato had acquired an enormous amount of cultural capital within the scientific community and prestige within the scientific establishment with his plague discovery. Following Kitasato's original publication with its uncertainty, and up until Kolle's findings that his sample was identical to Yersin's, various members of the university faction seized the opportunity to strip some of it away[280]. Though they always referred to Kitasato's lectures and articles published in Japanese, they themselves published in German or French. One possible explanation for this is that Kitasato had won his funding, and had been praised by an Imperial Prince of Japan – his position, nationally, was relatively secure, and with a competing discoverer his international prestige far more tenuous. Yabe's report is particularly incongruous: published in 1899, two years after Kolle's article, and a year after Kitasato himself had acknowledged in print that after further research he too had determined his bacillus to be "identical" to Yersin's, Yabe published in an article in French that Kitasato had made an "incredible error" – that his bacterium was nothing more than *Streptoccus pneumoniae* – a Streptococcus that, oddly, caused the bubonic plague in test animals. Yabe would have known that *S. pneumoniae* did not cause the plague; in other words, he would have, if anything, had to have made that same "incredible error" himself in his observations of Kitasato's bacillus – mistaking a mixed culture for a pure one[281]. Heedless of this most basic flaw in deductive reasoning, Yersin's student and passionate supporter Emile Lagrange would use it and Kitasato's other Japanese detractors as the

basis for a solitary claim of discovery for Yersin in publications and lectures for decades to come[282].

Repaying the One Who Pays You

It is important not to forget in this discussion a seemingly invisible player in most histories of science, ever-present on the same stage. All three researchers – Yersin, Lowson, and Kitasato – were agents of their respective states as well as members of the scientific community. All three also owed a debt of another kind. All three repaid it.

For Lowson, a public health officer of the British colonial government, it was the most direct. He was himself directly responsible for the quarantine measures intended to protect both colonists and colonial property. He needed not acknowledge the British government nor discuss his intended purpose; it was always implied. The knowledge produced by himself and Kitasato regarding the plague's cause and preventive measures added directly to his own ability to perform his role efficiently – added directly to his authority. It should be remembered that this was the era in which modern medicine truly surpassed the efficacy of its traditional counterparts. In the generations preceding, the success rate of European surgery, for example, was not demonstrably higher – it was in some cases demonstrably lower – than that of traditional Chinese internal medicine[283]. The introduction of antiserums, in other words the work done by Kitasato himself, changed this dramatically. Though some primitive vaccines had existed since the 1700s as preventative measures, for the first time in human history, infectious

diseases could be reliably cured. In the early stages of the epidemic, much to Lowson's frustration, many Chinese citizens of Hong Kong resisted hospitalization and turned to traditional healers instead. Until the cause of the plague, and its cure, were discovered, he had nothing better to offer them. In other words, his research efforts were paid directly back to the state that employed him, for use by all of their agents.

During this era Kitasato's state, Japan, felt this conflict between the traditional and the modern, the Western and the Eastern, sharply. The Meiji government championed modern technology wholeheartedly (its view of science was more nuanced, as we shall see in the chapters that follow) and Kitasato was one of the few fully funded and sent abroad to bring this technology back to enlighten the nation. Kitasato's scientific achievments were thus direct reflections of the wisdom, and the success, of their policies. Better, Kitasato demonstrated that Japanese scientists could not only adopt Western learning, but *create* it. At a time when nationalists within the Japanese government were dedicated to proving that the Japanese Empire could be the equal of any of its European peers, Kitasato's discoveries served this rhetoric well. Kitasato and Aoyama were sent to Hong Kong as state representatives; when Kitasato was given public funding, he was made a public servant, as well. When the plague reached the Japanese port of Kobe in 1899, it was Kitasato who now filled the role of Lowson, responsible for containment and quarantine.

On the surface, Yersin's role within the French scientific establishment seems more tenuous: he asked to go of his own volition, and would spend most of the rest of

his career doing humanitarian work outside of France – work for which both Lowson and Kitasato would praise him[284]. Whatever his personal motivations, Yersin's publication of his plague discovery makes clear his government's rationale in sending him. It deserves republication here in full:

"At the beginning of last May, there broke out in Hong Kong an epidemic of bubonic plague that proved deadly to the Chinese population of that city. The disease had been raging for a very long time in an endemic state on the high plateaus of Yunnan and from time to time had appeared quite near the frontier of our Indo-Chinese possessions at Meng-tsu, Lan-Chow, and Pei-hai. The great commercial movement between Canton and Hong Kong, on the one hand, and between Hong Kong and Tonkin, on the other, and the difficulty in establishing on the littoral of these lands a really effective quarantine makes the French government fear that Indochina will be invaded by the epidemic. I received from the Ministry of the Colonies the order to go to Hong Kong and to study the nature of the scourge, the conditions in which it spreads, and to seek the most effective measures to prevent it reaching our possessions."[285]

Yersin's contributions, which he personally intended as relief for the poor and suffering of Indochina, also protected the "possessions" of his home state. In another fortuitous coincidence, his publication in France, and the resulting controversy over the discovery aided the production of authority in another way. In his *Pasteurization of France*, Bruno Latour delineates the means by which, in France, more than any other nation, scientist-historians give credit to Pasteur and the Pasteur school of microbiologists alone for virtually all advances in bacteriology at this time[286]. With Yersin's publication and the doubt cast on Kitasato's by commentators such as Lagrange and Yabe, this historical narrative remained intact.

To summarize, each of the scientist's respective priorities can be summarized by noting to whom they sent the first notices of their discovery. Lowson went above his superiors to wire *The Lancet* directly[287], while Yersin first wrote his mother[288], and then reported to the French *Académie des Sciences*[289]. Kitasato, in a glimpse of the attitude that would drive the conflict between his principles and those of Meiji officials and lead to the seizure of his laboratory, first notified the Japanese Association of Hygienists – a collection of private scientists and clinicians – before reporting to his own government[290].

Conclusion: The Large Effects of Small Politics

In this chapter vividly demonstrated is the impossibility of detaching the politics of personality from the politics of state in the study of exchanges. Lowson stood to gain more from Kitasato than he did from Yersin, politically, but it would have been impossible for him to know that Kitasato would discover the plague bacillus. If his decision to favour one or the other was based purely on who could best return it, the more obvious answer would have been to support both equally until such a time at which it became obvious who would "win". This was not what Lowson chose, and thus these political justifications must necessarily be subordinate to personality and friendship. Yersin's scientific background and language abilities no doubt played a role in his decision to work alone, but it is unlikely that his introverted personality – so different from the forceful, headstrong, and sociable Kitasato and Lowson – did not also play a

role, perhaps even the decisive one. Kitasato's clever manipulation of the politics of language to repay his debt to Lowson, too, adds an entirely new dimension to the politics of exchange.

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[222] Bartholomew, James. "Science, bureaucracy, and freedom: Meiji and Taishō Japan" in *Conflict in Modern Japanese History: The Neglected Tradition.* Princeton: Princeton University Press, 1982, p. 327.

[223] Miyajima, Mikinosuke. *Teacher and Pupil*. Geneva: Sonor, 1935, p. 21.

[224] Hagstrom, Warren. *The Scientific Community.* New York: Basic Books, 1965, p. 44. For the basic characteristic types of scientists, see pp. 44-47.

[225] Bartholomew, James. "Japanese Culture and the Problem of Modern Science", in Mendholson, E. and Arnold Thackray, A. eds. Science and Values. New York: Humanities Press, 1974, pp. 118.

[226] Bowers, John Z. *Where the Twain Meet: The Rise of Western Medicine in Japan.* Baltimore: John Hopkins University Press, 1980, p. 113.

[227] Bartholomew, James. (1985) 'The "Feudalistic" Legacy of Japanese Science', *Science Communication*. 6(4) 350-376, p. 366.

[228] Bartholomew , James. *The Formation of Science in Japan: Building a Research Tradition*. New Haven: Yale University Press, 1989, p. 185.

[229] *Ibid.*, p. 152.

[230] Yule, W.L. "A Scottish Doctor's Association with the Discovery of the Plague Bacillus," *Scottish Medical Journal*. (1995) 40: 184-186, p. 184.

[231] Lowson, James A. Diary 1894. Pertinent sections extensively reproduced in Choa, Gerald H. "The Lowson Diary: A Record of the Early Phase of the Hong Kong Bubonic Plague 1894," *Journal of the Royal Asiatic Society Hong Kong Branch*. (1993) 33: 129-145.

[232] As we will see, Lowson translated Kitasato's reports from German into English.

[233] Referring, as an example, to the then-Governor of Hong Kong Sir William Robinson as an "obnoxious bloody fool" and traveling into heavily infected areas against instructions. Marriott insists that Kitasato's credentials must have impressed Lowson into deference, but he offers no evidence of this, nor does Lowson ever discuss Kitasato's credentials, and it would not have been in what we can observe of Lowson's character – who did not seem to defer to anyone's credentials if he disagreed with them and openly contradicted figures much his superior. This author is of the opinion friendship is a more likely explanation. Lowson (1894), quote also reproduced in Choa (1993) p. 139.

[234] Shinoda, Tatsuaki. "Tatakau ikon: Kitasato Shibasaburo" in *Nippon Naika Gakkai Zasshi.* 92(9): 1751-6, p. 1753.

[235] Hall, Peter. *150 Years of Cricket in Hong Kong*. Lewes, Sussex: The Book Guild Ltd, 1999. Lowson was: "A cricketer of eminence, a slashing hitter and a fastish medium bowler".

[236] Hong Kong Cricket Association. "Sinking of the SS Bokhara," Accessed 17 June 2012. http://www.cricket.com.hk/db/NATIONAL/ICC_MEMBERS/HKG/HISTORY/BOKHARA.html

[237] A number of entries in Lowson's diary refer to dining and trips with Kitasato and his associates. Some are reproduced in Choa (1993), Yule (1995), and Marriot (2002).

[238] Marriott, Edward. "Plague: A Story of Science, Rivalry, and the Scourge that Won't Go Away". New York: Metropolitan Books, 2002, pp.4-8. Yersin preferred the company of children to adults, disdained contact with women – whom he referred to as "apes" – and possessed a life-long single-minded focus on research that, even as a youth, led him to trap, strangle, and dissect his mother's own cat. It is difficult to imagine Yersin enjoying Lowson and Kitasato's social outings even if they were to include him.

[239] Hagstrom (1965) p. 46.

[240] *Ibid*., p. 47.

[241] Marriott (2003) p. 29.

[242] Klöppel, Ulrike. "Enacting Cultural Boundaries in French and German Diphtheria Serum Research," *Science in Context*. (2008) 21(2): 161-180.

[243] Mollaret, Henri and Brossollet, Jacquline. "Alexanre Yersin ou le vainquer de la peste" in *Les Inconnus de L'Histoire*. Paris: Fayard, 1985, p. 320.

[244] Nakase, Yasukiyo. "Kitasato Shibasaburo ni yoru Pesuto-kin hakken to sono shūhen," *Nihon saikingaku zasshi*. 50(30): 637-650, p. 639.

[245] Lowson, J. (1894) Personal correspondence. On display at the Kitasato Memorial Museum, printed in Nakase (1995).

[246] Marriott (2003) p. 92.

[247] Lowson, J. Diary 1894. Also mentioned in his personal correspondence to Kitasato (1894).

[248] Marriott (2003) p. 276.

[249] For a comparison of Yersin and Kitasato's respective findings and how they stack up against what are considered the definitive characteristics of Y. pesits today, see: Bibel, D.J. and Chen, T.H. "Diagnosis of plaque: an analysis of the Yersin-Kitasato controversy," *Bacteriological Reviews*. 40 (3): 633-651.

[250] Kitasato, S. "The Bacillus of the Bubonic Plague", *Lancet* (1894) ii: 428-430.

[251] Bennet, Michelle, Gadlin, Howard, Levine-Finley, Samantha. *Collaboration and Team Science: A Field Guide*. National Institutes of Health: electronic publication, 2010, p. 29.

[252] Shrum, Wesley *et al*. "Trust, Conflict and Performance in Scientific Collaborations," *Social Studies of Science* (2001) 31: 681-730.

[253] *Ibid*., p. 682.

[254] *Ibid.,* p. 683.

[255] Yule (1995) p. 185.

[256] Bibel (1976) p. 636.

[257] Lagrange, E. "Concerning the discovery of the plague bacillus," *Journal of Tropical Medicine and Hygiene*. (1926) 29: 299-303.

[258] Marriott (2003) p. 279.

[259] Yersin describes his first meeting with Kitasato uncharitably, writing that the Japanese research team mocked him for his poor German and turned their backs on him. Marriott (pp. 99-100) describes this meeting as an attempt on Yersin's part to collaborate with Kitasato, which was rebuked, but this seems an unlikely interpretation: as Marriott is forced to admit, even before meeting Kitasato, Yersin was "deliberately belittling" him in his notes (Marriott p. 98). And if Kitasato had truly regarded him with such contempt, why would he offer a demonstration of his results, a tour of his laboratory, and slides of his causative organism? This was not Yersin's sole misinterpretation of Kitasato's actions or intentions, as we saw with (unfounded) accusations of bribing the hospital staff. If he had intended to collaborate with Kitasato, but mistook good-natured laughter for mockery and felt affronted, this would not be the first time language difficulties had contributed to a rift in science – see Pasteur and Koch in Chapter 4.

[260] Choa, G. *The Life and Times of Sir Kai Ho Kai.* Hong Kong: The Chinese University Press, 1981, p. 118.

[261] Hagstrom (1965) pp. 114-115.

[262] Yule (1995) p. 185.

[263] Hagstrom (1965) p. 114.

[264] *Ibid.,* p. 114.

[265] Kitasato (1894) p. 430.

[266] Nakase (1995) p. 644.

[267] Kitasato, S. "A Report on the Plague Bacillus" delivered July 10, 1894 at the Public Civil Hospital of Hong Kong.

[268] Lowson, J. A. "The Plague at Hong Kong," *Lancet*. (1894) ii: 325.

[269] E.g. Severn; see Chapter 2.

[270] *Yomiuri Shimbun*, September 22, 1894.

[271] Miyajima (1935) p. 25.

[272] Yule (1995) p. 185.

[273] Lowson, J. *The Epidemic of Bubonic Plague in 1894: Medical Report*. Hong Kong: Noronha & Co, 1895.

[274] Lowson, J. "The Bacteriology of the Plague." *British Medical Journal* (1897) 1: 237-238.

[275] Kitasato, S. *et al*. "Die Pestepidemie in Kobe und Osaka," Veröffenlicht von der Sanitätsabteilung im Ministerium des Innern, Tokio. (1900) 1-104, reprinted in Mizunoe, K., ed. The Collected Papers of Shibasaburo Kitasato. Tokyo: Kitasato University, 1977, p. 238-239.

[276] Yule (1995) p. 185.

[277] *China Mail*. Discover of the Plague Bacillus. 20 June 1894.

[278] Aoyama, T. *MMFK*, p. 132. See Chapter 2.

[279] Bibel (1976) p. 646.

[280] For details about this controversy, see Chapter 2.

[281] Yabe, T. "Sur le microbe de la peste." *Archives of Naval Medicine.* (1900) 74: 469-472.

[282] Lagrange (1926).

[283] Nakayama, Shigeru. "Japanese Scientific Thought", in C.C. Gillespie, (ed.), *Discovery of Scientific Biography*, Vol.15, suppl. I. New York: Scribner's, 1978, p. 730.

[284] Lowson (1897) p. 238.

[285] As published in Latour, B. *The Pasteurization of France*. Sheridan, A. and Law, J. trans. Cambridge: Harvard University Press, 1988, pp. 98-99.

[286] Latour (1988) pp. 13-58.

[287] Lowson (1894).

[288] Nakase (1995), p. 643.

[289] Yersin, A. "Preliminary note," *Académie des Sciences.* 1894: p. 356.

[290] Nakase (1995), p. 643.

Chapter 4 – Berlin, 1890

Introduction: The Germ Theorists

Nine years before he sailed to Hong Kong to discover the plague bacillus, Kitasato was virtually unknown – one of the many students sent abroad by the Japanese government of the Meiji era to acquire foreign learning. While these *ryūgakusei*, as they were called, studied at institutions of higher learning all across the developed nations of Europe and the Americas, Kitasato's interest in clinical science and public heath saw him to the laboratory of Robert Koch at the University of Berlin.

The German Koch, along with the French Louis Pasteur, was one of the two great leaders of the newly-flourishing discipline of microbiology. Both ascribed disease to the germ theory, rejecting the hygienists' notion of complicated cellular imbalances brought on by any unhealthy conditions; in contrast, they believed that definite causes of diseases could be found, eliminated, and cured. Pasteur made good on the promises of research: his discoveries include the now-famous technique of Pasteurization that made milk and wine safe to drink without sacrificing taste and quality, as well as vaccines for anthrax and rabies. He soundly rejected the theory of spontaneous generation – that is, that micro-organisms would grow spontaneously from nothing in certain environments – by demonstrating that no bacterial growth would occur in sealed, sterilized nutrient broths until passage was created for them to travel inside[291]. Among his students in Paris were the aforementioned Alexandre Yersin and Emile Roux.

Though he was some twenty years younger than Pasteur and did not begin to study microbiology until comparatively later in life, Koch found something lacking in Pasteur's experiments: stringent controls. In spite of all of his successes Pasteur had failed to convince a number of highly influential opponents of the validity of their still-controversial discipline[292]. Firstly, Pasteur's experiments relied upon the assumption that his bacterial cultures were pure – that they contained only one type of organism – and secondly, they had not ruled out all other causes of disease. In nutrient broth used to cultivate bacteria at the time motile organisms – those organisms capable of moving through the broth – might travel from one colony to another; Koch used solid-state media instead where, in sufficient dilution, he could be sure that each colony came from a single bacterium. Though he used gelatin, solid state agar media is still used in the cultivation of microbes to this day. To solve the second problem, he developed his famous Postulates, sufficient to prove that a specific micro-organism causes a specific disease, which he applied to anthrax and tuberculosis:

1. The microorganism must be found in abundance in all organisms suffering from the disease, but should not be found in healthy organisms.
2. The microorganism must be isolated from a diseased organism and grown in pure culture.
3. The cultured microorganism should cause disease when introduced into a healthy organism.
4. The microorganism must be re-isolated from the inoculated, diseased experimental host and identified as being identical to the original specific causative agent.

Though Koch's work effectively shored up what had been done by Pasteur in years prior, this was not to say there was much love lost between the rival schools of Berlin

and Paris. Koch attacked Pasteur for his "blindly zealous researches"[293], while Pasteur ignored any findings of Koch's that contradicted his own altogether[294]. The two men traded barbs openly at the International Congress of Hygiene and Demography in 1882; afterwards Pasteur would write to Roux that Koch "acted ridiculous and made a fool of himself"[295], while Koch while responded in print that "all (Pasteur's) material served solely as a vehicle for a violent polemic against (him)"[296]. Partly to blame were different approaches in public health – Koch favoured sanitation and Pasteur immunization – and certainly a conflict in personality, but also the difficulty in communication. Neither Pasteur nor Koch could understand one another without an interpreter, and misinterpretation – mistaking the French word *recueil*, or compilation, for *orgueil*, or pride, and thus presuming an attack on a collection of German documents was an attack on German "pride" – led to at least one of their quarrels[297]. As we have seen, a language barrier drastically reduces the likelihood of collaboration and, combined with French-German political tensions in the wake of the Franco-Prussian War (to quote Pasteur in the wake of one of their arguments: "It was a triumph for France, that was all I wanted"[298]), had a profound influence on the conflict between the two schools – and doubtless on Yersin's decision to isolate himself from Kitasato, as well.

The Germ Theorist's Protégés

As for Kitasato himself, as mentioned during the latter half of the 19th century, the government of Japan sent promising students abroad as part of an official policy to

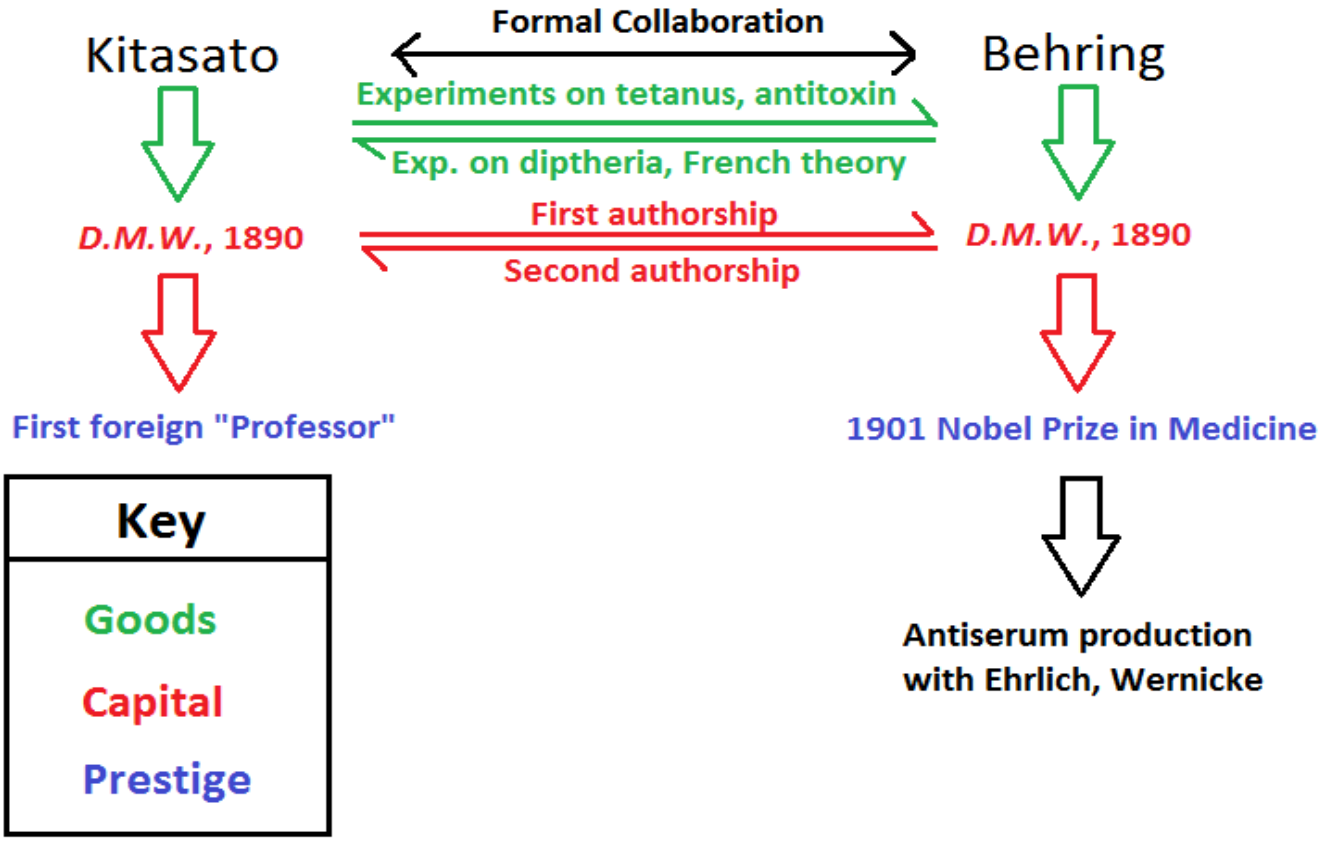
obtain Western knowledge. Those students were expected to return home and assist in the modernization of the nation[299]. Germany in particular was a favoured location for aspiring scientists; much of the Western scientific materials imported during the Tokugawa period by the Dutch were of German origin, and now Japanese intellectual curiosity could be satisfied at the source[300]. The Japanese state had decided to adopt the German model in a number of areas, and Germany was, at the time, a leader in many fields of science – but in none more than medicine. Hence we see that even though Kitasato's original mentor, Mansveldt, was Dutch, he learned German; though otherwise unremarkable at first as one of the many Japanese *ryūgakusei* who had passed through his laboratory, Koch first took notice of Kitasato's remarkable fluency in the German language[301]. Once again we see the influence of language at play: that ability would have increased the likelihood that Kitasato would find willing collaborators, such as Behring, and Weyl, and given him direct access to Koch's tutelage.

This explains, of course, why Japanese scholars would choose to collaborate with German scientists, but why would German scientists wish to collaborate with Japanese? For one, these Japanese scholars came fully funded by their government; free assistants to European researchers. Some German scientists, including Erwin Baelz (1849-1913), also viewed Japanese science much as Japanese scientists would come to view development in their Asian colonies: that the German people possessed an intellectual obligation to enlighten and modernize them[302]. Lastly, scientific collaboration follows a "strongest-partner" model in terms of quality and impact, rather than a "weakest-partner" model – in other words, collaboration between a more elite and less elite

researcher will more closely reflect the research quality and impact of the former[303]. German scientists had more to gain by collaborating with Japanese scientists than they had to lose.

In contrast, Emil von Behring was a Prussian military doctor by training, who had developed a keen interest in microbiology through the study of chemical disinfectant and sterilization techniques. By the accounts of his contemporaries Behring was intensely dedicated to his work, but could also be arrogant, competitive, and abrasive – in other words, like Lowson, he was a good match in temperament to Kitasato. According to his most recent biographer, Derek Linton, popular accounts of Behring as a conservative nationalist – accounts that, Linton believes, have caused Behring to fall out of favour in histories of microbiology – were based on artificial narratives constructed posthumously by the National Socialist Party[304]. Linton asserts (though his appeals have failed to convince some fellow historians[305]) that Behring was liberal-minded and committed to a free and democratic pursuit of science. Either way, Behring's beliefs closely mirrored those of Kitasato – who was decidedly committed to freedom of intellectual pursuits but also an unapologetic patriot. What distinguished Behring from Kitasato, and from most of Koch's laboratory for that matter, was his knowledge of French and his willingness to adopt the techniques and theories of the Pasteur school. The emphasis on sanitation was clear in his studies on disinfectants but this was, in his view, insufficient: communicable diseases required a cure as well as a deterrent. To this end he studied the diphtheria toxin, recently isolated by Emile Roux, with the intention of finding a way to neutralize it.

Figure 3. Reciprocity Between Kitasato and Behring.



Miraculous Inevitability: The Discovery of Natural Immunity

While Behring had set out with this goal in mind, Kitasato arrived at the discovery of the tetanus antitoxin from an entirely different direction. When he first arrived in Koch's laboratory in 1885 he was unknown and untried; to test his abilities Koch had him isolate common but technically demanding bacteria[306]. When Kitasato displayed a surprising aptitude for technical ingenuity in these experiments, Koch asked him to attempt to isolate the tetanus bacillus in pure culture – something that other more experienced microbiologists, including Kitasato's own supervisor Löffler, had failed to

do[307]. *Clostridium tetani* is an obligate anaerobe, meaning that it only grows under conditions with no oxygen, and grows in symbiotic cultures with other micro-organisms. Kitasato developed his own sealed apparatus in which to cultivate tetanus under a steady stream of nitrogen; he did so in pure culture by determining the temperature at which the symbiotic organisms would be killed by heating but *C. tetani* spores would survive. From his pure cultures, he was able to obtain pure tetanus toxin, and began a series of experiments with Teodor Weyl to show that not only was the toxin sufficient to cause disease in laboratory animals, but these animals could be acclimatized to it by repeated injections of non-lethal doses to the point that highly lethal doses would no longer cause symptoms[308]. Kitasato likened this to the acclimatization of organisms to increasing doses of poisons, such as cocaine – his theory was that after repeated exposure the cellular environment of the organism became increasingly hostile to the toxin. Behring, however, who had been keeping abreast of French theories regarding the transfer of immunity from one organism to another, saw another possible cause. Like Roux, he had until this point been unsuccessful in completing such a transfer. It was in his seminal paper with Kitasato on the transfer of immunity to tetanus through blood serum from one animal to another that the two proved the existence of what they called “antitoxins” – now called “antibodies” – and paved the way for the first curative therapy for infectious diseases that modern medicine would offer: “antiserums”[309].

Why did Behring and Kitasato collaborate? Japanese historian Takano Rokurō refers to the fact that the two ended up at the same problem from two remarkably different directions a “miraculous coincidence”[310], and while the subject of their experiments

may have indeed been coincidental, on the surface, the motivation behind their collaboration seems to have been anything but. Virtually all of the conditions known to improve the chances of a scientific collaboration were present. There was no language barrier, and the two were similar in age (at the time of their experiments in 1890 Kitasato was 37 and Behring 36) and informal rank[311]. Behring, as Koch's assistant, outranked Kitasato – a visiting scholar under Löffler – formally, but their positions within the scientific community as newly minted professionals with a handful of papers of moderate note were virtually identical. They were what Hagstrom refers to as "intradepartmentally oriented scientists"[312] – they lacked the prestige necessary to initiate collaborations with scientists outside Berlin. (By contrast, Koch was at this time a "student-oriented leader" – "felt to be the leader of a 'school'… whose eminence would stem partly from the success of his students in advancing his distinctive point of view"[313].) They also satisfied, of course, what is now called the "30m rule" – that the likelihood of collaboration decreases precipitously when the laboratories or offices of two scholars are more than 30 metres apart – which is to say, they worked in the same building[314]. When the two of them did find their experiments heading in the same direction, collaboration between the two was, at a glance, hardly miraculous. Rather, it appears to have been a clear example of a "free collaboration with a division of labour"[315], as Hagstrom puts it: Behring studied diphtheria, Kitasato tetanus, Behring determined the theory behind the experiments, Kitasato performed them.

When one looks closely at the circumstances of their discovery, and at the paper in question, however, the rationale for collaboration is not so clear. According to Miyajima,

a student of Kitasato's, in a version of events related to him by his mentor, Koch pulled the two of them aside and suggested that they collaborate; Behring's experiments had thus far been unsuccessful, so he should follow Kitasato's methods[316]. This account is somewhat suspect because, as Linton points out, the protocols they followed in their paper were clearly influenced by Behring's previous work[317]. Nevertheless, the original collaborative work, *The Mechanism of Immunity in Animals to Diphtheria and Tetanus,* reports a series of experiments performed with tetanus. Diphtheria is barely mentioned, in that "a series of experiments" done previously had shown that "the blood of immune animals had the ability to neutralize the diphtheria toxin" and formed the guiding principle behind the tetanus experiments[318]. The experiments with diphtheria were published later in a paper authored solely by Behring, in which he again mentions that they were done prior to his collaborative experiments on tetanus with Kitasato[319].

But if Behring had already completed his experiments on diphtheria, and established a theory to explain them, why did he collaborate with Kitasato and co-author a paper about tetanus? The evidence is considerable that, in spite of his position as second author, Kitasato was the one who wrote the article, including the fact that only he would republish parts of it while Behring would not[320] and that tetanus was his area of expertise – even Linton, a staunch supporter of the notion that Behring alone was the primary discoverer of serum therapy, concurs that it seems likely that their first paper was Kitasato's[321]. Behring may have been made first author in his place because of his higher formal position in the laboratory, combined with the fact that he had published in *Deutsche Medizinische Wochenschrift* before and was known to its editors, while

Kitasato had not[322]. According to Linton he deserves this primacy regardless because, as stated the paper, not only had he contributed to the methodology, he had come up with the explanation for their results based on his "previous set of experiments"[323]. But if this was so, why not publish the results of those first and stake a claim for himself as sole discoverer?

A possible answer is revealed in commentary on Behring's second paper by fellow microbiologists, both contemporary and in the decades that followed, with considerably more knowledge of microbiology: contemporary scientists described it as "vague"[324], and renowned scientist-historian Thomas D. Brock as "not completely convincing"[325]. In fact, Brock observes that it seems as though Behring had retroactively applied what was learned about tetanus to his explanation of natural immunity to diphtheria, as opposed to the other way around[326]. Others would note, in commentary on the development of immunology unrelated to any controversy surrounding who might deserve credit between the two scientists, what while in the first paper (the one likely written by Kitasato) the word "antitoxin" is used to describe the phenomenon they observed, in the second paper by Behring alone is it not[327] – evidence that, as least to some extent, Kitasato had a hand in the development of the theory as well as the experiments.

While Behring and his supporters would later claim that it was mostly his discovery, and Kitasato and his supporters would say the opposite, perhaps a more likely scenario based on this evidence is that both scientists provided both creative input and technical expertise. While it was Behring who did apply French theory to his experiments, it

seems likely that they were not yet at a publishable stage when Kitasato began to move in the same direction, and had he presented his paper on diphtheria first in an even more vague and less convincing state and –in the absence of their "completely conclusive" work with tetanus – it would have been subject to criticism and may not have had the impact he hoped, or worse, provide an opportunity for another scientist to obtain and publish more concrete results. Conversely, Kitasato, working in the same laboratory, must have been aware of Behring's experiments, and perhaps thought to apply them to his series of experiments on tetanus resistance already underway. One of Kitasato's other pupils, Hata Sahachirō, recounts that Koch told them to put both of their names on the paper "to avoid bad feelings" between the two[328]. If Kitasato published first, while Behring was still not ready, using experiments based on ideas that were Behring's, it may indeed have caused a rift. Collaboration, and a co-authored paper, would have been the peaceful solution – as we saw with Yersin, a rejection of an offer to collaborate certainly can create discord between scientists. Given the monumental nature of the discovery, it may have resulted in an outright feud – something that Koch's laboratory, already feuding with Pasteur's, little needed. It would have made perfect sense not only for the two of them to collaborate, but for Koch to insist that they do.

Aftermath of the Discovery: Reciprocation and Non-Reciprocation

To be sure, in the years following their discovery, Kitasato certainly was on good terms with Behring. In a letter to Behring following his departure from Europe in 1892 – which Behring had missed on account of illness, being prone to overwork – Kitasato mentions that his time collaborating with Behring was "among the most pleasant hours of my stay in Berlin", and closed by congratulating him on the success of his most recent work[329]. In another piece of equally cheerful personal correspondence to Behring, dated 1897, Kitasato asks if his "most diligent student" Kitajima Taichi (later Dean of Medicine at Keio University) could study with him in Germany, and mentions gifts of Japanese goods sent to Behring from Japan[330]. No record remains of how Behring responded to these letters; however, he must have thought well enough of Kitasato to accept Kitajima as a student for four years[331]. And why wouldn't Kitasato be on good terms with Behring? Their co-discovery produced new knowledge – that of natural immunity – and both scientists successfully converted this capital into prestige. Kitasato became the first foreign citizen to be granted the title of Professor, was offered positions at institutions in both America and Europe, and was even warmly received by Louis Pasteur himself at his Institute in Paris[332].

If anything, Kitasato remained on much better terms with him than did Behring's other collaborators. The German researchers had signed a waiver to exempt themselves from all financial rewards related to their discoveries at the institution, and so Behring convinced Koch to discontinue diphtheria serum therapy experiments at his Institute[333]. By this time Paul Ehrlich, a fellow associate of Koch's, had developed an enrichment process as well as a precise standardization protocol that developed antiserum therapy

from a possibility to a reliable, reproducible cure. Behring convinced Ehrlich, along with Erich Wernicke who had also done research on diphtheria serum, to go into business with him, signing a contract with the pharmaceutical company Hoechst to mass-produce antiserum. Both Ehrlich and Wernicke would fall out with Behring, claiming he cheated them out of due recognition and financial reward; this also caused a falling out with Koch[334]. A view of the scientific community rooted in gift theory explains why the resentment toward Behring for his actions was so strong: he had taken what was effectively community property, a cure based on contributions by Roux, Kitasato, Ehrlich, Wernicke, and others, and sold it for a profit that he kept largely for himself. In other transactional models of scientific exchange, most notably those based on market capitalism and speculation[335], there would be no reason for this, aside from jealousy – Behring patented his claim to the intellectual property first, and therefore was only doing what was in own rational self-interest. But even those who had no real claim to the fruits of their labor such as Koch, and thus for whom the jealousy argument makes little sense, resented his actions. In contrast, Kitasato turned his intellectual property rights over to the Japanese government; being, however, one of the only scientists alive at that time qualified to produce antiserum, and the only qualified person in Japan, he could be confident that they would turn the contract for serum production over to him[336]. Which they did, and his profits from their sale were considerable. Kitasato had little reason to resent Behring.

Aftermath of the Discovery: The 1901 Nobel Prize in Medicine

At least, until Behring alone won the 1901 Nobel Prize in Medicine for his research on serum therapy. Unlike some of his other colleagues Kitasato never mentioned it publically, but later in life he told his students that he couldn't understand why he had gone unrecognized[337]. To add insult to injury, in the Nobel lecture he delivered after receiving the award, Behring variously credited, cited, and thanked *seventeen* scientists and three patrons by name, including rivals in the Pasteur Institute and even chemists who had helped make his work possible – Kitasato, his co-author, was not among them[338]. Again, Behring had profited from community property, this time in terms of prestige, and again the lack of reciprocity caused alienation.

Though Kitasato's non-recognition continues to perplex Japanese scientists and historians to this day, there are a number of compelling reasons why the award committee might have chosen Behring over his collaborators. As Sachi Sri Kantha points out[339], diphtheria killed thousands of children a year prior to the development of an effective antiserum, whereas the full importance of the tetanus antiserum would not become clear until the First World War; certainly Behring's achievement had more popular acclaim. Linton adds to this the fact that, of course, Behring was first author of their paper, that according to said paper it was based on theory developed by Behring alone in experiments done prior to their collaboration, and that it was Behring and not Kitasato who transformed their experiment observations into a viable remedy[340]. As mentioned, Linton comes out strongly in support of recognition for Behring as the single

most important individual in the discovery and development of serum therapy. But if Roux's experiments on diphtheria and hypotheses regarding immunization through blood laid the groundwork for Behring's own experiments – as Behring himself says they did[341] – and if his seminal paper on immunity was written, and based on experiments performed by, Kitasato – a theory for which the supporting evidence is considerable – and if it was the combined efforts of Ehrlich, Wernicke, Roux, and many others in addition to Behring who made treatment based on these discoveries possible – an assertion for which the historical evidence is even greater – a portrayal of Behring as deserving of all recognition and all reward for serum therapy cannot reflect reality. Rather, these accomplishments belong to the scientific community that produced them, of which Behring was merely a part – albeit a very important one. Ironically, while he endeavours to disentangle Behring's legacy from the narrative produced about him in the prewar years by the National Socialists, by insisting on Behring's primacy and reducing the other characters involved to lesser, helper figures, Linton also reinforces it; what was a triumph of French, Japanese, and Jewish scientists as well as German becomes a great triumph of the latter alone.

Implications of the 1901 Nobel Prize

Ultimately, though, what does it matter if a given scientist wins a Nobel Prize or not? A great deal of luck is involved, as well as a great deal of political and institutional clout within the scientific establishment, and the selection committee is faced with the

unenviable task of weighing the importance of one discovery about the natural world against another. Moreover, at least during the early years of the awards, historians have documented a marked bias toward researchers of German and Scandinavian origin[342]. In the history of the awards a number of scientists' contributions have been forgotten or slighted, and undeserving individuals recognized[343]. Kitasato does not necessarily stand out from them, save as part of a larger trend amongst Japanese scientists with respect to Nobel Prize recognition.

Though the case for Kitasato is less than cut and dried, and claims by several Japanese historians and scientists for a Prize for Kitasato in place of Behring[344] are likely overstated (even if it was Kitasato's discovery, at first, Behring did far more to develop serum therapy afterward) a good argument can be made for what should have been a shared award in 1901. Even Linton agrees that this likely would have been the case today[345], in a climate where collaborative science is the norm rather than the exception – though his argument that it would have been "somewhat odd" and "highly controversial" in the era in question is unconvincing, given that not only was Kitasato nominated with precisely that recommendation[346], but he appeared on the shortlist of nominees[347].

But this was far from the only time in the prewar era that Japanese scientists had missed out on an award they likely should have been – or decidedly should have been – offered[348]. In what is widely regarded one of the greatest mistakes of in the history of the Nobel Prizes in science, Johannes Fibiger was awarded the 1926 Prize in Medicine

for his publication asserting that prolonged infection with worms could cause cancer in mice. At the same time, based on the same hypothesis, Yamagiwa Katsusaburō had demonstrated repeated application of tar to the ears of rabbits also caused tumors. Fibiger's result ultimately turned out to have been caused by vitamin A deficiencies common to laboratory animals; Yamagiwa's discovery was nothing less than that of carcinogens – of chemical agents capable of causing cancer. Nevertheless, because Fibiger was better-known at the time and because his paper preceded Yamagiwa's by several months, he was awarded. Recommendations of a shared Prize were also rejected[349]. In 1884, Takaki Kanehiro, a Japanese naval doctor, discovered that beriberi was caused by dietary deficiencies – a full decade before his European contemporaries would come to the same conclusion[350]. But by the time the award was given out for this discovery, in 1929, Takaki was already dead, and Nobel Prizes are not awarded posthumously. Thus it was awarded solely to Christiaan Eijkman and Sir Frederick Gowland Hopkins for their work on beriberi (that is not to say that, as we have seen, Takaki's share of it was guaranteed if he had still been alive). Kato Geni'ichi missed out on a share of the 1936 award for studies on the transmission of nerve impulses largely through lack of political clout[351].

None of these potential awards were necessarily a given, and most went to equally deserving individuals. But taken as a whole they produce a pattern that has had a profound influence on the way the history of science in Japan is perceived. If some, or even all, of these researchers had received a Prize, it would have been extremely difficult – if not impossible – to construct the popular narrative of an historically weak

scientific establishment in the years before the American Occupation. As it stands, however, the lack of Nobel Prizes reinforces that narrative: the first Nobel Prize awarded to Japanese scientist, Yukawa Hideki, was in 1949.

The Tuberculin Controversy: Future Directions for Behring and Kitasato

Regardless of the Nobel Prize outcome of 1901, Kitasato and Behring followed similar paths in their later scientific careers, profoundly influenced by their mentor. The same year that the former two scientists made their epoch-making discovery, Koch himself discovered an extract of the causative agent of tuberculosis, *Mycobacterium tuberculosis*, which he called tuberculin[352]. In a series of preliminary experiments it showed some sign of assisting infected animals, and Koch was cautiously optimistic that he had found something that might lead to a cure. As Koch himself pointed out: at the time one in seven human deaths was caused by tuberculosis; the disease was even more prolific and deadly that diphtheria[353]. This faint glimmer of hope was seized upon by Prussian officials, who pressured Koch – after all, they employed him – into vastly overstating tuberculin's effectiveness[354]. Tuberculin turned out to be a very useful tool in the diagnosis of tuberculosis, but had no curative properties, and it was not long before this became apparent. Koch, who had previously criticized the Pasteur school for its "blindly zealous" researches, had produced rushed and haphazard results.

The damage to his reputation as a scientist was irreparable, but he felt he had been given no choice, and the experience greatly embittered him toward the

governmental authorities that oversaw the administration of public health. The authority to be derived from knowledge of a cure for tuberculosis, particularly as discovered by one of their own scientists as opposed to a foreign subject, proved to be too tempting for them and the cost was Koch's own prestige. He left the University of Berlin 1891 and founded his own Institute for Infectious Diseases[355].

This was a lesson both of his pupils learned well. Behring used much of his profits from private sales of antiserums to fund his research; though he did receive both tenure at the University of Marburg and public grants, he made sure he was never reliant on them[356]. His research on antiserums for other communicable diseases and on tuberculosis in cattle took the direction Behring wanted it to and, unlike Koch, he did not answer to anyone.

Kitasato's solution was, in a very real sense, to *become* the official to whom he answered. Upon returning to Japan in 1892 he refused to work at the University of Tokyo and demanded his own laboratory. His prestige, lower than Koch's, was insufficient for one at that time, but it would soon grow. When he won public funding for that laboratory in the wake of his discovery in Hong Kong and further work on plague quarantine measures during the first epidemic in Japan, he insisted that it be placed under the control not of the Ministry of Education, where his political rivals held sway, but in the Ministry of Home Affairs with his political allies. Observers noted that, although he was ostensibly a low-ranking government official, he answered to no one and ran both his laboratory and his network of administrators like a *daimyō*. Just how a

career scientist managed to hold so much sway in the arena of politics, particularly a political establishment openly hostile political power in the hands of “academics”, is answered in the final chapter.

Conclusion: Prestige and Languages

Again we see the profound influence of language in scientific collaboration. A language barrier was part of the rift that divided the Koch school from the Pasteur school, and had a demonstrably deleterious effect on their relationship. But Kitasato and Behring were able to utilize language to their benefit. For Kitasato, exemplary skill in German allowed him to take part in far more collaborative work than most Japanese *ryūgakusei* of his era, with great success as a result. For Behring, the ability to read and understand French better than most of his colleagues – his papers and lectures are littered with French phrases and allusions to French research – gave him access to a wider breadth of knowledge, and it was doubtless a factor in why he was more welcome in their circles than his mentor. This information had a value he could bring to the collaboration with Kitasato, obligating Kitasato to reciprocate, either through technical expertise, or second authorship, or both. Also evident is the cumulative effect of individual cases of political favouritism on how history is commonly perceived; each narrowly missed Nobel Prize by Japanese scientists in the prewar years would have seemed an unfortunate but ultimately singular instance in the moment, but the overall result can be observed in popular histories of science to this day.

[291] For a collection of Koch and Pasteur's most important discovers in English translation, as well as their historical importance, see: Brock, Thomas D. *Milestones in Microbiology*. Englewood Cliffs: Prentice-Hall, 1961.

[292] Primarily hygienists, such as Max von Pettenkofer; see Dolman, Claude. "Max von Pettenkofer," in Charles S. Gillespie, ed., *Dictionary of Scientific Biography*, vol. 10. New York: Schribner's Sons, 1974, pp. 556-563.

[293] Koch, Robert. "Zur Untersuchung von pathogenen Organismen," *Mittheilungen aus dem Kaiserlichen Gesundheitsamte*. 1: 1-48, reprinted in Brock (1961), p. 103.

[294] Brock, Thomas D. *Robert Koch: A Life in Medicine and Bacteriology*. Madison: Science Tech Publishers, 1988, p. 171.

[295] *Ibid.*, p. 174.

[296] *Ibid*., p. 176

[297] *Ibid*., p. 175.

[298] *Ibid*., p. 175.

[299] Schmiedebach, Heinz-Peter. "German-Japanese relationship in pathology and forensic medicine during the late 19th and early 20th centuries", *Rechtsmedizin*, (2006) 16: 213-218, p. 213.

[300] *Ibid.*, p. 214

[301] Izumi, Yoshio *et al*. "Modern Japanese Medical History and European Influence," *Keio Journal of Medicine*. (2001) 50(2): 91-99, p.97.

[302] Bartholomew (1974), p. 109.

[303] Jones B.F., Wuchty, S., Uzzi B. "Multi-university research teams: shifting impact, geography, and stratification in science", *Science*. (2005) 322: 1259–1262, p. 1260.

[304] Linton, Derek S. *Emil von Behring: Infectious Disease, Immunology, Serum Therapy*. Philadelphia: American Philosophical Society, 2005.

[305] Huentelmann, Alex C. "One Side of the Serum Story," *Metascience*. (2008) 17: 117-121.

[306] Takano, Rokuro□. *Kitasato Shibasaburo*. To□kyo□ : Nihon Shobo□, 1959, p.24.

[307] Miyajima, Mikinosuke. *Teacher and Pupil.* Geneva: Sonor, 1935, pp. 16-17.

[308] Kitasato, S. and Weyl, T. "Zur Kenntniss der Anaeroben. Der Bacillus Tetani," *Zeitschrift für Hygiene und Infektionskrankheiten*.(1890) 8: 404-411.

[309] Von Behring, E. and Kitasato, S. "Ueber das Zustandekommen der Diphtherie-Immunität und der Tetanus-Immunität bei Thieren," *Deutsche Medizinische Wochenschrift*. (1890) 16: 1113-1114, reprinted in Brock (1961), pp. 138-140.

[310] Takano (1959) p. 40.

[311] Age, language, and prestige were three of the most important factors Hagstrom discovered influencing the likelihood of collaboration, and his finding have borne out by the social studies of scientific collaboration in the decades since.

[312] Hagstrom (1965) p. 46.

[313] *Ibid*., p. 45.

[314] Miller, Katharine. "Successful Collaborations: Social scientists who study science have noticed a trend," *Biomedical Computation Review.* (2008) Summer: 7-16, p.8.

[315] Hagstrom (1965) p. 116. Hagstrom describes the most common types of scientific collaboration he encountered. For other kinds, see pp. 116-121.

[316] Miyajima Mikinosuke. *Kitasato Shibasaburo den*. Tokyo: Iwanami Sho□ten, 1931, p. 50.

[317] Linton (2005) p. 80.

[318] Brock (1961) p. 139.

[319] Von Berhing, Emil. "Untersuchungen uber das Zustandekommen der Diphtheria-Immunitat bei Thieren," *Deutsche Medizinische Wochenschrift*, (1890) 16: 1145-1148, reprinted in Brock (1961) pp. 141-144, p.143.

[320] Bartholomew, James R. "Japanese Nobel Candidates in the First Half of the Twentieth Century," *Osiris*. (1998) 2(13): 238-284, p. 247.

[321] Linton (2005) p. 79.

[322] Bartholomew (1998) p. 249.

[323] Linton (2005) p. 450-451.

[324] Nakamura Teiri, "Seiji to jinmyaku ga karanda saikingakushan no jusho□" in *Noberusho□ no hikari to kage*. Kagaku Asahi, ed. Tokyo: Asahi Shimbun Sha, 1981, p. 134.

[325] Brock (1961) p. 144.

[326] *Ibid.,* p. 144.

[327] Lindenmann, J. "Origin of the terms 'antibody' and 'antigen'," *Scandanavian Journal of Immunology*. (1984) 19: 281-285.

[328] Hata, S. "Gakusha to shite no menboku," (1931) in Bartholomew (1998), p. 248.

[329] Kitasato, Shibasaburo. Personal correspondence. 1892. Reprinted in Mizunoe, K., ed. *Collected Papers of Shibasaburo Kitasato.* Tokyo: Kitasato Institue, 1977, p. 431.

[330] *Ibid*., 1897, p. 432.

[331] Nagaki, Daizō. *Kitasato Shibasaburo to sono ichimon*. Tokyo: Keio Tsushin, 1989, pp. 61-81.

[332] Miyajima (1931) pp. 58-59.

[333] Klöppel, Ulrike. (2008) "Enacting Cultural Boundaries in French and German Diphtheria Serum Research," *Science in Context*, 21(2): 161-180, p. 169.

[334] Ehrlich, Koch, and Wernicke's quarrels with Behring are covered in depth by several historians of microbiology; for the view that Koch and Ehrlich were essentially justified in their complaints, see Brock (1988). For a defense of Behring, see Linton (2005). For obvious reasons, I focus on his relationship with Kitasato here.

[335] Such as Latour's model. It could be argued that Koch and Ehrlich had "invested" in Behring and as such expected returns based on his success, but if a market-based model were the most accurate, and the metaphor of "shareholders" accurate, Behring would have been *required* to do so. Gift theory accounts for the monetization of gifts into commodities around the periphery.

[336] Miyajima (1935) p. 23.

[337] Miyajima (1931) and Hata (1931) agree on this account.

[338] *Nobel Lectures, Physiology or Medicine 1901-1921.* New York: Elsevier Publishing Company, 1967. It is interesting to note that, aside from obvious choices such as Ehrlich, Koch, and Roux, many of the scientists Behring *did* credit played a far less direct and more incidental role in the discovery and development of serum therapy than Kitasato.

[339] Sri Kantha, Sachi. "A Centennial Review; the 1890 Tetanus Antitoxin Paper of von Behring and Kitasato and the Related Developments", *Keio Journal of Medicine.* (1991) 40(1): 35-39.

[340] Linton (2005), pp. 450-451.

[341] Nobel Lectures (1967); to quote Behring: "Without this preliminary work by Löffler and Roux there would be no serum treatment for diphtheria."

[342] Crawford, Elisabeth. *Nationalism and Internationalism in Science, 1880-1939*. Cambridge: Cambridge University Press, 1992, pp. 67,74.

[343] See Yamagiwa, below. See also: "Mahatma Gandhi, the Missing Laureate". *Nobelprize.org.* Accessed 17 Jun 2012. http://www.nobelprize.org/nobel_prizes/peace/articles/gandhi/index.html

[344] Most notably Takano (1959) and Nakamura (1981).

[345] Linton (2005) p. 83. Quotes also from p. 83.

[346] Bartholomew (1998) p. 250.

[347] Sri Kantha (1991) p. 37.

[348] Though discussed briefly here, the controversies surrounding Yamagiwa and Kato – as well as Kitasato and several other potential Nobel candidates – are described in full in Bartholomew (1998). For Takaki, see: Hawk, Alan. "The Great Disease Enemy, Kak'ke (Beriberi) and the Imperial Japanese Army", *Military Medicine*. (2006) 171(4): 333-339.

[349] Bartholomew (1998), pp. 253-262.

[350] Hawk (2006) pp. 33-34.

[351] Bartholomew (1998), pp. 262-279.

[352] CDC. "Historical Perspectives Centennial: Koch's Discovery of the Tubercle Bacillus," *Morbidity and Mortality Weekly Report*. (1982) 31(10): 121-123.

[353] "Tuberculosis". *Nobelprize.org*. Accessed 17 June 2012. http://www.nobelprize.org/educational/medicine/tuberculosis/readmore.html

[354] Bartholomew, J.R. "Science, bureaucracy, and freedom: Meiji and Taishō Japan" in *Conflict in Modern Japanese History: The Neglected Tradition*. Princeton: Princeton University Press, 1982, p. 325.

[355] *Ibid.,* p. 326.

[356] Linton (2005) p. 195.

Chapter 5 – Tokyo, 1895-1914

Introduction: The Scientific Statesman

This chapter serves as a departure from the preceding, as the collaborations under investigation are not those in which Kitasato participated directly, but rather those of his most famous students: Shiga Kiyoshi and Hata Sahachirō. Kitasato's role in the scientific community changed from intradepartmentally-focused scientist in Berlin, to highly-involved leader in Hong Kong, to "scientific statesman" in his later career. I examine the process of this evolution: how it occurred and how it shaped the nature of the exchanges between Kitasato and his peers[357].

Hagstrom defines a "scientific statesman" as a scientist with a reputation in his own discipline so well-established that he may shift his focus on teaching, consulting for, and otherwise advising those outside his specialty without losing prestige[358]. Hagstrom notes that these scientific statesmen are often concerned with the standing of their discipline as perceived by outsiders in business, politics, and education, and Kitasato was no different. As we have seen, Kitasato's ambitions for the development of microbiology in Japan went well beyond basic research – if basic research was his only aspiration, he doubtless would have accepted one of the many offers he received for a professorship in Europe, the center of bacteriological research at the time. Kitasato felt an obligation to repay what had been invested in him, and his goal was the improvement of public health and sanitation in Japan, and ultimately, the

modernization of his scientific discipline. To accomplish these other goals, Kitasato underwent a transformation from one of Robert Koch's many renowned pupils to the "grand old man of medicine in Japan"[359].

State Agency: Negotiations within the Community

When Kitasato returned to Japan from his stay at Koch's laboratory in 1892, many of his decisions appear to be in reaction to the problems he encountered there. As mentioned in the previous chapter his mentor, Koch, was pressured by Prussian officials into publishing results regarding the newly-discovered tuberculin[360] in which he was not entirely confident. Based on these results tuberculin was hailed as a potential cure for tuberculosis; when it did not turn out to be effective, the damage to his career was irreparable. As Japan's only "world-famous" microbiologist[361] at the time, Kitasato recognized the danger for him, too, to have a career made dependent on the whims of administrators who might use his discoveries for personal gain. Compounding this was the falling-out between himself and Ogata Masanori, and the resulting feud with the Tokyo Imperial University's Faculty of Medicine. Although a position had been opened for him there as head of the department of parasitology[362], to Kitasato it was an invitation to a potentially smothering and definitely hostile environment.

Unlike Koch, when he first returned to Japan Kitasato's prestige, though considerable, was insufficient to demand his own research institute be provided for him. To this end he relied upon both personal connections, and allies who agreed with his

cause, for support. Fukuzawa Yukichi was one of the few prominent intellectuals of the era who thought freedom of scientific inquiry divorced from the state could be a benefit to the nation; the prevailing view was that they should be tightly controlled and kept clear of positions of power. He personally loaned Kitasato the funds for a small laboratory[363], the initial focus of which was on tuberculosis research and the private treatment of wealthy tuberculosis patients, profits from which – in addition to the manufacture and sale of serums in later – were sufficient to repay him within a year and continue to fund Kitasato's own pursuits[364]. As the head of his own research laboratory in 1894, when he was summoned to Hong Kong, Kitasato certainly qualified as a highly-involved leader[365].

The drawback to holding such an elevated position in the scientific community, one that Hagstrom identified in his studies of such members, is that the pursuit of highly demanding original research at the cutting edge of one's field combined with official obligations of that station to the community and establishment at large leaves little time for training students. In interviews, Hagstrom's leaders complained of having any students at all[366]. For Kitasato, forgoing students was an impossible choice. Bacteriology was an emerging field and technicians alone would lack the necessary skills to assist him, but more importantly, he could not necessarily rely on those researchers trained by the university due to the aforementioned factional rivalry. To create his own faction, Kitasato had to train his own researchers. By taking on budding bacteriologists like Kitajima, Hata, and Shiga, and dedicating crucial time to their professional development, Kitasato was taking the first step away from his position as research leader, and one

toward becoming more like his mentor Koch: a student-oriented leader, creator of the "Kitasato school" of microbiology[367].

To complete one's development as a professional bacteriologist at the time, one required a stint of training in Europe, and Kitasato's influence and personal connections were sufficient to spare his students the troubles he encountered during his own. *Ryūgakusei* studying the medical sciences in Europe traded places frequently, to sample the theories and techniques of a number of experts. The government's reasoning was clear: at the time germ theory was still controversial and Koch's postulates were but one of many explanations for human disease. (Recall that, before Kitasato's own discovery of antitoxin in blood serum, no more cures for infectious diseases were offered by germ theory than by hygiene, or even traditional Japanese medicine.) Officials did not share Kitasato's conviction in germ theory, and when his term with Koch was up, he was obliged to transfer to Munich to study under a hygienist and vacate his space for another *ryugakusei*[368]. What they had not taken into account was that Kitasato might be involved in ground-breaking research himself, not merely passively absorbing the teachings of European scientists. Kitasato refused to transfer and was almost left without funds until Koch wrote a personal letter to the Japanese Ministry of Education pleading his case. Had he not, Kitasato may never have isolated the cause of tuberculosis, or its toxin, or discovered a resistance to it after repeated injections in test subjects. Even after this discovery, Kitasato was forced to plead for another extension to work on tuberculin[369].

Kitasato assumed his own students would be contributing to important research themselves, and so as not to subject them to the same ordeals, he personally negotiated their internships. We see direct evidence of this in his letter to Behring regarding a position for Kitajima in 1897[370]; Behring, now dedicating himself to perfecting serum therapy, was one of the most prestigious scientists in Europe. Kitajima would not travel from mentor to mentor, but study directly under him for four years. When he returned in 1901 Shiga replaced him, only this time to study under Paul Ehrlich, Behring's collaborator; in 1908 Hata studied at Koch's laboratory under August von Wassermann – another associate and collaborator of Kitasato's – then switched to Ehrlich in 1909, who was now a Nobel Prize winner[371]. While Kitasato's influence cannot be demonstrated directly, it can certainly be inferred. The switch in preference from Behring to Ehrlich may also have reflected Kitasato's influence – following Behring's Nobel Prize win in 1901, after which Behring did not acknowledge Kitasato's contribution[372], Ehrlich – who shared his feelings toward Behring – would have seemed the more socially comfortable option.

State Agency: Negotiations within the Establishment

The nationalization of Kitasato's Institute for Infectious Diseases in 1899 is presented alternately by different historians as a victory or a defeat. National funding meant that he no longer had to exclusively rely on the private market and it constituted an increase in prestige, but it could also be seen as a loss of academic freedom for his

scholarly pursuits. Fukuzawa opposed the idea for that reason, but to Kitasato is was a step toward the realization of his other goals: the modernization of Japanese medical science and an improvement in public health.

The act itself was ambiguous, but Kitasato seized the opportunity to his best possible advantage. To start with, administration of the Institute was placed within the Home Ministry, rather than the Ministry of Education responsible for administrating the university. Prior to his research in Germany Kitasato had been employed by the Public Health Bureau of Japan, which fell under the former's purview – this gave him access to and personal connections with bureaucrats within the Home Ministry itself. Kitasato strengthened these connections with gifts – of the conventional, rather than intellectual kind. He frequently treated the officials responsible for his "oversight" to expensive meals, offered lavish gifts at the weddings of their children, and extended invitations to functions that included powerful members of the scientific establishment in Europe. Critics would call these tactics underhanded and "feudal" and they were not without their own cost. Socializing and networking consumed much of Kitasato's time, leaving less for research. Kitasato's rate of publication dropped precipitously from the early 1900s; his final academic publication was in 1909, following a three year gap from his last article[373]. As the increase in publication – and discovery – by his students makes evident, he sacrificed his own research, rather than the instruction of his "school", to maintain his political alliances, and so took another step away from his old stature within the community. In the end, the effect of Kitasato's choice of priorities was to transform his "national institution" into his "personal domain".

How can these scientific statesmen, as Kitasato was becoming, maintain their prestige within the scientific gift community if they are no longer contributing? Kitasato's example provides the answer: his students, and his institution. His mentorship involved the giving of knowledge, techniques, and expertise to receptive pupils – who were, in turn, obligated to reciprocate. They did so usually by acknowledgement; criticisms by some historians of Kitasato's students for unnecessary obsequiousness toward their "revered teacher"[374] represent a fundamental misunderstanding of how the scientific community operates. By praising Kitasato's guidance in print, his name continues to appear in scholarly publications, and this makes his otherwise unseen but valuable contributions visible. Gifts are inalienable; having been passed from Kitasato to Shiga or Hata, and from Shiga or Hata into the community, Kitasato is still owed a debt of obligation, and his continued inclusion at scientific conferences around the world, such as chair of the 1911 Plague Conference at Mukden, and president of the 1925 Congress of the Far Eastern Association of Tropical Medicine.

Shiga Kiyoshi: Discoverer of *Shigella*

The careers of two of Kitasato's students illustrate this general concept. His students would necessarily have completed their initial training at Tokyo University's medical school, an environment hostile to Kitasato's Institute, but here Kitasato's commitment to producing knowledge for all members of the scientific establishment – including technicians, health officials, and students – rewarded him well. While studying

medicine at the university, Shiga Kiyoshi attended a public lecture by Kitasato, and he would later write that Kitasato's charismatic and confident personality "deeply impressed" him[375]. Born in 1871 to a samurai family that lost much of their fortune in the Meiji Restoration, Shiga showed great promise as an academic in his early years and was eventually adopted by his mother's parents who funded his education. When his medical degree was complete he joined the Institute as a research assistant.

Though his original focus was on the more widespread diseases of tuberculosis and diphtheria, Kitasato redirected Shiga's research to dysentery following a large-scale outbreak of the disease in Tokyo in 1897. In that crisis Kitasato saw an opportunity for his young protégé to make his name, similar to how he had improved his considerably with the discovery of the plague bacillus during the outbreak in Hong Kong. While outbreaks of dysentery happened sporadically across Europe, especially amongst soldiers in times of war, Japan had cyclic epidemics of the disease each summer – but none on quite this scale, which saw hundreds of thousands afflicted. During the epidemic, Shiga isolated the bacillus that would be later named *Shigella dysenteriae*; in 1898 he published his findings in a German journal of microbiology[376].

Shiga was not the only microbiologist to claim to have found the cause of dysentery. Several European scientists had done so previously, though not without dissent, and Ogata Masanori published his own causative organism isolated during the same outbreak that Shiga studied. Shiga's findings did not gain wide acceptance until they were supported by American microbiologist Simon Flexner – first director of the

Rockefeller Institute for Medical Research and discoverer of the first serum treatment for meningitis – who stated in his article in *The Philadelphia Medical Journal* that Shiga's methods served as the basis for his experiments and that his findings were identical to those of the Japanese scientist[377]. Shiga's bacillus was identified as the true cause, and when the genus was named in 1930 it was called *Shigella* in his honor. For his part, Shiga did not forget this debt of gratitude – in a review article of the etiology and treatment of dysentery almost forty years later in 1936, as a well-established member of the field, he opened by heaping praise on Flexner's research[378]. And in both his original article and this one, he thanked Kitasato.

As mentioned, Shiga travelled to Germany to study under Ehrlich for four years between 1901 and 1905, where he worked on early studies chemotherapy for trypanosomiasis[379]. Though Ehrlich received his Nobel Prize in 1908 for his studies on immunity, his Nobel lecture was largely given over to the promise of this line of research – arsenic compounds in particular, he believed, would become his "magic bullet" in the fight against disease[380].

Hata Sahachirū: Discoverer of Salvarsan

It was Hata Sahachirō who found the first round of this ammunition. Unlike his predecessors, Hata completed his initial education at the newly-minted Kyoto University, but like Shiga, was drawn to the Kitasato Institute after attending one of its director's public lectures[381]. Hata's first research was on plague quarantine measures,

co-authoring several papers with Kitasato and other members of the Institute. These student-teacher collaborations served a dual purpose: lightening Kitasato's workload so that he could focus on institution-building while remaining productive, and getting his students access to prestigious publications and the recognition they could provide. He was set to travel to Europe upon Shiga's return, but upon the outbreak of the Russo-Japanese War (1904-1905) volunteered as an army doctor instead, serving a tour of duty before returning to the Institute. After studying under Wasserman for a year in 1908, Hata asked to be transferred to Ehrlich – who accepted him based on his admiration for Hata's tireless, and well-circulated, work on the plague.

In a challenge that echoed Koch and Kitasato, Ehrlich's first task for Hata was a methodical test of his technical prowess. He was to check all of the new arsenic compounds synthesized by Ehrlich's laboratory – of which there were hundreds – for their effectiveness against syphilis. Hata did him one better, and tested the compounds already tried and found ineffective – it was one of these, compound 606 (arsphenamine), that he discovered in 1909 demonstrated a remarkable anti-syphilitic property[382]. Hata and Ehrlich subjected 606 to hundreds of trials before releasing the drug for mass-production by Hoescht under the name "Salvarsan".

Remarkably, some modern histories of microbiology do not mention Hata when discussing chemotherapy, syphilis, or even Salvarsan[383]. Chemotherapy was, of course, based on Ehrlich's theories, and the idea to use arsenic compounds was entirely Ehrlich's own, but given that 606 had been written off as ineffective by his own

laboratory, it is possible or even likely that without Hata, the "magic bullet" would never have come to fruition. In echoes of Kitasato and Behring, Hata served as second author of a book describing his *own* research on arsenic compounds published in 1911 – Ehrlich, the first author, contributed only a small concluding section about the principles behind the treatment[384]. With the struggle described previously of many Japanese microbiologists to gain recognition for their discoveries in this era, Shiga may only have managed to do so by virtue of sole authorship of his original article, and by Flexner's – who never failed to credit Shiga as the original discoverer – staunch support.

Loss of State Agency: The Transfer of the Institute for Infectious Diseases

It is no coincidence that Kitasato's final publication appeared in 1912. The transfer of the Institute for Infectious Diseases in 1914 out of the Home Ministry and into the Ministry of Education – as part of Tokyo Imperial University, no less – forced Kitasato to redirect his efforts permanently out of the community and into the establishment and its periphery.

When the new, stated-funded Institute was being constructed in 1899 in the wealthy district of Shirokane, Tokyo, it was to great protest from citizens and academics alike[385]. The Tokyo University faction claimed it was unnecessary and should be incorporated into the school; the citizens dwelling nearby feared outbreaks of the deadly diseases studied therein. To counter this sentiment, Kitasato appealed directly to the public: in lectures and newspaper articles, he pointed out that such Institutes were

common in Europe, that no one came to any harm, and that it was part of the process of scientific modernization. He enlisted members of the Society for Hygiene – doctors and clinical scientists lacking a university affiliation – as allies (recall that this was the organization to which Kitasato first disseminated his plague discovery). His opponents – both political and academic – were standing in the way of progress by clinging to the centralized institutions of the past. This appeal to modernization, one of the core tenets of the Meiji government, was very effective. By embracing the Institute ordinary citizens could possess a more enlightened, modern, and international attitude than the scholars of the university – an attitude that government officials were hard-pressed to criticize.

Appealing to public sentiment, in Kitasato's case was, however, double-edged. Fukuzawa correctly warned him that this would mark him as a threat to bureaucrats and elected officials alike, and that they, too, could play at that game[386]. Though most doctors and scientists within the government supported Kitasato and his Institute, those grew fewer and fewer as Japan modernized. Developing nations tend to enlist a large number of technical experts to create infrastructure, then phase them out in favour of bureaucrats with a background in law or political science – Japan was no different. By 1914 his adversaries finally outnumbered his supporters, the latter of whom thought him "indifferent to meaningful state control"[387], and his degree of independence inappropriate for someone of his station. Government officials could not assert the same kind of influence on Kitasato as they had Koch, and that meant the Institute was not serving its purpose. With the outset of the First World War, appeals to the public to

turn against Kitasato on the basis of his lack of patriotism – even his support of German scientists, who were at that time citizens of an enemy state, was called into question.

Kitasato fought back. In response to the transfer, he resigned, which may have seemed at first like surrender, even when all members of the Institute resigned with him[388]. But the Institute and its scientists were the only people in Japan – the most prestigious microbiologists of their time – with the expertise to produce antiserums. Doctors and clinical scientists trained at the old Institute refused to join the new one out of loyalty. Facing a health crisis of unimaginable proportions, the Japanese government found army doctors who had been trained at the old Institute and ordered them to start producing antiserums or be court martialled and possibly tried for treason[389]. They did so, but what little they could supply could hardly keep up with demand, and a result they began to imported cheaper, lower-quality foreign serums[390].

Luckily, the newly established Kitasato Institute had also acquired a licence to produce antiserums. The officials responsible for granting these licenses were the Tokyo Metropolitan Police, a part of the Home Ministry, and allies of Kitasato. After the licence was issued, it needed to be signed by the Chief of the Bureau of Public Health's Section for Preventing Epidemics – one Kitajima Taichi, Kitasato's own protégé, whom he had personally sent to study some two decades ago under Behring[391]. They tested the efficacy of their antiserums against those produced by the university scientists; when the antiserums produced by the Kitasato Institute proved more effective, they published

and widely disseminated these results, damaging the university-affiliated Institute's reputation[392].

Even so, Kitasato was back where he started: relying on private funds for research. To this end he founded what would become known as the Terumo Corporation, a manufacturer of medical supplies that still exists to this day[393]. With no room for them elsewhere in the establishment, some of Kitasato's students moved abroad to the colonies of Taiwan and Korea[394]; the best and brightest, including Hata, Shiga, and Kitajima, served as deans and professors of medicine at Keio University – an institution of higher learning original founded by Fukuzawa. Kitasato would join Keio's faculty himself, and between his official duties as part of the scientific establishment, such as the 1925 Congress, his teaching, and the management of a private corporation he would never publish research again.

Conclusions

Though many of the events that surrounded it were dramatic, Kitasato's transition from researcher to scientific statesman reflects the experiences of many similar individuals who were to follow. Had he accepted a position abroad, or even the one offered by the university at home, Kitasato's obligations would have greatly decreased, and it is likely he would have remained an active researcher into his old age. The branches of his career into teaching and institution-building necessarily led away from this path, and these are considerations scientists a century later may take into

account when plotting their own professional work – particularly if this work involves becoming an agent of the state.

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[357] As mentioned in Chapter 2, James Bartholomew has already written a great deal of excellent scholarship regarding Kitasato as an administrator and his role in the modernization of Japanese science. So as not to retread old ground, the focus will be on Kitasato's role in the scientific community. For a thorough treatment of Kitasato's political role, see Bartholomew *The Formation of Science in Japan: Building a Research Tradition*, Yale University Press, 1989.

[358] Hagstrom, Warren. *The Scientific Community.* New York: Basic Books, 1965, p. 45.

[359] Severn, Milliott. "A Note Concerning the Discovery of Bacillus pestis," *Journal of Tropical Medicine and Hygiene*. (1927) 30: 208-209, p. 209.

[360] As a silver lining tuberculin was, however, useful in other ways: the extract is used in skin testing for tuberculosis infection in both humans and animals.

[361] Bartholomew (1989) p. 209. Quoting the Viscount Kiyoura Keigo.

[362] Bartholomew (1982) p. 327.

[363] Miyajima, Mikinosuke. *Teacher and Pupil*. Geneva: Sonor, 1935, p. 21.

[364] Bartholomew (1989) p. 207.

[365] Hagstrom (1965) p. 44.

[366] *Ibid.,* p. 44.

[367] *Ibid.,* p. 45. Compare this role with that of Koch and Pasteur described in Chapter 4.

[368] Bartholomew (1989) p. 73.

[369] Miyajima (1935) pp. 20.

[370] Kitasato, Shibasaburo. Personal correspondence. 1897. Reprinted in Mizunoe, K., ed. *Collected Papers of Shibasaburo Kitasato*. Tokyo: Kitasato Institue, 1977, p. 432. See Chapter 4.

[371] Nagaki, Daizō. *Kitasato Shibasaburo to sono ichimon*. Tokyo: Keio Tsushin, 1989, p. 82-129.

[372] *Nobel Lectures, Physiology or Medicine 1901-1921.* New York: Elsevier Publishing Company, 1967.

[373] See Mizunoe, K., ed. (1977) for a complete collection of Kitasato's works. Compare the 28 papers published by KItasato between 1887-1892 to the 5 published from 1904-1909.

[374] Howard-Jones, Norman. (1973). "Was Shibasaburo Kitasato the Co-Discoverer of the Plague Bacilllus?," *Perspectives in Biology and Medicine* (Winter): 292 – 307, p.300.

[375] Trofa, Andrew *et al.* "Dr. Kiyoshi Shiga: Discoverer of the Dysentery Bacillus," *Clinical Infectious Diseases* .(1999) 29: 1303–6, p. 1303.

[376] Shiga, Kiyoshi. "Ueber den Erreger der Dysenterie in Japan." *Zentralblatt für Bakteriologie, Mikrobiologie und Hygiene.* (1898) 23: 599–600.

[377] Flexner, Simon. "On the etiology of tropical dysentery," *Philadelphia Medical Journal* (1900) 6: 414–21.

[378] Shiga, K. "The trend of prevention, therapy, and epidemiology of dysentery since the discovery of its causative organism." *New England Journal of Medicine.* (1936) 215: 1205–11.

[379] Nagaki (1989) p. 91-103.

[380] *Nobel Lectures, Physiology or Medicine 1901-1921.* New York: Elsevier Publishing Company, 1967

[381] Nagaki (1989) p. 112-129.

[382] Volansky, Rob. "Paul Ehrlich: the man behind the 'magic bullet'," *Hematology/Oncology Today*. (2009) May: 40.

[383] E.g., Plesset, Isabel. *Noguchi and His Patrons.* Rutherford: Fairleigh Dickinson University Press, 1980, p.126.

[384] Ehrlich, Paul and Hata, Sahachiro. *The experimental chemotherapy of spirilloses*. New York: Rebman, 1911.

[385] See Bartholomew, James. "Science, bureaucracy, and freedom: Meiji and Taishō Japan" *in Conflict in Modern Japanese History: The Neglected Tradition*. Princeton: Princeton University Press, 1982.

[386] For a detailed review of the transfer of the Institute for Infectious Diseases and its implications, see Bartholomew (1989) pp. 201-212.

[387] Bartholomew, James. "The "Feudalistic" Legacy of Japanese Science," *Science Communication*. (1985) 6(4) 350-376, p. 369.

[388] Miyajima (1935) p. 32-34.

[389] Bartholomew (1982) p. 339.

[390] Bartholomew (1989) p. 207.

[391] *Ibid.,* p. 207.

[392] *Ibid.,* pp. 208-209.

[393] *Terumo Corporation Annual Report 2011*. Accessed 17 June 2012. http://www.terumo.co.jp/archive/ar_e/2011/AR_2011_E_01.pdf

[394] Liu, Shiyung. "The Ripples of Rivalry: The Spread of Modern Medicine from Japan to its Colonies," *East Asian Science, Technology, and Society: an International Journal*. (2008) 2: 47-71.

Conclusions

Several conclusions can be drawn from the preceding case study of the career of a single scientist. The first is that the findings of the social studies of science, and the theories derived from those data, some seventy to a hundred years later are still broadly applicable to late 19th century scientific collaboration. Social theories of interpersonal interaction – Tomiyama's large and small politics[395] – also held true in this historical context. Finally, and unexpectedly, that language plays a highly influential, even decisive, role in both science and the study of scientists.

While Kitasato and Behring's collaboration – derived from concurrent studies on entirely different organisms in, initially, different directions – has been viewed previously as a "miraculous coincidence", Hagstrom's studies in *The Scientific Community*[396] readily provide a list of factors that were in their favour, to the point that collaboration between the two was not only likely, but predictable. The depiction of the scientific community as a gift community, too, predicts his colleagues' reaction to Behring's monetization of their intellectual labours, as well as the lengths to which Kitasato went to ensure that Lowson's contributions during the 1894 epidemic were recognized, and all involved parties' reactions to a rejection of an offer to collaborate for that research. Moreover, Kitasato's career serves as an example of how a scientist transitions from a visiting researcher to a preeminent statesman representing his field, and provides an answer to how a scientist may remain an active part of the gift community when he is no longer making direct contributions. Additionally, in Kitasato's

career we may observe that – although generally thought to be a product of the "Big Science" collaborations of the 1930s-40s – highly sophisticated, "modern" styles of collaboration such as adversarial collaboration existed as early as the late 1800s.

Also pronounced is the inseparability of small, interpersonal politics, and politics on the national stage. Lowson's decision to favour Kitasato, and vice versa, and Yersin's to decline were readily predictable along national lines: the Anglo-French rivalry, and the Anglo-Japanese alliance in the offing. But Lowson and Kitasato were also highly sociable sportsmen; Yersin preferred solitude and study. Koch and Pasteur, too, differed in personality and in their approaches to the application of their scientific discipline; still, "a victory for France" was all Pasteur wanted when they quarreled, and Koch rankled over perceived slights against Germany, not his scientific method. Similarly, a falling-out may have led Kitasato to prefer to send his students to Ehrlich over Behring in his later career, but Behring's "large" politics may have at this time drifted further from Kitasato's own, as well.

That socio-politics influence scientific collaboration is widely known, but what was more surprising in this historical case study was the profound influence of language – not only on the collaborations themselves, but how they are perceived. A superior grasp of German, and a functional knowledge of English, gave Kitasato better access to the British doctors in Hong Kong. It also made him far more desirable to them in the sense that he could, and was willing to, give interviews and lectures and ultimately publish his results in their language. While most of the grand discoveries of the day

were published in French and German, the discovery of the cause of the infamous Black Death could be published in the English-language Lancet if Kitasato was the one who made it – not Yersin. And the lack of a common language may have been the deciding factor in Yersin and Kitasato's non-cooperation, while Kitasato's fluency in German would have made him a more desirable research partner In Koch's lab than a *ryūgakusei* with a poorer grasp of their tongue. At least one dramatic quarrel between Koch and Pasteur was due entirely to miscommunication; conversely, Behring's knowledge of French gave him a considerable advantage in keeping abreast of the latest bacteriological theories from Pasteur's laboratory. Flexner's support of Shiga's discovery of dysentery in English – in addition to the original German – doubtless aided in his recognition.

Finally, though it was sheer historical accident that the 1894 plague epidemic – the first after the advent of microbiology as a modern scientific discipline – took place in Hong Kong, that particular happenstance has shaped how the English-speaking world views the accomplishments of Kitasato. Given that a cure for the disease would not be available until much later, and that Yersin was present to make the same discovery if Kitasato had not, his discovery of antiserum with Behring and his institution-building in Japan were undoubtedly his more important contributions. Indeed, when one examines the Japanese-language and German-language materials available on Kitasato, the focus is on the founding of microbiological research in Japan and his discoveries as part of Koch's laboratory, respectively.

A review of English-language obituaries of Kitasato reveals that this used to be the case in English, as well. These obituaries appear in *The British Medical Journal*[397], the *Proceedings of the Royal Society of London*[398], the *American Journal of Public Health*[399], the *Bulletin of the New York Academy of Medicine*[400], *The Canadian Medical Association Journal*[401], *The Journal of Pathology and Bacteriology*[402], and *Science*[403]. Some were written by his English-speaking pupil, Miyajima Mikinosuke, but others were written by his peers, such as cancer researcher Francis Carter Wood and scientist-historian William Bulloch. Without exception, the bulk of these overviews of Kitasato's life are dedicated to his discoveries at Koch's laboratory, though Miyajima's also convey his contributions to Japanese medical research in great detail. With one exception, his discovery of Bacillus pestis is given at most several lines and is presented as uncontroversial fact. The sole exception – Bulloch's lengthy obituary of Kitasato in *The Journal of Pathology and Bacteriology* – is a response to Lagrange's challenge, reopening the controversy nearly three decades after Sata and Yabe had published conflicting accounts (at a time when Kitasato had largely retired from research and was almost, as Linton puts it for those challenges to Behring's role in the discovery of serum therapy, "safely dead".[404]) Bulloch is not only aware of Lagrange's publication, but those which it cites: Kitasato's challenges from his countrymen[405]. To Bulloch, in other words, Japanese medical research was not homogenous.

Yet even in these obituaries we begin to see the historical tropes that would characterize Japanese scientists in the coming decades. Wood writes that: "While his name will always be associated with some of the most important discoveries in

bacteriology, his achievements were due more to industry and skilled laboratory technique under the tuition of his great teacher, Dr. Koch than to great original genius. Therefore he cannot be ranked intellectually with Koch, Pasteur, or even Loeffler and von Behring, who made the great fundamental discoveries in the golden era of bacteriology"[406]. Kitasato was a skilled technician and tireless worker; the genius behind his discoveries belonged to his European peers – the same description would be applied to Hata, and Ehrlich. Yamagiwa's discoveries would be seen as industrious derivatives of Fibiger. To be sure, Koch and Pasteur are widely regarded as the founders of microbiology, but Löffler and Behring? Kitasato was Behring's collaborator, not his technician, and it was Kitasato's creativity, not his hard work, that enabled him to solve a problem Löffler could not and isolate the tetanus bacillus.

So why would Wood choose to describe Kitasato this way? Likely because that was how Kitasato himself, and several of his students, would describe him: not particularly brilliant, but dedicated and hard-working, owing all to his mentor. What Wood and, perhaps others, fail to realize is that this is a cultural ideal, not necessarily reality. One that is at odds with anecdotes from Kitasato's life, wherein he was reprimanded by Koch for poor attendance at the laboratory[407], and spent much of his time during his years at medical school playing sports and socializing[408]. Cultural nuances such as these, even with a shared language, are difficult to convey; therefore it would be interesting to investigate whether or not, in this era of English as the *lingua franca* of science, the profound influence of language has diminished. Perhaps the language of science is not so universal after all.

[395] Tomiyama, Ichirō. "Spy: Mobilization and Identity in Wartime Okinawa" in *Senri Ethnological Studies*. 51, 2000.

[396] Hagstrom, Warren. *The Scientific Community*. New York: Basic Books, 1965,

[397] Miyajima, Mikinosuke "Baron Shibasaburo Kitasato," *The British Medical Journal.* (1931) 1:1141-1142.

[398] Bulloch, William. "Shibasaburo Kitasato,"*Proceedings of the Royal Society of London*. (1931) 109(764): xi – xvi.

[399] Anonymous. "Shibasaburo Kitasato, M.D.,"*American Journal of Public Health*. (1931) 21(9): 1030-1031.

[400] Wood, Francis Carter. ""Shibasaburo Kitasato," *Bulletin of the New York Academy of Medicine*. (1931) 7(7): 545-547.

[401] A.G.M. "The Late Baron Shibasaburo Kitasato," *The Canadian Medical Association Journal*. (1931) 25(2): 206.

[402] Bulloch, William. "Shibasaburo, Baron Kitasato," *The Journal of Pathology and Bacteriology*. (1931) 34(5): 597-602.

[403] Miyajima, Mikinosuke. "Shibasaburo Kitasato," *Science.* (1931) 74(1909): 124-125.

[404] Linton, Derek. *Emil von Behring: Infectious Disease, Immunology, Serum Therapy.* Philadelphia: American Philosophical Society, 2005, p. 77.

[405] Bulloch (1931) p. 600-601.

[406],Wood (1931) p. 547.

[407] Anonymous.(1931) p. 1031.

[408] Bartholomew, James. *The Acculturation of Science in Japan: Kitasato Shibasaburo and the Japanese Bacteriological Community,* 1885-1920. Los Angeles: Stanford, 1971, p. 31.

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