Chapter Seven

Exiled to California

Sternberg engaged in various bacteriological projects for the National Board of Health (NBH) during the spring and summer of 1881. The highly virulent *Streptococcus pneumoniae* was an excellent subject for disinfectant efficacy studies, and he tested it against several nongaseous disinfectants in the laboratory at Johns Hopkins University. Through H. Newell Martin he obtained anthrax spores from British physiologist and pathologist, Dr. John Scott Burdon-Sanderson, raised his own colonies of anthrax, and made photomicrographs of the deadly bacillus. He also studied the micrococci of diphtheria and gonorrhea, and evaluated and described some of the normal microbial flora found in the human urinary and gastrointestinal tracts. Louis Pasteur's research had shown that micrococci, which normally inhabit the distal portion of the male urethra, were responsible for the decomposition of a urine sample over time. Joseph Lister had demonstrated urine taken from a healthy bladder was sterile and remained sterile if not inoculated with bacteria from some external source. Considering these two facts, Sternberg collected first flow and mid-stream urine samples in sterile flasks and observed them for turbidity indicative of microbial growth. While the first flow samples always became turbid with micrococci, variable results were obtained with mid-stream collections. He modified the collection procedure by disinfecting the penile tip—his own—with a 3 percent carbolic acid solution before collection. All of these samples remained transparent. For the first time, the value of the "clean catch" urine specimen was demonstrated and reported to physicians. Sternberg commented, however, that the application of carbolic acid to tender mucous membranes "produced some pain, and a little soreness upon passing urine was felt for two or three days."[1]

In "A Contribution to the Study of the Bacterial Organisms Commonly Found upon Exposed Mucous Surfaces and in the Alimentary Canal of Healthy Individuals," Sternberg identified, described, and photographed commensal organisms in the oral cavity and alimentary tract. This paper was a succinct distillation of his

thoughts, ideas, and laboratory techniques. It was presented in a readable literary style and illustrated two of his most valuable assets as a scientist:

1. the intellectual ability to absorb, understand, and synthesize a continually increasing amount of scientific data, and to generate hypotheses that—for the time—were remarkably accurate; and
2. the technical creativity and manual dexterity to solve methodological problems at the laboratory bench.

The importance of these studies was clearly evident to him. "That there are many widely distributed forms (species?) which are ordinarily harmless…. It is evident that a precise knowledge of the morphology and development…of these common forms is an essential prerequisite to the recognition of unusual forms and to the…study of…such forms to any particular disease with which they may be found associated."[2] Sternberg suggested that these common bacteria were not merely parasites, but also played a vital role in the human body's daily functions. Under certain conditions, pathogenic organisms could overwhelm the body's resisting power and displace this normal flora, and he perceived the degree of virulence of an organism was directly related to the route of entry into the body, a proper environment for growth, and the organism's ability to multiply rapidly. Concerning this resisting power, he noted, "It has occurred to me that possibly the white corpuscles may have the office of picking up and digesting bacterial organisms when…they find their way into the blood. The propensity exhibited by the leucocytes for picking up inorganic granules is well known, and that they may… assimilate, and so dispose of…bacteria…does not seem to me very improbable in view of the fact that amoebae, which resemble them so closely, feed upon bacteria and similar organisms."[3] Sternberg would add little to this preliminary hypothesis in his book *Bacteria*; however, he boldly suggested his precedence for asserting this idea, worked out by Elie Metchnikoff in 1883, in later years.[4]

A skilled lab bench technician, Sternberg developed a method for ensuring the sterility of liquid culture media. Using quarter-inch glass tubing and a foot-operated bellows or Bunsen burner, he manufactured small bulbs with elongated necks that could be filled with whatever liquid media he desired, heat sealed, and then sterilized in a bath of oil, paraffin, or concentrated salt solution. By using these "Sternberg tubes," as they became known, he could maintain the sterility of his solutions indefinitely. This was an advantage over Pasteur's flasks and even Robert Koch's new plating techniques, and he could always have media at hand because of the easy transportability of the tubes.[5]

At Johns Hopkins, he was comfortably ensconced in the academia of one of America's most progressive institutions. But more than bacterial cultures would soon be incubating in the laboratory. In December 1879, Dr. James Cabell, president of the NBH, asked Dr. John W. Mallet, professor of general and industrial chemistry at the University of Virginia, to plan a series of water analysis studies. The board liked the plan, approved funding, and authorized Mallet to begin work. Like his friend and colleague Cabell, Mallet worked and directed his assistants from the

university in Charlottesville. As the water analysis project progressed, Mallet requested bacteriological support. Initially, Doctors Martin and Edward M. Hartwell provided this support, but Sternberg replaced Martin at the beginning of March 1881 when the latter was called away. For unknown reasons, Mallet also wanted to conduct certain yellow fever experiments in the Baltimore laboratory, and he probably thought that since Sternberg was an authority on the subject and now a part of his team, he would be glad to participate. But differences between Sternberg and Mallet regarding the experimental methods soon developed. Sternberg strongly disagreed with Mallet's approach and said so. He also offered to go to a quarantine station, presumably to conduct the experiments, but Mallet never responded to the suggestion. By July, this initially professional struggle had degenerated into a largely personal tug-of-war over scientific jurisdiction and control. Mallet finally informed Sternberg that he accepted full responsibility for such experiments, and the laboratory was to proceed with them.[6]

Sternberg interpreted the directive as an indication that Mallet regarded him in a subordinate capacity. It is doubtful whether the professor could have said or done anything that would have provoked Sternberg's sense of professional position, pride, and dignity more than to suggest such a relationship. It appears, too, that this may not have been the first instance in which Mallet gave the major less respect than he thought he was due. In a letter written to Dr. Thomas Turner, secretary of the NBH, Sternberg stated, "I can not doubt that the earnest remonstrance made by me when Prof Mallet first proposed that experiments with yellow fever material should be made in Baltimore, if made by Dr. Martin would have induced him to abandon the scheme at once…. He took the ground that I was not the equal of Prof Martin & himself but his subordinate. I freely admit that Prof Martin is my superior as a physiologist & Dr. Mallet as a Chemist, but do not admit that the title of Surgeon U.S. Army is in any way inferior to that of Professor of Chemistry or of Physiology…."[7] Sternberg quickly communicated this slight to Cabell hoping that the board president would adjudicate the situation in his favor. Whether Cabell was dutifully closing ranks with a university colleague or just remaining above the fray is unclear, but his return telegram made it obvious that he also considered Sternberg in a subordinate role: "Apply to Mallet, whatever he agrees to will be acceptable to me, but his directions must govern you."[8] Sternberg was furious and immediately tendered his resignation from the board to the army adjutant general.[9]

He regained his composure enough on the following day, July 18, to write a civil letter to Cabell explaining his proposed methods for conducting the yellow fever experiments. This correspondence has not survived, but whatever was said stimulated Cabell to ask Mallet to consider a compromise. Sternberg received this word from the NBH president, and a note from Mallet that declined any experimental compromise on July 19, after returning from a meeting with Turner in Washington to insist his request for relief was forwarded. Sternberg told Cabell, "This action was grounded upon your telegram of July 17th…. This telegram placing me directly under the orders of Dr. Mallet made it necessary for me to apply to

be detached from this duty as I am unwilling to serve the Board in a subordinate capacity. Some of the younger members of the Medical Corps may not feel as I do, in this matter, but being a surgeon of twenty years service & having occupied many responsible positions I much prefer my army duties to any subordinate position with the Board of Health. I was quite willing to aid Dr. Mallet as Prof Martin's substitute during his absence, and have made every effort to carry out his wishes, but I judge…he considers me under his immediate orders while it had not occurred to me that such was your intention until I received your telegram."[10] In conclusion, Sternberg thanked him for the "kind and liberal treatment [he] had received…up to the time you consented to sacrifice my interests upon the altar of Dr. Mallet's yellow fever experiments. I can not help thinking that this was done without a full knowledge of the circumstances & if I have done you an injustice by drawing too hasty a conclusion I ask you pardon."[11]

Sternberg's abrupt resignation caught the Surgeon General's Office by surprise. Colonel Crane's immediate response was to table the document for a couple of weeks to allow time to sort out the situation. In the meantime, he told Sternberg to take a trip to the beach to cool off. Sternberg dutifully packed up Martha, and they spent the next week in Asbury Park, New Jersey. But if Crane thought a few days of ocean air and sea bathing would put Sternberg in a more conciliatory mood, he did not know Levi Sternberg's son very well. Sternberg commented that they enjoyed the seashore, but he remained preoccupied with the current issue and soon became restless with the imposed inactivity. He wrote a farewell to the NBH on July 26 just before leaving Asbury Park. He asked the board's secretary to explain to the membership his reasons for leaving and stated, "I should be sorry to be considered ungrateful for past favors or to lose the good opinion of my friends in the Board & I desire to acknowledge my high appreciation of the consideration with which I have been treated during the two years I have been in the service of the Board…. I have never been willing to occupy the position of handy man to be called upon when needed for miscellaneous work, but have looked upon myself as an earnest & industrious worker in the difficult paths of experimental investigation by which we hope eventually to shed some light upon the unsolved problems relating to the etiology of epidemic & infectious diseases."[12] He also sent a courtesy copy of this letter to Crane. The matter was settled in Sternberg's mind, and he was ready for a new assignment. He hinted to Crane he "would be admirably situated for pursuing my studies & experimental researches if stationed at Fort McHenry as I would have the use of the library & laboratory at Johns Hopkins," but then excused himself as he did not know the practicality of the idea or "how far the Surgeon General will be disposed to favor me in my desire to continue my experimental studies."[13] Over the next five days, however, it appears that the board attempted to reconcile the issue in Sternberg's favor. Sternberg again wrote to Crane asking for advice on what he should do if the board asked him to return. He was not only willing to consider what they offered, but also ready to accept it, unless Crane could give him "a station where I can settle down for two or three years & where I will have some facilities for prosecuting the experimental studies in which I am

interested…."[14] Crane replied tersely, "I do not believe you can properly recede from the position you have taken and my advice, is, that you do nothing! This is the best time for you to dissolve your connection with the 'National Board of Health,' and resume your legitimate duties as a medical officer of the army. As soon as the matter can be discussed with the Surgeon General I will inform you of the assignment which will be made for your further station."[15] The deputy surgeon general had lost patience with Sternberg and the idea of his assignment to the NBH. For reasons unknown, the discussions to keep Sternberg with the NBH rapidly unraveled, and the surgeon general—very likely on Crane's recommendation—was not inclined to grant him any favors or concessions concerning his next assignment. On August 10, Major Sternberg was relieved from duty with the NBH and assigned to Fort Mason in the Department of California.[16]

The scant historical evidence available concerning this incident makes it difficult to clearly understand all of the personal and professional factors involved. Certain tentative conclusions may be drawn when the affair is observed on a broader scope. Sternberg was a respected soldier and physician with 20 years of exceptionally active service. At this point in his career, by today's standards, he would have held the rank of colonel in a hospital command or senior medical staff position. His interests in science led him very early in his career down an unknown path—and one considered essentially useless—by the majority of his Medical Corps colleagues. Combat record aside, Sternberg was an oddity, something of an outcast in the army medical community of the time, but he persevered with his work and convictions until technological advancements in science and medicine demonstrated he was not as far out in left field as perceived. His clinical and epidemiological studies on yellow fever and skills as a microscopist gained the attention of the national medical community, and his laboratory investigations in Cuba solidified his position as an expert on the disease and as a laboratory scientist. By late 1879, Sternberg clearly recognized his status and expected to receive the respect to which this entitled him. From his active involvement with the American Public Health Association (APHA), it appears that he was accorded this respect from his colleagues in that organization. However, university faculty members, who considered themselves professional academicians, may have regarded him as a part-time scientist, or an upstart military surgeon with an over-inflated ego. Sternberg saw himself as a very serious medical scientist and academician. Yellow fever research had brought him to the NBH, and—justified or not—he felt a keen sense of ownership over any research bearing on the disease. When Mallet proposed conducting yellow fever experiments without Sternberg's blessing, he encroached on the army scientist's perceived territory and authority. His desire to be treated in a professional manner was understandable, yet Sternberg's vanity overcame his own professional standards and good sense in resolving the conflict, not only with Mallet, but also with the surgeon general. He kept the Surgeon General's Office fully informed about the status of the affair and expected support from this quarter. But he failed to consider the political impact his resignation would have on an organization struggling to establish its credibility in Washington, and

how it would reflect on the Medical Department. The Army and the Medical Department had supported Sternberg's special assignment to the NBH because of his superb abilities as a laboratory scientist. It was expected that his performance would reflect well on the army and Medical Department, and be their endorsement of the new health agency (the NBH). For Sternberg to unilaterally back out because he could not get along with his fellow scientists appeared as dissension within the board and only fueled the fire of antagonism the organization was facing on Capitol Hill. Barnes may also have been personally irritated with his decision. He had been listening to Sternberg complain for years about the importance of his research, and the difficulty to pursue it with little support from the Medical Department. The surgeon general had assisted in giving him the opportunity and support he had wanted for so long, but now Sternberg wanted to quit over an issue that was—when considered in the larger scheme—minor. He then had the audacity to suggest where his next assignment should be so he could continue the same research and requested the NBH loan him the necessary equipment. Barnes was in no humor to do Sternberg any favors, a fact reflected in the assignment he selected. By ordering him to the Department of California, Barnes essentially exiled him to do penance in a scientific desert for being so stubborn and intractable.[17]

If Sternberg was depressed or felt any remorse over the Mallet affair, as he and Martha rode the train west once again, it was only temporary. "The order relieving him from his experimental work in the East might have been so discouraging for many men that they would have given up the self-imposed task," Mrs. Sternberg noted, "but such was not the case with him."[18] With Crane's approval, they stopped briefly in Cincinnati, where Sternberg read his paper on common alimentary organisms to the American Association for the Advancement of Science, before proceeding to Indianapolis and then Ellsworth in late August.[19]

In early September, the Sternbergs were situated in their new quarters at Fort Mason on Point San Jose in San Francisco, which was "a charming little house on the side of a high bluff, overlooking the bay," according to Mrs. Sternberg.[20] As usual, her husband quickly fashioned a laboratory in their home. To furnish it, he applied—through Crane to the surgeon general—for microscopical equipment. It probably came as no surprise that the request was denied. "I presume…you have learned from Genl Crane that the Surgeon General disapproves your application for microscopical apparatus. For this I am personally sorry," wrote a sympathetic Joseph Woodward. "I may mention that General Crane showed me your application and asked me what I thought of it. I told him that I hoped it would be granted and believed you would make good use of the apparatus."[21] Sternberg was not to be denied by his disgruntled chief, so he purchased the equipment himself.

In addition to Sternberg's clinical duties and participation on various army evaluation boards, he found time to pursue science and literary endeavors. He wrote *Photomicrographs and How to Make Them*, a handbook for the novice photomicrographer and one of the earliest instructional texts on the subject. Sternberg provided complete and concise directions on how to collect, stain, and mount bacteria for photographing. He believed—as Koch did—that when the subject was

appropriate, photomicrographs were superior to drawings and should be made and used as proof in scientific investigations. Although the San Francisco posting had made his scientific pursuits more difficult, it does not appear that they were significantly impeded. His laboratory work, although conducted under more austere conditions, was a continuation of the ever-broadening bacteriological research he had conducted in Baltimore. In retrospect, the time he spent with the NBH had tremendously influenced the direction of his career as a bacteriologist and medical scientist. Sternberg became the first laboratory authority in the United States to confirm new discoveries in bacteriology. Mrs. Sternberg commented more than once that it was a shame so much of her husband's time was engaged in confirming the observations of others. In reality, Sternberg's technical skills, interests, and objectivity and conservatism as a scientist placed him in this role, one he considered critical for medical science advancement. His attempts to demonstrate the gonococcus of Albert Neisser and the tubercle bacillus using Koch's method from the spring to the fall of 1882 illustrate these points very well.[22]

Neisser, a young dermatologist and bacteriologist in Breslau, had demonstrated the gonococcus in urethral discharges of male and female patients suffering from purulent urethritis and in infants with blennorrhea neonatorum in 1879.[23] Although he did not prove cause and effect, the gonococcus immediately joined the list of specific bacterial disease agents. Sternberg's interest in the gonococcus originated in Baltimore in 1881, after his yellow fever and malarial research was temporarily halted. In 1882, presumably using the methyl-violet staining method advocated by Neisser, Sternberg found plenty of micrococci in gonorrheal discharges, but identified them as *Micrococcus ureae*, a common commensal organism of the distal male urinary tract. He conducted culture and inoculation experiments anyway, and his commentary on these experiments provided valuable and interesting insight concerning his scientific philosophy and methods, and the state of medical research. He initiated his work with culture and inoculation experiments with specimens from a case of gonorrhea diagnosed in a soldier at Fort Mason. Unable to produce the disease in dogs or find any willing subjects through bribery, he obtained male volunteers through Dr. Joseph O. Hirschfelder at the San Francisco City and County Hospital.[24] According to Sternberg, "These patients consented…with a full knowledge of the possible results, from a desire to please their doctor, and under the *promise* of [a] speedy cure [italics mine] and a suitable recompense in case of successful inoculation."[25]

The ethical implications of such experimentation are glaring today, but in 1882 modern experimental medicine was in its infancy. Ethical responsibility for medical experimentation devolved upon the moral character of the individual physician. No sanctioned ethical code for medical experimentation existed, and the only guidance for appropriate human experimentation came from Claude Bernard who wrote: "It is our duty and right to perform an experiment on man whenever it can save his life, cure him or gain him some personal benefit. The principle of medical and surgical morality, therefore, consists in never performing on man an experiment which might be harmful to him to any extent, even though the result might be highly advantageous to science, that is, to the health of others."[26]

Hirschfelder's volunteers consisted of three bedridden patients who suffered from end-stage diseases. Their desire to please Hirschfelder and need for compensation cannot be assessed, but probably influenced their decision significantly. Although Sternberg did obtain what he considered informed consent, it is questionable how much of his explanation they understood. That he truly believed cure was possible—using urethral injections of mercuric bichloride or zinc sulfate solutions—is confirmed by the fact that he swabbed his own urethra with gonorrheal cultures during his research. No cases were forthcoming, but by comparing *M ureae* to the septic micrococcus (*S pneumoniae*) of rabbits he did show—to his satisfaction anyway—that structural and functional differences do exist among bacteria. It was, therefore, possible to develop a taxonomic categorization of these organisms as advocated by Ferdinand Cohn and Koch. Confident that he had made no technical errors, Sternberg wrote about his results. In the introduction of this report, he cautioned his colleagues to maintain "a proper scientific conservatism" and not rush to embrace the idea that all infectious diseases were caused by specific microorganisms.[27] This last statement is incongruous today, but in 1882 it was plausible because the nature of infection and infectious disease was just beginning to be elucidated. Sternberg concluded that Neisser was wrong to claim that his organism could be differentiated from other micrococci by morphological characteristics and that the gonococcus and *M ureae* were the same organism.[28]

On March 24, 1882, Koch demonstrated the tubercle bacillus for the first time to colleagues in Berlin.[29] His work was published on April 10. By late May, Sternberg had read the abstracts of Koch's work and conducted his own examinations of tuberculous material obtained from Hirschfelder. For the next 10 weeks, he was repeatedly frustrated by his failure to find the bacillus using Koch's or Paul Baumgarten's staining methods. He was not alone. Other American and European physicians also found Koch's method too difficult to manage, which led to skepticism and outright disbelief in the German discovery. Sternberg never mastered Koch's technique, but on August 8, he finally demonstrated the bacillus in postmortem lung preparations using Paul Ehrlich's method, a variation on Koch's theme, which he apparently had only become cognizant of at the beginning of the month. Cultivating the organism and producing disease in laboratory animals were difficult. Although Koch was convinced the bacillus was the specific causative agent of tuberculosis, Sternberg remained unconvinced, a fact that he wrote about in a five-article series in the *Medical News* between July and the end of December. His skepticism stemmed not from a lack of faith in Koch's techniques, a man whose intellect and technical competence he greatly admired, but from his own understanding of the tubercular disease process, the variability of his repeated experimentation, and a healthy scientific conservatism. Tubercular nodules were considered to originate from a local inflammation that could be infectious or noninfectious. He was convinced of this because he had seen numerous postmortem sections of tubercular lungs devoid of Koch's bacillus. While Sternberg believed in the validity of the germ theory of disease causation, he freely admitted that it did not yet rest upon a solid experimental foundation. Negative evidence from his

own laboratory led him to argue that Koch's bacillus could be associated with the disease, but no undisputed proof of its specificity as an agent existed. Moreover, other bacteria apparently could produce tubercular nodules just as easily as Koch's bacillus if they were situated in one of these inflammatory areas. Sternberg reluctantly concluded this work in October. Obtaining laboratory animals was difficult, and anti-vivisectionist sentiment—fueled by Mrs. Irvin McDowell, the garrison commander's wife—was strong. Moreover, the large expenditure of time and money required and the lack of proper facilities had become too great a burden to continue such work. He returned to an old passion, disinfectants, a venture he considered more universally lucrative in the long run for public health and bacteriology.[30]

Not all of the standard disinfectant agents then on the market were equally effective. Some killed bacteria (germicides) while others only inhibited bacterial growth (antiseptics), and some—in concentrations or volumes practical for public or clinical use—had no effect on bacteria, but were good deodorizers. Some of these germicides were also used as therapeutic agents. Sternberg evaluated the germicidal power of many agents at different concentrations and compared his results with current clinical experience. His experiments performed on various micrococci demonstrated that the value of these agents depended on the concentrations used and the microorganisms to which they were applied.[31]

This series of investigations was the last he performed at Fort Mason. The year 1883 would be a busy one for him, but at his desk and in the Surgeon General's Library rather than in the laboratory. *Photomicrographs and How to Make Them* and an article on malaria were published in January, the "Germicidal Value of Certain Therapeutic Agents" appeared in April, and he was heavily engaged in completing the second edition of his translation of Magnin's *Bacteria*. Nevertheless, he accepted a request from the APHA to report on the value of experimental evidence of the etiology of malaria for presentation in November, and, with considerable hesitation, one from the William Wood Publishing Company to compose a book on malaria and malarial diseases. Sternberg's complete transition from laboratory bench to literary endeavors—at a great distance from required resource materials—suggests he needed a break from the laboratory, but it would be uncharacteristic of him not to have had a professional goal for this transition. One such goal may have been to establish himself as the foremost authority in bacteriology in the United States by writing the definitive treatise on the subject.[32]

Sternberg's translation of Magnin's work had been well received, and it was an instructive handbook to American bacteriologists. By 1883, its contents had become outdated and incomplete. In the three years since publication, technological advancements in the cultivation, recognition, staining, and attenuation of microorganisms were made; germicides and antiseptics were more clearly defined and understood; the list of pathogenic organisms grew considerably longer; and the volume of bacteriological literature became massive. Just as he had perceived the need for a basic manual on the subject, Sternberg now recognized the need to update it for the benefit of his colleagues. Magnin's original work comprised only the first third of the second edition. The remainder originated from Sternberg, a complete,

comprehensive, detailed, and thoroughly understandable tour de force in bacteriology. Probably no other scientist in America—perhaps even in the world—had so universal an understanding of this field in 1883, the intellectual ability to synthesize and condense this knowledge, and the literary skills to put it on paper. In producing a second edition of *Bacteria*, he ensured that his name and ideas were associated with the most current bacteriological knowledge and progress.

Mrs. Sternberg's status as a widow to a man driven by science changed in style, but not in substance in 1883. Her biography contains no comments for this year until late in November. Sternberg's personal papers also provide little information concerning his activities, an indication that—apart from routine post surgeon duties—he was consumed by his study. By the middle of August, he had completed the second edition of *Bacteria*, and on October 6, he boarded the eastbound train for Washington, where he immersed himself in the malaria literature available in the Surgeon General's Library for the next 5 weeks. At first glance, *Malaria and Malarial Diseases* appears as another example of Sternberg's compulsion to always have more work than one scientist could accomplish. A more accurate assessment is that he accepted the request as a challenge to write the definitive work on malarial fevers.[33]

In the fall of 1883, the Army Medical Museum and Surgeon General's Library were about to undergo some major administrative and personnel changes. It was proposed that the museum and library be reorganized under one head and the current curator, surgeon David Low Huntington, be reassigned as an assistant to the surgeon general. How privy Sternberg was to these changes before his visit is unclear, but he apparently attempted to maneuver himself into the curatorship and, presumably, looked to Surgeon General Crane for support. Unfortunately, Crane, who had assumed the Medical Department helm from retiring Surgeon General Barnes in June 1882, died on October 10, 1883, and Robert Murray replaced him in November. Surgeon General Murray opted for John Shaw Billings to assume the combined museum and library duties, and Sternberg learned of Murray's decision before he left Washington, DC, for the APHA meeting in Detroit. Sternberg was tremendously upset over Billings' selection. His professional timing—so critical in securing choice assignments—had been off since his tenure with the NBH. When the museum's previous curator, surgeon George A. Otis, died in February 1881, Huntington assumed the curatorship. When Joseph Woodward became ill in early 1882, Major Charles Smart, Sternberg's former laboratory colleague who was still working for the NBH, was tapped to continue Woodward's work on the *Medical and Surgical History of the War of the Rebellion*. Now Billings, who had been in Washington since the Civil War, was given the dual responsibilities of museum and library, while Sternberg languished in California.[34] By the time he returned to Fort Mason, he had mulled the entire issue over and over again. On November 27, he prepared a lengthy missal for the surgeon general in which he vented his grievances:

> "I would respectfully ask your attention to the following statements relating to my future career….It is my earnest desire to devote my time to scientific and literary work and especially to microscopical and experimental studies relating to the etiology of infectious diseases. Since leaving the National Board of Health, Aug. 23, 1881, I have been obliged to prosecute my experimental work at my own expense…in order not to drop out of sight as an investigator,

in a field in which I have gained some distinction. And this notwithstanding the fact that apparatus of the same kind, purchased with government money, has been for two years lying idle [in Washington].

"With the experience and special training I now have and with proper facilities I…could cultivate this field still more successfully in [the] future and…by such labors accomplish more for humanity, for the credit of the Medical Corps of the Army, and for my own reputation than by continuing to perform the routine duties of an Army Surgeon. But I am satisfied that it is useless to continue my attempts in this direction…without encouragement and material assistance from some source.

"I find also that I labor under great difficulties in prosecuting the literary work which I have undertaken on account of my remoteness from libraries and from my publishers…. I would further respectfully represent that my Army service has been mostly at remote posts; that I have seen my full share of epidemics and Indian wars; that I have had but one brief tour of duty in the East (1870–1872); that this was broken by three changes of station and an epidemic of yellow fever, that when I accepted a detail as member of the Havana Yellow Fever Commission I made considerable sacrifices, and enlisted in the cause of scientific research; that my tour of duty with the National Board of Health ought not to have been counted against me as Eastern service as I spent the first summer in Havana and the second in New Orleans and I was only in Washington during the winter months for the purpose of writing my reports and recuperating my strength.

"I would further respectfully represent that two positions, which I have felt that I had some claim to, have been filled by the detail of officers junior to me in the service and both of whom had just served a tour of duty in the East. I refer to the position of Curator of the Army Medical Museum and to that of a member of the National Board of Health. Either of these details would enable me to pursue my microscopical and experimental studies and to continue my literary labors under favorable circumstances."[35]

The sacrifices Sternberg mentions in relation to the first Havana Yellow Fever Commission (unless he is referring to the time he spent away from Mrs. Sternberg) remain obscure. Whatever sacrifices he thought he made paled in comparison to the recognition he received as a scientist. His complaint about a junior officer, Billings, being selected over him for curator must be taken in context. Sternberg had no personal or professional animosity toward Billings or any of the other museum staff members. His relationship with all of them through the years had been one of mutual support and cooperation. However, he felt excluded from a club in which he felt he had earned a place—the Mallet affair notwithstanding—but was continually denied membership. That Billings understood this—and was thinking of Sternberg's future—is evident in his remarks to him in a letter from mid-January 1884: "I am sorry that it has not been found possible to meet your wishes by placing you in charge of this department, but I hope I shall be able to help you to a part at least of what you want after a little [time?]."[36] Whether Sternberg's letter or Billings' advocacy materially influenced Surgeon General Murray's decision to retrieve him from the west coast can only be speculated, but in April 1884, Sternberg received orders to report to Governors Island, New York, in the Department of the East.[37]

The importance of Sternberg's contributions to the field of bacteriology while in California has been glossed over, usually with the statement that he was the first—or most likely the first—scientist to demonstrate the tubercle bacillus in America, or it

has been totally missed by historians and biographers. He was the first to demonstrate the tubercle bacillus in America. This event, however, was significant to Sternberg and other scientists because it validated the work of Koch and Ehrlich and, thereby, supported the existence of a tubercle bacillus, and not because he was the first American scientist to do so. Regrettably, the primacy of giving Sternberg a "first" at something—a landmark for posterity—has become the focus of his California experience and a roadblock to understanding his true intentions, goals, and accomplishments performed under difficult conditions. During his three-year tour at Fort Mason, he persevered with his laboratory and literary work upon the tubercle bacillus, a variety of micrococci, and disinfectants. All of these labors had the same two fundamental objectives: (1) to materially assist the development of bacteriological science through hypothesis testing, reproducing and verifying the work of others, and comparing their experiments with his own; and (2) to educate and mentor American scientists via the written word. These are the successes—the firsts he established for bacteriology in America—for which he should be remembered. While young bacteriologists, like William Henry Welch, T. Mitchell Prudden, William Councilman, Herman Biggs, and others were studying at the feet of the masters in Germany, Sternberg was already well versed in the most current experimental methodology. His many theories, experimental philosophy, and laboratory techniques reached only a limited audience, however, until the publication of the second edition of *Bacteria*. This book, a culmination of all of his research and an objective discussion on current bacteriological issues, as well as a valuable laboratory manual, launched Sternberg to the forefront, established him as an undisputed authority in theoretical and practical bacteriology, and provided the groundwork for his magnum opus, a *Manual of Bacteriology*, published in 1893.[38]