Chapter Six

Debut on the National Stage

In October 1877, the Sternbergs returned to the quiet garrison life of Fort Walla Walla. Transient officers, such as Dr. George M. Kober, were still welcomed. Kober, a 27-year-old German immigrant who had served in the army as a hospital steward and worked for Lieutenant Colonel Woodward in the Surgeon General's Office while he attended Georgetown Medical School, became a contract surgeon in July 1875. Kober became acquainted with the Sternbergs when he passed through Fort Walla Walla on his way to the field hospital at Camp MacBeth, Kamiah, Idaho Territory. Their friendship was instantaneous and—as will be seen—enduring.[1]

Preliminary experiments to test the efficacy of disinfectants, which Sternberg began in 1876, continued as well as efforts to refine his photomicrographic skills in an upstairs bedroom he had converted into a photographic gallery.[2] In a letter to the surgeon general's office in December 1878, he requested a heliostat and two objectives for his microscope, and stated he had become a "good practical photographer."[3] It is unknown whether Sternberg attempted to produce photomicrographs of bacteria at Walla Walla. Robert Koch had accomplished that feat using the anthrax bacillus earlier in 1877 in the small town of Wollenstein in Prussia. Koch's paper explaining his techniques and demonstrating his results was not published until November in *Beiträge zur Biologie der Pflanzen*. Although it is unlikely Sternberg was a subscriber, he probably knew of Koch's work through Joseph Woodward, a skilled German linguist and one of the premier photomicroscopists in the world, at the Army Medical Museum.[4]

Sternberg's interest in disinfectants began in Kansas and was broadened by experiences with sanitary measures during the yellow fever epidemics at Fort Barrancas. Also, Joseph Lister's method of antiseptic wound treatment with carbolic acid remained an active topic of discussion in the medical profession at large and in the literature, and it very likely played a role in his continued interest in the preventive and therapeutic value of disinfectants. Apparently, Sternberg never seriously

experimented with Lister's technique. He did apply carbolic acid dressings to the wounds of the Clearwater casualties and left gunshot wounds open with the long ends of ligatures on blood vessels dangling from the wound. Regrettably, he left no comments on the benefit of Lister's method. This is interesting because Sternberg, with his deep interest in the most current developments in medical science and particularly the germ theory of disease, did not attempt to validate the technique, or at least report on his experience with it. Sternberg may have attempted his own case series using the procedure at Fort Walla Walla and obtained poor or equivocal results that he regarded as unreportable. If he could not reproduce Lister's results, he may have decided the method was more trouble than it was worth and lost interest in it to address disinfectants on a broader scope.[5]

Field duties notwithstanding, Sternberg also remained focused on the study of yellow fever during his tour in the northwest. His last paper concerning the yellow fever epidemics at Fort Barrancas, "A Study of the Natural History of Yellow Fever, and Some Remarks upon the Treatment Based upon the Same; with Cases and Tables of Observations upon the Temperature and Urine," was published in March 1877.[6] His photomicrographic work and expanding interest in disinfectants, like his ideas concerning the germ theory of disease, were not separate endeavors, but vital and interrelated pieces of the yellow fever puzzle. Sternberg was convinced that he could assist in solving the mystery of yellow fever etiology and develop preventive modalities through continuing research. Through experience in four epidemics, publication in the literature, and involvement with the American Public Health Association (APHA), he had established himself as an authority on yellow fever not only in the army, but also in the civilian medical community. Whether he perceived his rise in professional status among his medical peers in the east or from the distant confines of Fort Walla Walla is not apparent, but events in the Mississippi River Valley in the summer of 1878 placed Sternberg at the forefront of yellow fever investigations and research in the United States.

On July 12, 1878, the first official case of yellow fever was reported in New Orleans. In the ensuing weeks, an estimated 40,000 citizens of the Crescent City fled northward by wagon, steamer, and rail. This massive migration—and the disease it brought with it—overwhelmed the quarantines enacted against New Orleans by other cities and smaller communities along the way. By the time the epidemic ended in November, cases and deaths were reported as far north as Gallipolis, Ohio. It was estimated that more than 100,000 cases and 20,000 deaths occurred in 200 communities. The broad swathe of devastation left by the epidemic cut through all levels of society and generated a sense of helplessness and frustration on a large scale. Existing control measures had been inadequate, and immediate action was demanded at the national level. Under this pressure, Congress was forced to address not only the current system of quarantine, but also the growing agitation for public health reform.[7]

The public health movement in the United States was glacially slow. Some quarantine and sanitary regulations had been known since the colonial period, but these enactments were generally instituted during times of epidemic disease

and rapidly forgotten when the threat had passed. Health, like most social issues in 19th century America, was essentially local. Public health reform would not have a nationally oriented champion until the establishment of the APHA in the spring of 1872.[8]

The variability of a state or central public health system led to the creation of the APHA, and it eagerly joined in the campaign to establish a national health agency with quarantine supervision as one of its main functions. Advocates of a nationally regulated quarantine system substantiated their position based on the Federal government's constitutional right to regulate commerce. Those in opposition maintained such a reading of the commerce clause was a violation of states rights. Furthermore, a national quarantine would remove the authority of local health officials most knowledgeable on conditions in their cities. An ironic situation was created when political battle lines were drawn. Northeastern congressmen, directed by their states' well-paid quarantine officials and many southerners still clinging to the idea of state sovereignty, labored against a national quarantine, while southern congressmen and physicians rallied around the APHA in urging for federal control of quarantine operations. Northern interests, however, defeated or watered down the first four quarantine bills in the early 1870s. This was the situation in the spring of 1878, as Congress recessed and yellow fever docked in New Orleans aboard ships from the Caribbean and South America and drove up the Mississippi Valley.[9]

As the epidemic raged in late summer, John M. Woodworth, Supervising Surgeon General of the Marine Hospital Service, began a campaign to gain control of any national quarantine service, but APHA leadership—James L. Cabell, John S. Billings, Henry I. Bowditch, and Elisha Harris—feared him both politically and scientifically. As the November meeting approached, tensions between Woodworth and the APHA intensified.[10]

On November 2, Surgeon General Barnes directed Sternberg to be one of the Medical Department representatives to the APHA meeting. At the conclusion of the session, he was to report in person to Barnes in Washington. Barnes' selection of Sternberg and the fact that he directed him to report personally afterwards are significant. If Barnes had simply wanted an army representative experienced in yellow fever and knowledgeable on quarantine operations, it would have been easier and less expensive to send surgeon Harvey Brown. If he had wanted someone to provide him with a report of the meeting, he could have asked Billings or Woodward. Barnes believed Sternberg's experience with yellow fever would be valuable, and Barnes put special merit in his medical opinion of the proceedings. However, in light of the political tensions building between Billings and Woodworth, and the pro states rights faction of the APHA, Barnes may have been looking for Sternberg to provide a truly objective commentary of the events. Sternberg, who had worked closely with southern public health officials during the most recent yellow fever epidemics, was intimately familiar with quarantine problems along the Gulf coast. This experience put him solidly in the national quarantine camp, yet Sternberg, unlike Billings, Woodward, or Brown, was a Washington clique outsider. As such, his opinion may have carried extra weight with the surgeon general.[11]

The APHA meeting was called to order on the evening of November 19. That morning, Woodworth distributed an unauthorized program that allowed for all hypotheses regarding yellow fever causation and prevention to be aired during the convention. Such discussions caused dissension among those supporting an independent national health agency and permitted Woodworth to establish himself as the preeminent leader of the public health movement. The executive committee promptly crushed Woodworth's attempted coup . Although tensions remained, the drama of the opening session soon faded into rather anticlimactic discussions on the origin and progression of yellow fever in the south and the failure of preventive measures. After three days of intense haggling over the nature, origin, transmission, and prevention of yellow fever, a set of six propositions defining APHA's conclusions and position regarding the recent epidemic was prepared for a vote:

1. Yellow fever, in 1878, was a specific disease, not indigenous to or originating in the United States, and it was due to a specific cause.
2. Quarantine establishing nonintercourse will prevent the importation of yellow fever.
3. It is the duty of the government to establish such a quarantine.
4. It is the duty of the government to appoint and fund an expert commission to investigate the causes of yellow fever, and methods of preventing its introduction into this country.
5. It is the duty of the government to invite foreign nations to cooperate in establishing effective international quarantine regulations.
6. State and municipal authorities should ensure that local sanitary measures are attended to at all times. (All six propositions passed easily in the waning hours of the last session on November 22, and APHA adjourned for another year.[12])

Sternberg was less than satisfied with the results of the convention when he departed Richmond. The content of his conversation with Barnes did not survive, but he composed an editorial concerning the Richmond meeting for the *Medical Record* that provided his opinions. Sternberg advocated—and believed the majority of those attending the convention also did—that the APHA should be a controlling influence on urgently needed national public health legislation that included a national quarantine. He also felt the majority agreed "that yellow fever in the United States usually results from the importation of cases or fomites, and… importation can be prevented by proper quarantine restrictions. I think…a majority were of the opinion that yellow fever never originates in the United States; but no vote having been taken upon the proposition formulated by a committee…. I cannot be sure that I am right."[13] He referred to the first of the six propositions. Originally, it had stated that yellow fever was a specific disease that never originated in the United States except by importation, but since the commission had not completed its work, a compromise was reached that stated it was considered to be imported only in 1878. Sternberg was extremely displeased that the APHA had

accepted such a diluted position on the issue. His displeasure centered on the fact that "it gives color to the prevalent popular belief that the doctors know little or nothing about yellow fever, and that the late epidemic has upset all preconceived theories and opinions, and left us all afloat."[14] He also emphatically stated that the "etiology of yellow fever is as well settled as is that of typhoid or remittent fever, and that those in and out of the profession who are still in doubt as to how epidemics of yellow fever originate and progress may obtain reliable information upon the subject by consulting…standard medical works…."[15] Sternberg was convinced yellow fever was always imported and contracted from exposure to an infected area. "The facts observed and recorded by myself for four minor epidemics fully support this statement," he concluded, "and the matter is so thoroughly settled that in future investigations,…we should turn our attention to the discovery of the unknown special cause [of the disease]."[16] Although Sternberg's pronouncements seemed brash, he did not miss the mark. More importantly, he was truly sparing in the ring of medical science. Sternberg's blood was up, and Levi must have been extremely proud.

On December 2 in his annual address to Congress, President Rutherford B. Hayes urged "that Congress give the whole subject [of quarantine and public health] early and careful consideration."[17] Faced with an expectation to do something, Congress created the National Board of Health (NBH), but gave it little or no real power. The first official meetings of the NBH were held in early April in the Army Medical Museum in Washington, DC. Officers were elected—James L. Cabell became president, John Shaw Billings became vice president, and Thomas Turner became secretary—and standing committees were organized. Without power or money, the membership determined that the board's duties would be cooperative and advisory, and they established three objectives for the coming year:

1. the institutionalization of scientific investigation and collection of public health information,
2. the advisement to various governmental branches, and
3. the submission to Congress of a plan for a permanent NBH.

Also discussed in earnest was the organization of a commission to study yellow fever in Havana, Cuba. The act creating the board authorized it to "make…such special examinations and investigations at any place or places within the United States, or at foreign ports, as they may deem best."[18] The board organized the First Havana Yellow Fever Commission over a 3-week period, and Congress supported the effort with a $13,000 appropriation. The following designations were made: Stanford E. Chaille as chairman, Thomas S. Hardee as sanitary engineer, Juan Guiteras as pathologist, Henry Mancel as official photographer, and Rudolph Matas as clerk. Early on, the board decided a bacteriologist was essential to the investigation's success, and, by law, it was permitted to request the loan of personnel from other governmental branches. The executive committee—most probably at the urging of Billings—lost no time in securing Sternberg as secretary and bacteriologist for the commission.[19]

Sternberg received orders to report to the surgeon general on April 18, but his new assignment was not specified. However, by the time he and Martha journeyed eastward, he must have had some idea of what awaited him. Mrs. Sternberg commented that he knew "that he would be able there [in the east] to pursue under favorable conditions the scientific and sanitary research on which he was engaged."[20]

At the initial planning sessions in Washington, the objectives of the commission, equipment and training requirements, and other operational details were discussed in depth. Between July and October, the commission was to do the following:

1. "ascertain the…sanitary conditions of principal ports in Cuba…to determine how…sanitary conditions can best be made satisfactory, and …what can and should be done to prevent the introduction of the cause of yellow fever into the shipping of these ports,"
2. "increase the existing knowledge of the pathology of yellow fever," and
3. "obtain as much information as possible with regard to the…endemicity of yellow fever in Cuba, and the conditions which may…determine such endemicity."[21]

Chaille and Colonel Hardee would address objectives 1 and 3, Guiteras would concentrate on objective 2, and Sternberg was tasked with the additional problems relating to yellow fever. It was an ambitious task with the time and money available, and the board members made it clear that they did not expect a complete investigation.[22]

Sternberg must have been pleased with his assignment. For the first time in his army career, he had been recognized for his abilities as a medical scientist. He had been given carte blanche to conduct yellow fever research in any direction he considered appropriate, and the most modern equipment had been procured for him to do so. Sternberg's assumptions were that yellow fever had a bacterial or fungal origin that acted on the blood to change its constitution and, therefore, he determined the first line of inquiry would be the examination and culture of blood from yellow fever patients. Then transmission experiments upon lower animals would be performed, and a complete examination would be conducted of the water and air of Havana. If the disease was an organism visible under the microscope, Sternberg was confident that he could find, culture, and photograph it.[23]

The commission arrived in Havana on July 7. The San Carlos Hotel, overlooking the harbor, had been selected to provide office and laboratory space because of its moderate price—$100 a month for five rooms—and because H. C. Hall, the U.S. Consul-General, resided there. Captain-General Don Ramon Blanco, the Spanish governor of the island, welcomed the commission and declared Spain's enthusiastic support for its work and his personal assistance with its mission. In that regard, carpenters configured two rooms in the San Carlos to Sternberg's specifications for photographic purposes. Blanco also appointed a 12-man auxiliary commission that included, among others, Dr. Carlos Finlay, to assist the commission locally and form a permanent organization in Cuba to continue yellow fever studies.

He also assured the commission that there was no dearth of yellow fever cases at the San Ambrosio Military Hospital.[24]

While Chaille gathered information on port sanitation, commerce, and the endemicity of yellow fever on the island and Guiteras began pathological examinations in the morgue, Sternberg collected blood from patients at the military hospital. At the end of the first week, he wrote Martha that he was "working away in the laboratory and had commenced some experiments."[25] As promised, plenty of yellow fever cases existed, but problems—over which he had no control—emerged and caused him anxiety. In a letter to John Shaw Billings, he confided his impatience with the constant parade of Cuban physicians through his laboratory that delayed his work, Guiteras' slow and unaggressive nature—a remark he would later retract—and how Matas was an industrious, but inaccurate clerk.[26] Although these comments sound like those of an obsessive-compulsive scientist incapable of understanding anyone less gifted or dedicated, they were merely the symptoms of larger personal issues. He had been ill for several days, and it delayed his work. Moreover, it intensified his own anxieties concerning his abilities to find the yellow fever germ in the short time allotted.

In one of his more prophetic statements, Sternberg told Billings, "I am satisfied that the man who succeeds in solving the problems connected to yellow fever must devote himself to the investigation not for three months but probably for years."[27] With meticulous technique, he spent hundreds of hours patiently preparing and analyzing blood smears, photomicrographs, and culture preparations. He eventually examined 98 blood specimens from 41 confirmed yellow fever cases and produced 105 photomicrographs, which Woodward praised highly later. However, he found nothing significant. Culture experiments with urine, black vomit, and infected blood produced a wide variety of bacterial and fungal growth, but none of them had any correlation to his blood smear preparations. His attempts to find anything of interest in the water of Havana Harbor or the air of the city proved fruitless, as did his experiments on lower animals. However, he still hoped that the appearance of fatty granules in the red blood cells would prove pathognomonic of the disease. He commented to Billings in late August, "If this appearance in yellow fever blood is peculiar to this disease, and if by drawing…blood & examining it by the microscope a positive diagnosis can be made at the outset of a case the discovery will be of great importance. I speak of it as a discovery as I know of no recorded observation of a similar appearance wither in the blood of yellow fever or any other disease. If you know of any such observation please inform me at once. I propose at some future time to examine the blood of other febrile diseases & especially of pernicious remittent [malarial fever]…. If in the meantime you can learn anything in relation to this matter from any source I hope that you will inform me of the fact."[28]

By late August, Sternberg's exasperation had increased with the conduct of the investigation and the commission members. He wrote to Billings on August 29, "This has been a bad week for me & I am in swearing humor today. My work had been interrupted by visitors who think nothing of spending three or four hours

with us & expect to see the photographs & look through the microscope. Some of these visitors are friends & relatives of Dr. Guiteras, some are doctors that Dr. Chaille or myself are under slight obligations to & who must consequently be treated politely. Then I have been bothered by cloudy weather & worried by heat & mosquitoes in my work room & fretted by mistakes made by the clerk in making out vouchers & exasperated by the fact that Dr. G is in Matanzas at the end of the month & I have been obliged to send him my vouchers for approval by Matas the clerk, at a time when his services were required here, etc. etc. I do not propose to make any complaints against my confreres of the Comm – but I assure you that I will not play second fiddle a second time in any future investigation of yellow fever – Nor will I serve on equal terms with a young man who has not passed the period in life when sweethearts & aunts & uncles are of primary importance. I am not in a position to order or direct & yet cannot help fretting. When I see time wasted – I must stop harping on this string. G is a clever fellow & I like him."[29] More importantly, Sternberg's hope that he would discover yellow fever's causative agent had been obliterated by a plethora of negative laboratory findings. His work continued to be "…about the same thing everyday. Going to the hospital for specimens and looking through the microscope at blood and bilge water and black vomit and urine and all those nice things."[30] He told Mrs. Sternberg, "I have not found the yellow fever germ…," but consoled himself by commenting, "…I have done good work here and think I will get credit for it with the Board of Health."[31] It was the best that could be hoped for. By the end of the month he had concluded, "If there is any organism in the blood of yellow fever demonstrable by the highest powers of the microscope as at present perfected, the photographs taken in Havana should show it. No such organism is shown in any preparation photographed immediately after collection."[32] In one of his last letters home, Sternberg's loneliness and fatigue from the tedium and monotony of the investigation was clearly apparent, "It will be a happy day for me when I reach Washington and take my dear wife in my loving arms again. I feel that I need rest and the comforts of home and the company of my dear wife. This living in a hotel and working from 7 in the morning until 10 at night gets to be an old story after awhile."[33] The commission departed Havana in early October. Sternberg returned to Georgetown to rest and write the commission report.[34]

The report, presented to the NBH at the APHA meeting in Nashville, Tennessee, on November 18, was well received. It described the following:

1. principal Cuban ports,
2. the amount of intercourse these ports had with American cities and its correlation with annual yellow fever activity,
3. yellow fever endemicity on the island with a description of unsanitary conditions in major harbors and cities, and
4. Sternberg's laboratory investigations and Guiteras' pathological work.

The commission had done excellent, comprehensive epidemiological work and offered recommendations to correct sanitation, and it suggested an international

sanitary conference be organized among nations who traded with Cuba. Although Sternberg's ego had been bruised by not finding the yellow fever germ, the NBH had been impressed with his laboratory methods and results, and it wanted him to continue with yellow fever research and other projects. Furthermore, the board had another medical corps officer on loan from the surgeon general, Captain Charles Smart, who had been assigned to perform "chemical and microscopical work connected with sanitary investigations."[35] To employ them both productively, the NBH established a laboratory in the building it rented in Washington at 1410 G Street, NW. Sternberg and Smart now had a scientific home in which to perform their duties.[36]

In December 1879, Sternberg was directed to pursue investigations on the value of gaseous and volatile disinfectants and to examine airborne dust particles for the presence of microorganisms. An ever-increasing array of chemicals claimed to have disinfectant properties was being used by public health officers and surgeons to preclude or halt widespread epidemics and local infectious processes. The precise nature, mode of action, and true practical value of these agents, however, were unknown. In his earlier work on disinfectants, he had noted the methodological dilemma of obtaining truly pure cultures in liquid media and then finding a reliable technique to appropriately evaluate the effect of an agent on an organism. For his experiments, Sternberg constructed an air chamber in which he could expose bacteria to various concentrations of disinfectant gases, such as chlorine, ammonia, and sulfuric, carbolic, and nitric acid. A microscope mounted on the chamber allowed him to observe the bacteria for cessation of motility during exposure. He also made the same tests using smallpox vaccine. Surprisingly to medical personnel today, after exposing doses of vaccine to sulfuric acid gas, he and Dr. Smith Townsend, Health Officer for Washington, DC, rubbed exposed and unexposed vaccine into the scarified arms of children from the public institutions in the capital. With but a few exceptions, all of the vaccine exposed to the gas failed to produce the expected vesicles, while the unexposed vaccine gave the usual reaction. Sternberg concluded, "Exposure for 6 hours or more to an atmosphere containing at least 1% of sulphurous acid gas, chlorine, or nitrous acid gas, is a reliable method of disinfection."[37] As for carbolic acid, he determined that it had no disinfecting capability—either in the gaseous or crude solid form—in the concentrations currently being used by public health officials and surgeons. Although historians have given Koch much acclaim for initiating comparative studies of disinfectant effects on certain bacteria and destroying the belief that carbolic acid had any therapeutic value, Sternberg's experiments predated Koch's work by at least a year.[38]

These experiments, in retrospect, were simple, relatively easy to accomplish, and provided the sought-after information in relatively rapid time. Determining the microbial content of the air and its significance would not be so simple. Sternberg had read the studies on atmospheric dust in relation to cholera and dysentery in India by Royal Army Surgeon David Douglas Cunningham and Pierre Miquel's similar studies conducted in Paris. He found "no gross or conspicuous germ or organism…in the air of infected localities" in Cuba, but had identified considerable

numbers of acicular and prismatic crystals.[39] Although these crystals were enigmatic, no one knew what the normal composition of microbes, crystals, and so forth in free air was or what happened to them once inspired into the lungs. With this in mind, the board had distributed small wooden boxes containing two glass slides to many of its members after a meeting in October so they could collect dust from their homes. The boxes were returned to Sternberg by mail. He examined all specimens closely and made an interesting discovery. Six pairs of the glasses had been exposed in rooms occupied by yellow fever patients in various areas of Louisiana. All 12 slides contained a large number of radiating acicular crystals exactly like he had found in the military hospital in Havana. In addition, slides from New Orleans demonstrated prismatic crystals, also similar to crystals found in the air of Havana, but specimens from Washington, DC, Philadelphia, Boston, Mobile, and Bellevue Hospital in New York City did not. With nothing else to go on, and probably at Sternberg's urging, the board sent him to New Orleans in early February.[40]

Sternberg arrived in New Orleans with instructions not only to examine the air of the city and blood of yellow fever patients for evidence of crystal formation, but also attempt to repeat the recent joint experimental work of Doctors Edwin Klebs and Corrado Tommasi-Crudelli on malaria. They claimed to have isolated the etiological bacteria of malaria, *Bacillus malariae*, the previous summer from the Pontine marshes near Rome and reproduced the disease in laboratory rabbits. The scientific community had generally accepted the high reputations of both investigators—Klebs as a bacteriologist and Tommasi-Crudelli as a malariologist—and their careful laboratory work. The NBH reasoned that if a swamp-dwelling bacterium caused the disease, then Sternberg should be able to find it in the malarious environs of New Orleans.[41]

From February until well into May, Sternberg exclusively pursued the investigation of suspended particles in the air. The results obtained left him so unimpressed that he did not feel justified in publishing them until January of the following year. Although no common microbe from infected atmospheres was demonstrated, Sternberg felt that more extended researches should be performed before a negative result was accepted. Furthermore, he commented, the "possibility of the existence of organisms morphologically alike, but differing in the physiological action must be borne in mind in investigations relating to the etiology of disease."[42] He concluded the most important result—and one that agreed with the observations of Cunningham, Miquel, and others—was that "bacterial organisms are not found in the atmosphere, even in crowded cities in a southern latitude…during summer months, in any considerable number…and consequently that the method by direct examination…does not give promise of definite results in regard to the supposed relation of these…organisms to the prevalence of epidemics."[43]

By May, Sternberg was ready to let the dust in the air settle and prepared to address the malaria question. He obtained and analyzed in detail a translation of Klebs' and Tommasi-Crudelli's report, "Studi sulla Natura della Malaria." They had isolated a bacillus that, when injected into rabbits, produced a cyclic temperature curve, enlarged spleens, and black pigment in their blood and various organs. Sternberg initiated his experiments by mixing mud gathered from suburban marshes with distilled water and

subcutaneously injecting varying amounts of this solution into rabbits in his laboratory. However, he—not the rabbits—became ill as May progressed. He requested a leave of absence to travel north for his health in May and did not return to New Orleans until September 2.[44]

Sternberg had hardly reopened his laboratory when his old friend and NBH representative in New Orleans, Dr. Samuel Bemiss, contacted him with rumors of a yellow fever outbreak 52 miles south of New Orleans in Plaquemines Parish. Dr. J. B. Wilkinson, "the oldest and most experienced physician on the lower coast," according to Bemiss, had diagnosed six cases—four of them fatal—in the Giordano family and advised residents to remove their unacclimated children from the area.[45] Bemiss had wired the NBH, Dr. Joseph Jones, president of the State Board of Health of Louisiana, and physicians in Plaquemines Parish immediately to verify the rumor. He also offered financial assistance from the NBH to assist in precluding the spread of the disease. Jones replied that Dr. B. N. Taylor and other physicians in the area did not believe yellow fever was circulating, but rather only mild malaria, and directed inspections and sanitary precautions be instituted as required. However, Jones' response did not convince Bemiss of the absence of yellowjack along the lower coast.[46]

Relations between the two men and the public health agencies they represented had never been cordial. The National Quarantine Act of June 2, 1879 had given the NBH quarantine authority over states that failed in these duties. Jones, an unreconstructed Confederate, became president of the Louisiana State Board of Health on April 8, 1880, and resented what he considered federal interference in state matters. He saw himself as the champion of public health in New Orleans and the defender of the city's commerce against federal incursion, but undoubtedly enjoyed the income and political patronage that came with quarantine control. During the summer, Jones' relationship with Bemiss and the NBH deteriorated dramatically. NBH's failure to provide financial aid requested by the Louisiana Board of Health over the past year, and its desire to shift primary quarantine operations from Mississippi River Station—65 miles below New Orleans—to Ship Island Station off the coast of Mississippi led to state and federal difficulties in coordinating and maintaining an effective quarantine along the Gulf coast. In late 1879, the NBH reported that Mississippi River Station was in the least desirable location possible. It could not preclude communications with New Orleans or the inhabitants along the river in Plaquemines Parish, infected ships could not be segregated from noninfected ones, hospital facilities were inadequate, and mosquitoes were rampant. Jones maintained the state-board-operated station provided appropriate protection to the Mississippi Valley. In July, with an eye toward public health power in the Mississippi River Valley and New Orleans commerce, he requested $10,000 from the NBH for needed repairs and equipment, and the same amount to be provided to the state board for use in an epidemic. The request was denied, Jones declared, because he had not agreed with the federal agency in establishing the primacy of the Ship Island Station. As Bemiss and Jones wrestled with this issue, an erroneous message was released stating that the NBH reported 11 yellow fever deaths had occurred in New Orleans. Jones was sure the NBH had

orchestrated the incident to force the Ship Island Station issue. Although amends were made, the damage had been done. Now Bemiss could not be sure that Jones was not hiding a yellow fever outbreak just to maintain trade in the Mississippi Delta and his own sense of power. He needed Sternberg to visit the lower coast and render a verdict.[47]

On September 7 or 8, Sternberg conducted a field investigation at Pointe Michel and Pointe à la Hache in the company of two local physicians, Doctors Hays and B. N. Taylor. He visited 20 cases of the prevailing fever and found a total of 65 cases and six deaths had occurred. Most of the cases had been young children in the predominantly French Creole population. He described the disease as "a continued fever of single paroxysm, lasting…from a few hours to four or five days. No regular temperature observations have been made, but from the statements of Dr. Hays, and my own observations, I am satisfied that the fever is a mild grade, and not characterized by remissions or intermissions."[48] Only three cases demonstrated any hemorrhaging, and while only Dr. N. M. Hebert's fatal case demonstrated albumin in the urine, Sternberg found only three such cases during his visit. Although six deaths had occurred, he diagnosed the outbreak as one of benign, abortive, or incomplete yellow fever. He concluded in his September 10 report to Bemiss, "For me the fever is identical to yellow fever, and only differs in degree from the more severe forms…. It seems…extremely unscientific to make our diagnosis depend upon a greater or less percentage of mortality, and the sooner physicians in the yellow fever zone, admit…that yellow fever is not always a malignant disease…that the immunity of creoles is due to their having suffered (generally in childhood) from this milder form of the disease…and that it is not a birth right, the better will it be for the progress of medical science and the true interests of commerce where these diseases prevail."[49]

Bemiss was pleased with Sternberg's work, sent his report to Jones on September 13, and again reminded him that he was authorized to draw up to $10,000 from the NBH coffers for preventive measures. If Bemiss thought Jones would now subvert his political position by accepting federal dollars previously refused, he was sadly mistaken. Jones curtly replied the same day, "The communication…has been received and noted. The board of health of the State of Louisiana has investigated the malarial fever…. Such measures as the board of health deem necessary have been instituted."[50] Apparently, no report of this supposed investigation was forwarded to Bemiss, but he did receive a large dose of criticism from Jones concerning Sternberg's opinion of the outbreak. The state board's president made it clear that he considered the investigation a deliberate attempt to create a panic. Bemiss was furious and decided to repeat the investigation with a composite team of state board of health and NBH members. The committee on fever on the lower coast consisted of Doctors J. Dickson Bruns and J. P. Davidson, both of the board of health; Doctors Robert W. Mitchel and Sternberg, representing the NBH; and Mr. H. D. Bruns, son of Dr. Bruns, who would perform any required autopsies.[51]

Just before noon on September 15, the committee docked at Myrtle Grove, the plantation of Dr. J. B. Wilkinson. For the next two days, committee members

tramped about the lower coast between Pointe Michel and Pointe à la Hache seeing convalescent patients in the company of Doctors Hebert, A. B. Hays, and Westerfield. The local physicians were extremely accommodating, but Sternberg noted that their clinical histories were less than robust, urine testing for albumin was performed haphazardly, and temperature charts were nonexistent (Hays was the only physician to own a thermometer). Bruns reported upon returning that the illness was "an endemic malarial fever, of remittent type, and…of a mild character. Its unusual prevalence is due partly, to the meteorological conditions of the past summer, and partly…to the widely increased cultivation of rice."[52] As to the nature of the malady, he stated "neither in its special features nor in their entirety, could I realize a single prominent characteristic of yellow fever."[53] As to the deaths, he attributed them to noncompliance in taking the prescribed medication, quinine.[54]

Sternberg, unconvinced that malaria alone had generated all of the fevers and deaths noted, provided a minority report in which he said that nothing he had observed during his second tour of the lower coast or anything he had read in Bruns' report induced him to change his opinion of the outbreak. Although he admitted he had not seen any single case that enabled him to positively diagnose yellow fever, he believed—nonetheless—the origin and progress of the outbreak supported his conclusions. The first fever cases had occurred in Westerfield's practice, directly across the river from the quarantine station where the bark *Excelsior*, which was infected with yellow fever, had anchored from mid-July until mid-August. Westerfield saw his first case on August 1. Hays began to have an abundance of cases in mid-August, and, in early September, Hebert began to treat the same fever several miles north of Hays' area of practice. Additionally, Westerfield noted this fever was the same as in 1878 and did not recall that any of the current epidemic victims had the fever then.[55]

Sternberg was adamant that the theory of the fever being malarial and emanating from the local rice fields was erroneous. Even though he agreed some cases were probably malarial, the natural history of the outbreak did not support an exclusively malarial diagnosis. Adults were exposed to the rice fields on a daily basis, but children suffered the most from this outbreak. Little of the fever was seen in rice-growing areas, yet some cases occurred in an area devoid of rice cultivation, but near a custom house station where infected ships docked before disinfection. Dr. Wilkinson, Sr., who was considered a most experienced and competent physician by his colleagues and was familiar with the presentation of endemic malarial and yellow fever seen on the lower coast, consulted on many cases in Hays' and Hebert's practices. He also concluded that it was continuous and definitely was yellow fever. Wilkinson's assessment notwithstanding, the majority of New Orleans physicians closed ranks with Jones and the state board of health in upholding the malaria diagnosis. They declared the NBH had intentionally tried to cause a yellow fever panic and considered Sternberg's knowledge of yellow fever in Louisiana as "tenth rate."[56] Crescent City newspapers added their weight to this public flogging by painting the NBH representatives as inept scoundrels, and Sternberg as an "ignorant charlatan, unfitted for his position."[57]

Sternberg returned to the mud of the river batture and Lafayette and Congo squares in New Orleans. He collected a few buckets of ooze, ladled it into terrariums he constructed in his laboratory, and observed these artificial marshes for microbial growth over the next six weeks. By the end of November, he had observed and photographed numerous organisms from his terrariums and injected a total of 37 rabbits with mud solutions and, as controls, saline and his own saliva. Ten rabbits had become ill and died, but he found little evidence that they had succumbed to malaria. Sternberg concluded: "Among the organisms found…are some which closely resemble and, perhaps, are identical with… Bacillus malariae…but there is no satisfactory evidence that these, or any other of the bacterial organisms…when injected beneath the skin of a rabbit, give rise to malarial fever.… The evidence upon which Klebs and Tommasi-Crudelli have based their claim of…discovery…cannot be accepted as sufficient; (a) because in their experiments and in my own the temperature curve in the rabbits…has in no case exhibited a marked and distinctive paroxysmal character; (b) because healthy rabbits sometimes exhibit diurnal variations of temperature as marked as those shown in their charts; (c) because changes in the spleen…are not evidence of death from malarial fever…as similar changes occur in the spleens of rabbits dead of septicemia produced by the sub-cutaneous injection of human saliva; (d) because the presence of dark-colored pigment in the spleen cannot be taken as evidence of death from malarial fever…as this is frequently found in the spleen of septicemic rabbits."[58]

After weeks of diligent and intensive experimentation, Sternberg could only state that, from his point of view, the results of Klebs and Tommasi-Crudelli were too weak to substantiate their claim. He admitted he had not found the *B malariae* in the mud of New Orleans and, more importantly, if the bacillus did exist, he could not say that it did not produce malaria in humans. The world paid scant attention to Sternberg's work in Louisiana, but it lauded the two researchers in Italy for their masterful bacteriological work. Ironically, however, his efforts to find the *B malariae* led him to stumble onto an unexpected and intriguing development. Of the 10 rabbits that died in his laboratory, he noted one had been injected with saliva and died from a "diffuse cellulitis and septicemia…."[59] Sternberg had no clue of what might exist in his own saliva to precipitate such an event, but was eager to investigate the issue in his Washington laboratory. Before these researches could begin, however, he had one more obligation to attend to in New Orleans.

APHA's annual meeting would be held in the Crescent City in early December. Sternberg had accepted a request from the APHA to prepare a presentation on yellow fever and national quarantine. The meeting provided him with the appropriate professional forum to return fire on his detractors, and he prepared his attack on the inadequacy of quarantine measures in New Orleans with skill and precision. Past experience, he declared, had demonstrated to quarantine officials that yellow fever is not transmitted person to person, but it is carried in some other fashion aboard cargo vessels. "It is evident," he declared, "that we, as sanitarians, cannot remain silent spectators of the administration of a quarantine based upon fallacious principles, and which

does not furnish any adequate protection to the people of the great Mississippi Valley, without a certain amount of criminal complicity. It is high time…this matter be thoroughly discussed…. I am strongly inclined to believe, that, for New Orleans, unrestricted intercourse and the quick dispatch of vessels from infected ports would present but few dangers beyond those annually incurred under the present system of quarantine administration. Let us have a quarantine worthy of the name, or none at all."[60] Sternberg admitted New Orleans had many issues to contend with in the execution of an effective quarantine that other cities did not. "The numerous water-ways by which the city may be approached; its extended commerce; the strenuous opposition to quarantine on the part of an interested portion of the community; the support given to these opponents by a certain number of physicians who believe that yellow fever is an endemic disease in the city; and, finally, the difficulty of obtaining an efficient administration where politics control everything,…"[61] He blasted operations at the Mississippi River Station as worthless and remarked that the continual whining about lost commerce was grossly overestimated and would never amount to the economic loss of one epidemic such as was seen in 1878. In light of the current situation in New Orleans, Sternberg defended complete non-intercourse quarantine measures, but he advocated a more practical solution to the problem. "I believe…sanitary science is…in a position to indicate methods, which, if faithfully executed, will reduce these risks to such an extent as to make a quarantine of non-intercourse unnecessary.… It is evident…we require uniformity of laws, and inflexibility in their execution; which can only be obtained by allowing the laws to emanate from a central authority, and their execution to depend upon persons removed from the domain of politics."[62] He then outlined the fundamental principles for a rational quarantine and—in so doing—defended the logic of the Ship Island Station:

1. all vessels, cargoes, ballast, passengers, crew, and baggage coming from an infected port should be considered infected and treated as such;
2. detention in quarantine for a longer time than needed to disinfect the vessel and cargo is unnecessary and unjustifiable;
3. all cargo must be removed to properly disinfect a vessel;
4. keeping passengers and crew aboard a vessel suspected of being infected upon arrival in port in order to test the question of infection is unscientific, unreliable, and inhumane practice; and
5. a quarantine station should be considered an infected area, employing only immune individuals, and should be located so that unauthorized persons cannot gain access to it.

He concluded by saying, "…I believe it as much the duty of the National Government to protect the country from the invasion of pestilential diseases as from a foreign enemy; and consequently the maintenance of such a quarantine should devolve upon it,…and this without any tax upon commerce, or upon the unfortunate people who

are subjected to detention. A great government should resort to no petty measures when she stretches out her hand to protect the people from a serious evil."[63] Sternberg's part in Louisiana quarantine politics was finished. He returned home, wrote his report on malaria, and eagerly awaited the publication of his translation of Magnin.

The English translation of Magnin's *Bacteria*, the first general text on bacteriology in English, appeared in bookshops late in 1880. This text had been an invaluable handbook for Sternberg during his experiments over the past year. His translation grew out of a desire to share this knowledge with his colleagues and fill a void in the scientific literature. Sternberg was convinced that the dearth of American literature on the subject belied the true interest of American scientists in bacteriology. A reliable textbook for experimentation was needed that also provided a foundation of knowledge by which individual American scientists could begin to correctly judge the value of bacteriological work emanating from Europe. Laudatory reviews of his translation from the *Medical Record* and the *American Journal of the Medical Sciences* indicate that his efforts to remedy this situation in an unbiased and scientific manner were well received by scientists and physicians alike.[64]

In January 1881, the United States hosted the 5th International Sanitary Conference in Washington, DC. The focus of the first four conferences revolved around discussions of cholera and international quarantine agreements to limit its dissemination. The United States had not taken part in any of these conferences, even though it was just as vulnerable to cholera as the rest of the world. Participation at this juncture was based on purely political motives covered in a thin veil of interest in international sanitary science. In response to the increased incidence of yellow fever over the past 2 years, Congress passed an act on June 2, 1879 that prevented the introduction of contagious or infectious diseases into the United States. Any vessel destined for America needed a sanitary history certificate verified by the U.S. consul in the country of origin, and this required the consul to inspect the ship. Obtaining international agreement on this piece of domestic legislation was crucial or it would be unenforceable, and hence the motive for sponsorship of the conference. Although the conference was largely an administrative exercise, the seventh session did include a prescient scientific announcement. On February 18, Finlay, representing Cuba and Puerto Rico, stated three conditions necessary for the propagation of yellow fever:

1. "The presence of a previous case of yellow fever within certain limits of time….
2. The presence of a person apt to contract this disease…and
3. The presence of an agent entirely independent for its existence both of the disease and of the sick man, but…necessary in order that the disease shall be conveyed from the yellow-fever patient to a healthy individual."[65]

Finlay admitted his theory of an intermediate agent was "a mere hypothesis," but maintained its validity, and in August he would proclaim the mosquito as that agent. His announcements, however, fell on deaf ears.[66]

Sternberg left no written opinion of the conference proceedings, and Finlay's hypothesis left him unmoved. His mind was engaged elsewhere. Experiments relating to the virulence of his saliva and work on disinfectants were begun in January in the Washington laboratory and continued in Dr. H. Newell Martin's laboratory at Johns Hopkins University, where he was reassigned by the NBH at the beginning of March. Martin, professor of biology at Johns Hopkins, was an outstanding research physiologist who had been trained by Michael Foster and Thomas Huxley, and had been recruited by the university in 1876. His small laboratory rapidly became a center for physiological research, and there Martin made many significant contributions to the physiology of the circulation. Sternberg's experiments, with what would prove to be *Streptococcus pneumoniae*, were as close as he would ever come to presenting a previously unknown microorganism to the world. Soon after he began his studies, he became aware that Louis Pasteur had found—and reported—a "new disease" generated by the subcutaneous injection of saliva from a child dying with rabies into rabbits. The etiological agent was a micrococcus, but it was weeks before Sternberg could unequivocally declare that the micrococcus recovered by Pasteur was identical to the one he had found in his mouth in New Orleans. His research, however, was independent of Pasteur's, and his report demonstrates the elegant, logical, and comprehensive approach to solving laboratory questions for which he became famous.[67]

Sternberg hypothesized that one to two cubic centimeters of his saliva injected subcutaneously into rabbits invariably produced death within 48 hours. To prove this contention, he first injected rabbits with other fluids—blood, putrefying bouillon, fecal and mud solutions—and saliva from colleagues in Philadelphia and students in Baltimore. Only solutions of mud from New Orleans and saliva from Philadelphia produced death. Sternberg's oral secretions were apparently not unique in their virulence, a difference he attributed to exposure to septic material by his colleagues and himself over the years. Then he attempted to produce fatalities in other laboratory animals, specifically dogs, guinea pigs, chickens, and rats. As with Pasteur's experiments, only the guinea pigs succumbed. However, one of the dogs died when injected with serum from a rabbit recently dead from septicemia. Apparently, whatever killed the rabbits became more virulent in the process and was capable of killing larger animals when transmitted by serum injection.[68]

He described the disease as a septicemia, both in the nature of its course and at postmortem examinations. Shortly after, injection fever developed, which was followed by marked inflammation and edema at the injection site. At 24 hours, the animal was sluggish, without appetite, and death usually occurred by 48 hours. He said that an examination of venous and arterial blood and the bloody serum from subcutaneous connective tissue revealed "an immense number of micrococci, usually joined in pairs."[69] The virulence of the microbe was destroyed by boiling or incubating at 37°C for 24 hours. Filtration through a layer of plaster of Paris rendered these fluids innocuous, and, therefore, Sternberg concluded the virulence factor was particulate in nature. Virulence was maintained in initial and successive cultures in bouillon and serum from healthy dogs, and he noted, as did Pasteur, the capsule surrounding each organism.[70]

Finally, Sternberg concluded the microbe found in his saliva was morphologically identical to that found by Pasteur in the mouth of a child dying of rabies in Paris. He was quick to point out, however, that identical structural characteristics did not guarantee the diseases generated were identical. Sternberg was adamant on this point and stated, "The man of science soon finds that things which look alike are not necessarily of the same kind…. The argument…that because a certain bacillus, or spirillum, or micrococcus, is morphologically identical with another, which is proved to be harmless…consequently it must be harmless, has no support from analogy any more than it has from experiment. And it is high time that naturalists and physicians should open their eyes to the fallacy of such an argument, as it not only has a tendency to close the minds of those who receive it to the reception of demonstrated truth, but also acts…as a bar to the progress of science in this direction."[71] He admitted the two diseases had many characteristics in common, "but I am not prepared to pronounce a positive opinion upon this point, especially since Pasteur, who had previously given much attention to the study of septicaemia, pronounces the disease observed by him to be new, while I see no reason…for supposing that the disease observed by me differs essentially from the experimental septicaemia produced by Davaine, Koch and other investigators, who, however, obtained their first supply of septic organisms from a different source."[72]

Sternberg, for personal reasons, would have liked the two diseases to be different. But his obligation to science was to analyze and present his results as they were and correlate them with previously reported data. In doing so, he made these conclusions concerning systemic infections, their distribution in nature, and impact on society: "In the light of what we know now, it seems very probable that puerperal fever, hospital gangrene, and the various forms of septicaemia…result from the development of pathogenic varieties of harmless and widely-distributed species of micrococci…. The fact…that during the summer months the mud in the gutters of New Orleans possesses an extraordinary degree of virulence shows that pathogenic varieties of bacteria are not alone bred in the bodies of living animals. The more I study this subject the more probable it seems…that in this direction lies the explanation of many problems which have puzzled epidemiologists, and that the sanitarians are right in fighting against filth as a prime factor in the production of epidemics…. The presence of septic organisms, possessing different degrees of virulence depending upon the abundance and kind of pabulum furnished them and upon meteorological conditions more or less favorable, constitutes…the epidemic constitution of the atmosphere, which wise men were wont to speak of not many years ago as a cloak of ignorance. It must be remembered…the gutter mud of today, with its deadly septic organisms, is the dust of tomorrow, which in respiration is deposited upon the mucous membrane of the respiratory passages of those who breathe the air loaded with it. Whether the peculiar poison of each specific disease is of the same nature or not…it is altogether probable…this factor often gives a malignant character to epidemics of diseases which uncomplicated, are of a comparatively trivial nature."[73]

This passage reflects not only that Sternberg had acquired a bit of the preacher

from his father and had developed his epidemiological skills since 1870, but also that he recognized the broader implications and applications of laboratory medicine and a responsibility to articulate them. He understood that the very nature and distribution of microorganisms demanded sanitarians and bacteriologists to work together for the good of public health science. He also recognized in these experiments that morphological characteristics were not indicative of microbial virulence. The work of Casimir Davaine and Koch with the anthrax bacillus and Otto Obermeier with the spirillum of relapsing fever had generated a hope that structurally distinct organisms would be found for each infectious disease. Sternberg saw this as an easy answer to a complex subject, a forlorn hope. Virulence did not necessarily exist unchanged over time, and its effects were influenced by many host and environmental factors.

Sternberg undoubtedly recovered and observed *S pneumoniae* two months before Pasteur, but he was unable to describe its structural and virulent characteristics until three months after the French chemist had published his work, and, therefore, priority of discovery fell to Pasteur. He could claim, however, he was the first scientist in the United States to independently identify the organism. The fact that *S pneumoniae* was originally recovered from healthy carriers of the organism—not from patients ill with streptococcal disease—does not appear to have been considered significant, nor was the capsule surrounding the organism given much consideration. The medical communities in Europe and America gave little attention to the discovery. One more microorganism that induced septicemia in laboratory animals had been discovered, but no substantial connection with human disease had been offered.[74]

Nonetheless, the spring of 1881 was a high water mark for the 43-year-old Sternberg. His ambition, boundless energy, and motivation for studying, experimenting, and publishing overcame the difficulties multiple army relocations imposed on his scientific endeavors. He was a subject matter expert on yellow fever and microscopy and, through his NBH investigations, was recognized as one of the premier laboratory scientists in the nation and a leader in the fledgling field of bacteriology. Designated a fellow by courtesy of Johns Hopkins University, Sternberg settled easily into academic life at the university. With no army-imposed time limits bracketing his special assignment to the board, it appeared likely that he would continue in this capacity for an indefinite period.