Chapter Eight

Dean of American Bacteriology

Sternberg completed *Malaria and Malarial Diseases* just before leaving Fort Mason in April 1884. Laboratory and sentimental belongings were carefully packed, and the rest of the Sternberg household was consigned to the auctioneer. "Crossing the continent was no longer a novelty," Mrs. Sternberg commented years later, "but we took considerable interest in drawing comparisons between conditions on this trip and on those we had previously made. The railroad had made great strides and…the new dining car service a great improvement on the eating stations of earlier days. Many little towns were springing up near the railroad, marking the advance of civilization across the plains. The immense herds of wild animals that formerly roamed at will were almost annihilated. Where as…there were in the sixties such great numbers of buffalo that they blocked the railroads, we now saw only small bands. Immense numbers had been slaughtered for their skins alone, or for the tongues, as these were considered a great delicacy.… The buffalo, self-supporting on the grass of the unclaimed prairie, deserved a better fate, more especially as the red man drew largely upon him for subsistence."[1] For the next two months, the Sternbergs lived an unsettled life. He reported for duty at Department of the East headquarters, Governors Island, New York, on April 28, and then attended the American Medical Association meeting in Washington in early May. By the middle of the month, he was on unspecified temporary duty in the Surgeon General's Office, and on June 6 he received permanent assignment as attending surgeon and examiner of recruits in Baltimore. The assignment suited Sternberg perfectly because he could perform army duties and still have time to conduct bacteriologic research in Dr. H. Newell Martin's laboratory at the downtown campus of Johns Hopkins University.[2]

The Sternbergs rented a furnished home at 52 McMechan Street. Mrs. Sternberg was extremely pleased to be back in Baltimore for its social, cultural, and educational opportunities. She studied art history with Miss Jane Addams, and she and her husband found classes on French literature at Johns Hopkins University a relaxing

and entertaining way to spend late afternoons together. She also admits for the first time that she "went frequently to the laboratory" as Sternberg had "little or no assistance, and I tried to make myself useful, for with a little instruction I had learned to make bouillon and other bacteriologic media."[3]

The leadership at Johns Hopkins, inspired and directed by Dr. Daniel Coit Gilman, had planned for medical research and education to be part of the university complex from the school's beginning in 1876. This concept and the interrelationship of medicine, science, and research are obvious today, but in the last quarter of the 19th century it was radical—even threatening—to the profession at large. Many practitioners still could not see anything practical emanating from the laboratory. Furthermore, as John Harley Warner wrote, "The laboratory, and particularly reasoning from the bench to the bedside, threatened to remove medical knowledge from the realm of common experience, not only that of the public but also that of most regular practitioners."[4] The laboratory would destroy empiricism through the remystification of medical knowledge. Little had changed from when Sternberg graduated in 1860. American colleges and universities contributed essentially nothing to medical research. Physicians who wanted to continue their medical education and stay abreast of current medical developments still had to go to Europe, yet now they booked passage to Berlin rather than Paris. In the spring of 1883, Gilman organized the nucleus of a medical faculty consisting of Ira Remsen, Professor of Chemistry, H. Newell Martin, Professor of Biology, and John S. Billings, Professor of Hygiene. For the Pathology professorship, Gilman was interested in a 33-year-old pathologist named William Henry Welch, who had studied under Julius Cohnheim in Germany and was employed at New York's Bellevue Hospital. Welch wanted to conduct original bacteriological research in his laboratory, but the Bellevue leadership ignored his proposals. Gilman was so impressed with the modest, quiet, gentlemanly pathologist after one interview that he offered Welch the position, the promise of a new laboratory near the school, and a one-year university-sponsored sabbatical to Europe to become thoroughly familiar with the latest laboratory methods and equipment. Although Sternberg did not record his initial impression of Welch, it may be presumed that these two like minds found common ground almost immediately, and rapidly developed the friendship and mutual admiration that would last for the next 31 years.[5]

While Welch was in Europe, Sternberg and his assistant, Dr. Alexander C. Abbott, continued original bacteriologic research in Martin's small laboratory. On October 17, Sternberg presented his latest paper titled "Disease Germs" to the annual meeting of the American Public Health Association (APHA) in St. Louis. The theme of the presentation revolved around variations in the natural origin and existence of microorganisms and their virulence, a topic that became more convoluted and confusing as the list of disease-causing organisms grew. Researchers around the world questioned daily where these organisms lived, external to human and animal bodies, and whether pathogenic bacteria were distinct species with permanent physiological characteristics that determined pathogenicity, or varieties of common bacteria that became pathogenic due to environmental conditions. Dr.

Henry Formad of Philadelphia had claimed that pulmonary tuberculosis could develop from injected inorganic material. Facilities available in Baltimore had allowed Sternberg to invite Formad for a repetition of the Philadelphian's experiments during the summer. The results of these studies removed Sternberg's indecision on the issue. Inorganic substances had no ability to produce tuberculosis. While he had assured himself that pulmonary tuberculosis resulted from infection with the tubercle bacillus, he also felt some additional factor had to be present for nodule formation. It was clear from his work with septic micrococci that not all animals respond in the same way to these organisms; therefore, "The supposition that…different pathogenic organisms give off different kinds of poisonous products…is sustained by what is known of the action of non-pathogenic organisms of the same class in various processes of fermentation and putrefaction, and by the facts which relate to the influence of protective inoculations and the non-recurrence of the specific infectious diseases in the same individual."[6] Although Sternberg presciently touched on natural immunity and the existence of individual and specific organism virulence factors here, they remained suppositions. It was clearly manifest to Sternberg that several pathogenic organisms lived freely in nature. Sternberg stated the following about erysipelas and hospital gangrene: "It seems to me beyond question that these diseases may…originate de novo…without direct or indirect infection from a preceding case. And hospital gangrene especially is so rare…we can…suppose…outbreaks which occasionally occur at widely remote localities are necessarily connected with preceding cases…."[7] He believed the cocci, which induced septicemia in mice and rabbits, existed in the same way, "and as regards the cholera bacillus…of Koch…there seems to be ample evidence of the power of multiplication external to and independently of the human organism."[8] Robert Koch's cholera work was new, and although he had not produced the disease in experimental animals and, therefore, had not fulfilled the postulates for which he would become famous, Sternberg thought that Koch's bacteriological and epidemiological work was solid enough to tentatively accept it.[9]

During the past summer, cholera had reemerged from Asia in the shipyards at Toulon and Marseilles and then spread into Italy. In the United States, fears of cholera generated a lot of discussion at the annual APHA meeting, and Dr. James F. Hibberd introduced a resolution to compile a formulary of genuinely potent disinfectants for rapid and efficient use by physicians and sanitarians. The proposal was approved, and a committee was appointed.[10] Sternberg was selected as chair, and the new committee met on November 20 in Baltimore. A complete and exhaustive investigation of all disinfectants and antiseptics was impractical, and, therefore, Sternberg limited the committee's work to "agents…capable of destroying the infecting power of infectious material,"[11] and those "most relied upon by sanitarians for disinfecting purposes."[12] Only the biological test of disinfecting power was employed, that is, concentrations of disinfecting agents were applied to organisms and then these cultures were observed for growth. The work was divided into two subcommittees. One committee examined the literature, abstracted and tabulated the results, and investigated the relative germicidal value of the various substances

used as disinfectants in the biological laboratory at Johns Hopkins, while the other one investigated the practical application of disinfectants on a large scale, including their cost, methods of use, chemical relations, effects on furniture or fabrics, and effects on humans and animals.[13]

Although the conclusions of the committee were not ready for presentation until the fall, preliminary reports of their experiments were prepared and released in late January and early February 1885. Much of Sternberg's work was repetition and revalidation of his earlier studies from April 1883. This allowed him time to compose a section on the destruction of cholera germs in *A Treatise on Asiatic Cholera*, edited by Edmund C. Wendt, for which he drew heavily upon the committee's preliminary work. Sternberg asked whether it was "practicable to destroy cholera germs in the alimentary canal, and thus arrest the progress of the disease, or prevent its development? And, if so what agents are best suited to accomplish this purpose?"[14] In one of the earliest published scientific discussions on specific antimicrobial therapy, Sternberg theoretically proposed that medicinal doses of mercuric chloride—the most potent germicide he had tested —should, if continually present in the intestine, inhibit the growth of cholera or any other bacteria. To test this hypothesis, he suggested—as a clinical experiment only—the use of 0.01 of a grain mercuric chloride tablets administered two at a time every five minutes for one hour, then every 10 minutes for two hours. Remembering the severe cholera epidemic at Fort Harker in 1867—and the rapid death of his first wife—he added that therapeutic success would be more likely in those treated in the early stages of the disease.[15]

In the early spring of 1885, Sternberg's laboratory work was put on hold. President Grover Cleveland designated him as the U.S. representative to the International Cholera and Sanitary Conference to be held in Rome in mid-May. The conference, stimulated by the recent cholera epidemic in Naples, was a forum for the discussion of, and agreement upon, practical sanitary and quarantine regulations to preclude such epidemics in the future. Sternberg advised on preventive and remedial measures against cholera and, because of his fluency in French, also translated the conference proceedings. With the stroke of a pen, Cleveland validated the last 17 years of Sternberg's life to the American scientific establishment. Sternberg has been called the "father of American bacteriology," but it is more accurate to say that by 1885 he was the undisputed dean of this science in the United States. The moment was not lost on Sternberg. Now the opportunity was at hand to meet with international colleagues, share ideas and laboratory techniques, and personally engage in the polemics of bacteriology that—heretofore—had only been conducted through the scientific literature. Such interaction was rewarding for him personally, and because it allowed him to dismantle some of the "geographical bias" (as he called it) that Europeans held for American bacteriological science.[16]

The Sixth International Sanitary Conference convened on May 20. As the sole U.S. delegate, Sternberg provided a brief synopsis of the previous conference in Washington to his colleagues. Presumably, he returned to his seat eager to listen to and engage in discussions of the etiology and transmission of cholera and assist in

formulating preventive strategies to preclude its dissemination. Koch's pronouncement from India that the comma bacillus was the cause of the disease was only 15 months old. The German government and scientific community had hailed Koch and the German cholera commission he led as conquering heroes, but a large part of the world remained openly skeptical as Koch had failed to produce the disease from pure cultures in experimental animals. Great Britain, however, led the most organized and vocal opposition to Koch's claims. Although cholera was a perennial health threat to British troops in India, and Britain was considered the major purveyor of cholera to the world, Britain's government had political and economic interests in India and the Suez Canal that would suffer quarantine restrictions should Koch's discovery be accepted. The potential loss was considered so great that the British government sent Dr. Emmanuel Klein, the most eminent British bacteriologist of the era, and Heneage Gibbs to India in the autumn of 1884 to conduct independent investigations to demonstrate the flaws in Koch's hypothesis. Their report, published just 2 months before the conference in Rome, stated they had found many villagers who remained disease free after consuming water from contaminated cisterns, and they maintained that until pure cultures of the bacillus produced disease in an animal model the theory remained unproven. Armed with a scientific refutation of Koch's work, the British delegation began to manipulate the direction of the conference proceedings. Britain and India were given separate voting delegations, and Dr. Jacob Moleschott, the Italian delegate and technical committee chairman, was persuaded not to include any reference to the etiology and transmission of cholera in committee discussions because it was too controversial. With Koch stifled, British and Indian delegations focused on evading quarantine regulations in Indian ports and the Suez Canal.[17]

From May 20 until June 6, delegates debated sanitary and quarantine precautions to be taken before, during, and after international travel whether on land, sea, or river. Upon Sternberg's request, Moleschott appointed a special committee on disinfectants that consisted of Sternberg, Koch of Germany, Achille Adrien Proust of France, Sir Richard Thorne-Thorne of Britain, Nikolai Eck of Russia, Georg Hoffmann-Wellenhof of Austria, and Mariano Semmola of Italy. Special regulations for transit through the Red Sea and the Mediterranean, and pilgrimages to Mecca were discussed. As in the United States during the 1870s, the majority of disagreement arose over the inherent value and duration of quarantine and the impact it had on commerce. The British and Indian consortium attempted in vain to block port inspection of ships and the disembarkation and isolation of passengers if the ship became infected. Although British medical authorities had developed and perfected sanitary surveillance and preventive measures that effectively kept cholera from Britain's shores, southern European cities did not enjoy the protection of a sound sanitary infrastructure, and saw quarantine as the only practical way to avoid cholera epidemics. A modified quarantine resolution that isolated passengers only long enough to disinfect the ship was finally agreed to, but passed by a slim margin. All delegations did agree on two issues. First, cholera invades countries not by de novo development, but as a result of human

intercourse; and second, certain local unsanitary conditions are required for the disease to gain a foothold. Considering this and yellow fever, Sternberg introduced a proposition during the last session that passed with only one dissenting vote: "The measures recommended against cholera are…applicable to yellow-fever, and to other diseases which prevail in epidemic form under…bad sanitary conditions, and which are transmitted by human intercourse. The most effectual means for preventing the propagation of diseases of this class are: The sanitary improvement…of seaport towns, and…vessels sailing from infected ports; isolation of the sick; and disinfection of infected or suspected articles and localities."[18]

The conference concluded on June 13. Overall, Sternberg was satisfied as he reflected on the work accomplished at the conference on his way back to New York. As he later commented, "Epidemics are not an unmixed evil. Indeed…they are productive of more good than harm. They call attention to sanitary sins, and lead to sanitary reforms, which…would often not be made."[19] Although no international code of sanitary regulations had been agreed to, he believed that "the interchange of opinions among leading sanitarians…the formulating of the knowledge which has been gained in the laboratory, or by the practical management of epidemics, the publication of explicit directions relating to quarantine, disinfection, municipal and maritime sanitary supervision, etc. cannot fail to be useful."[20] The conference was intense and exhausting, but fatigue was only transient. The stimulating professional interaction with men such as Koch, Ettore Marchiafava, and Angelo Celli lasted forever; that was the breath of life for Sternberg. Before he departed Rome, Sternberg was made an honorary member of the Royal Italian Academy of Medicine and given a tour of Santo Spirito Hospital by Marchiafava and Celli. Marchiafava also included a microscopical demonstration that erased any doubts Sternberg entertained about Alphonse Laveran's hypothesis on malaria. Marchiafava searched directly under the microscope for the wriggling parasite in a thin blood smear. Within a few minutes, he stepped back and allowed Sternberg to observe what he had found. "I saw the amoeboid movements very distinctly and cannot doubt that the extremely minute, transparent, and apparently structureless mass which I was looking at was, in truth, a living organism."[21] Nine months later, on March 24, 1886, Sternberg demonstrated the malarial parasite for the first time in America to Dr. Welch, Abbott, Councilman, and others in the laboratory at Johns Hopkins.[22]

In late summer, Secretary of State Thomas Bayard notified Sternberg that he would attend the follow-up sanitary conference in mid-November in Rome. The Italians had been tremendously impressed with Sternberg's professional and technical competence, hard-working nature, and tactful manner. This commendation affirmed to Bayard that the right man had been selected for the task, and he saw no advantage in changing horses in a race, which if lost, could result in a deadly victory for cholera in the United States.[23]

Sternberg was eager to return to Europe and requested five weeks of leave in conjunction with his return travel to Rome so he could visit Koch's laboratory in the Hygienic Institute in Berlin.[24] Although Sternberg was interested in studying

the latest staining techniques with Koch that could be applied to yellow fever tissue sections obtained from Havana, his real motivation for visiting Berlin had its inception in the Johns Hopkins laboratory in January 1885. At that time, he and Abbott found the micrococcus of rabbit septicemia in sputum specimens from a pneumonia patient, a significant discovery that allowed Sternberg to personally make the etiologic connection between this organism and croupous pneumonia. That theory was then being advocated by G. Salvioli and Nikolai Gamaleia in Italy; Charles Talamon in France; and Carl Gunter, Albert Fraenkel, and Carl Friedlander in Germany, who had found and described the organism in pneumonic sputum and conducted animal experiments with the organism. In 1882, Friedlander reported—and clearly described—diplococci in fibrinous exudates from lung and pleural tissues of eight patients ill with pneumonia. In November of the following year, he introduced the organism to the Medical Society of Berlin as the etiologic agent of croupous pneumonia. This announcement was based on the isolation of organisms from the lung tissues of nearly all of 50 additional pneumonia cases. Friedlander described the capsule and regarded it as characteristic of the organisms he had found. However, while his organism was lethal to mice and guinea pigs, it failed to kill rabbits. Eleven days later, Talamon presented similar studies to the Anatomical Society in Paris. He had injected pure cultures of *Coccus lanceole de la pneumonie* (*Streptococcus pneumoniae*) directly into the lungs of guinea pigs, dogs, and rabbits. While the guinea pigs and dogs showed no adverse effects, 16 of the 20 rabbits injected died, and eight of these demonstrated fibrinous pneumonia. Fraenkel presented supporting experiments to the Third Congress for Internal Medicine in Berlin on April 24, 1884. He also had found cocci in the lung sections of pneumonia patients. His organism was lethal to rabbits, but only variably so to guinea pigs. Fraenkel also argued that neither growth patterns nor capsule formation were essential characters of the causative agent of pneumonia.[25]

Sternberg had followed these developments closely with great interest. Upon reviewing Friedlander's work and comparing the German's description of the microbe with his own, he published an article confirming that the two organisms were structurally and physiologically identical and took the liberty of naming it *Micrococcus pasteuri*, in honor of Louis Pasteur. Sternberg also thought it "extremely probable…this micrococcus is concerned in the etiology of croupous pneumonia…but…cannot be considered as definitely established by the experiments which have thus far been made upon lower animals."[26] He rejected Friedlander's view that the capsule was a distinguishing characteristic of the organism because it was not constantly present. Sternberg published the first photomicrographs of capsular formation in 1881, and commented later: "The development of this external envelope of mucine…is altogether exceptional. I have not…ascertained the…conditions which control the development of this envelope, but believe it to be most marked in a rich culture-medium, and as a result of an exceptionally vigorous and rapid development of the micrococcus."[27] That Friedlander's *pneumonia coccus* was not lethal to rabbits, and only variably so in experiments conducted by Talamon and Salvioli, Sternberg explained as a variation in pathogenic power observed

repeatedly during his work with the organism. He was now eager to get to Koch's laboratory, where he could see the organism he had so recently advocated as identical with the micrococcus he had found in 1880. After four pleasant and rewarding weeks with Koch, and with slide preparations of Friedlander's pneumococcus carefully packed in his luggage, Sternberg caught the train for Rome. Upon arriving, he learned that the second conference was postponed indefinitely by the Italian government and returned home.[28]

In the spring of 1886, Mrs. Sternberg accompanied her husband on a second trip to Koch's Institute. While she enjoyed Berlin's museums and art galleries, Sternberg huddled over microscopes and culture plates with Koch and his assistants, reviewed cholera and typhoid preparations, and discussed Friedlander's organism at length. Koch was so impressed with Sternberg's self-taught laboratory skills that he admitted he could add little to them. The German master did request a demonstration of *Micrococcus pasteuri* from Sternberg's oral secretions, and Sternberg confidently consented. On the eve of the demonstration, he confided to his wife his anxiety over the event. "How dreadful I would feel," he told her, "if I have lost that germ…and could not demonstrate a thing that I have written and talked so much about."[29] His anxiety was unfounded. The micrococcus was alive and well in his mouth, and the demonstration was successful.[30]

Sternberg had returned from his first visit to Koch's laboratory with an altered opinion of Friedlander's micrococcus. His second visit confirmed—in his mind—that Friedlander's organism and *M pasteuri* were not the same. He quickly put his opinion into print, and expounded on it in another article in 1889 and in his *Manual of Bacteriology* in 1893. In doing so, he became entangled in the historical confusion over exactly what organism Friedlander was looking at and working with in 1882–1883, and who should be given credit for linking the pneumococcus etiologically with pneumonia.[31]

Sternberg had long maintained—as had Koch—that organisms appearing to be the same structurally can be very different physiologically. While he had no doubts—at least in 1885—that Friedlander was working with a micrococcus structurally identical with *M pasteuri*, by the following year he recognized "differences which [he could not] reconcile with the idea of specific identity."[32] He also maintained that pathogenic variations existed within the same species of microorganism. He had used this theory to explain the differences in experimental results—namely rabbit mortality—between Friedlander's work and his own in his earlier paper. However, after again reviewing the work of Talamon and Salvioli, and Fraenkel's most recent work from earlier in the year, Sternberg was convinced that he and these scientists were working with the same organism, and Friedlander had identified a variant of this species of micrococcus. Sternberg's about-face was based on three issues. First—and oddly enough—Friedlander's coccus was not lethal to rabbits. Second, Friedlander had injected cultures directly into the lungs of mice and still did not produce pneumonia in all of these animals. Third, the recent work of Fraenkel, who like Sternberg had found the micrococcus in his own mouth, made identical culture and inoculation experiments, noted reduced

virulence of the organism in convalescent sputum, and—through it all—had been oblivious to Sternberg's earlier work. However, other conundrums generated by pneumococcal peculiarities and another pneumonia-causing microbe confused the issue.[33]

The capsule surrounding the pneumococcus is integral to the natural survival of the organism. Composed of complex polysaccharides, this antigenic coating is the organism's primary virulence factor and protects it from being consumed by white blood cells. In vitro capsular development is extremely dependent on rather complex nutritional and environmental requirements that include protein and an increased carbon dioxide concentration with some strains. When grown on nutritionally adequate solid media, the capsule gives the colony a shiny appearance, but if not, the capsule will be smaller and virulence can be reduced or lost completely. Sternberg and others noted difficulties growing pneumococci on a variety of media, and the variations in media preparation most likely produced the differing capsular formations noted. Both Sternberg and Fraenkel were also aware that the pathogenicity of the microbe in their saliva varied at different times, and that older colonies and those that had undergone serial plating demonstrated reduced virulence. Therefore, in 1883, when Friedlander carried colonies through eight culture plate passages to ensure culture purity, he altered capsular formation drastically and significantly attenuated his cultures. The cultures were variably lethal when injected into mice, yet they had no ill effect on rabbits.[34]

Although the weak pathogenicity of Friedlander's original cultures became the anchor for Sternberg's rejection of his claims, his earlier explanation of these results would have made this an untenable position had he not observed some obvious differences among the organisms on the slide preparations in Koch's laboratory. Ironically, Friedlander inadvertently introduced these differences during his studies with pneumonic tissues in 1883. That same year his laboratory assistant, Dr. Christian Gram, developed a new staining technique that allowed pneumococci to be discerned more readily from other cellular material and debris. Gram's method became the basis of the Gram-positive and Gram-negative classification, according to the staining properties of the cell wall, which is still used today. However, this property was not appreciated until late 1885 at the earliest. In his report of March 1884, Gram remarked that he had examined sections of lung from 20 cases of fatal lobar pneumonia. Of these 20 cases, 19 remained brightly stained, but one case became decolorized. Concerning these results, he wrote: "One case of croupus pneumonia with capsule coccus. Here one finds very many cocci which do not all lie in the cell walls of the exudate. They decolorize very easily in alcohol…with and without treatment with iodine. From this case stem a great part of the cultures of Dr. Friedlander. Most of those [cocci] from animals injected and exposed to infection behave in this fashion."[35] It appears that during these experiments, Friedlander isolated what he later called Kapselbacterium, and what is recognized today as *Klebsiella pneumoniae*. Without an appreciation of cell wall staining characteristics, it is easy to understand the confusion this organism introduced into all of these studies. *K pneumoniae* is an encapsulated gram-negative rod and a bacillus,

but it can appear as a very short, fat, and rather round organism, and its capsule is thick. It, too, can be found in the mouth and nasopharynx of healthy individuals and can induce pneumonia, although much less frequently than the pneumococcus. It is evident from Friedlander's description that the organism he saw in 1882 was a pneumococcus, but whether he injected laboratory animals in 1883 with attenuated strains of pneumococci or cultures of *Klebsiella* will never be known. Fraenkel, however, continued to vie for his piece of glory in linking the pneumococcus with lobar pneumonia. When Friedlander suggested that more than one agent may be responsible for pneumonia, Fraenkel heartily agreed. Although Friedlander was correct once again, this suggestion and the negative mortality in rabbits created a suspicion among scientists that what he had originally isolated in 1882—before the advent of Gram's staining method—had been a bacillus and not a coccus.[36]

Just exactly what organism Sternberg saw—in 1885 and 1886—on the slides labeled "Pneumococcus of Friedlander" remains obscure. In his paper from 1886, "Micrococcus Pasteuri," he never confused Friedlander's organism with a Gram-negative bacillus, but maintained his opinion that it was a variant micrococcal species that may cause pneumonia. However, seven years later in his *Manual of Bacteriology*, Sternberg rewrote his part in the pneumococcal controversy when he stated: "I fell into the error of inference, previously made by…others, and assumed that the 'pneumococcus' which Friedlander had obtained from the same source was the same, although I found it difficult to reconcile the experimental data, inasmuch as he had obtained uniformly negative results in his inoculations into rabbits. To explain this discrepancy I suggested that Friedlander's pneumococcus was probably a variety having a different degree of pathogenic power…. This supposition seemed to find support in the fact…that my Micrococcus Pasteuri became attenuated, as to its pathogenic power, when the cultures were kept for some time; and…there seemed…to be different pathogenic varieties in the buccal secretions of different individuals. At this time I had not seen a culture of Friedlander's bacillus. Later, in the autumn of 1885, when I made its acquaintance in Dr. Koch's laboratory, I recognized my mistake and hastened to correct the error."[37] Sternberg quoted Gameleia as saying: "As to the researches of the authors who preceded Fraenkel, it is sure that the microbe which they often found in sections of diseased lungs, and which they called the microbe of Friedlander, was in fact the microbe of Pasteur, since it was colored by the method of Gram, which decolorizes the bacillus of Friedlander. Many of the positive results…which have been reported relative to the last-mentioned microorganism, ought to be put to the account of the other."[38] To this Sternberg added, "This opinion the present writer has entertained since his researches made in 1885."[39]

Sternberg's comments from a paper written in 1889 and his manual in 1893 are difficult to reconcile with his earlier papers. He obviously found something amiss upon scrutinizing the slides and cultures of Friedlander's organism in Koch's laboratory, but he never defined what it was. Sternberg disregarded the growth characteristics noted by others, discounted the significance of capsular formation,

never differentiated the microbes he reviewed on the basis of Gram staining, and, in 1886, still referred to Friedlander's organism as a coccus. Furthermore, if he did entertain Gameleia's idea in 1885, he did not make that opinion public. More accurately, Sternberg considered Friedlander's experimental results less than robust, whereas he was tremendously impressed with the research of Talamon and Fraenkel. He may have demonstrated some bias in favor of Fraenkel, although he would have been horrified at the accusation, because of the similarities with his own research that Fraenkel obtained independently. However, he maintained it was Talamon—not Fraenkel—who first demonstrated the etiologic relationship of the pneumococcus to lobar pneumonia.[40]

By the time Sternberg made his second trip to Berlin, a new and spacious laboratory facility was under construction at Johns Hopkins University. Welch, who had returned from Europe in October 1885, had the two-story morgue on the downtown campus renovated to adequately accommodate students closer to the hospital wards. Martin's small laboratory was moved into the Old Pathological—as the building became known—and immediately went to work during these renovations. Welch and his assistant, Dr. William T. Councilman, along with Sternberg, Abbott, Martin, Franklin P. Mall, and E. Meade Bolton, prepared lectures and laboratory exercises for two postgraduate courses, pathological histology and bacteriology, to be offered to physicians beginning in February 1886. The primary purpose of this facility, however, was for bacteriological research, not teaching. During the spring and summer of 1886, Sternberg published a review article on studies of the typhoid bacillus (*Salmonella typhi*) and commenced experiments on the thermal death point of microorganisms. These experiments provided sanitarians with the exact temperature required to destroy organisms, such as typhoid and cholera, in the excreta in patients, infected clothing, and drinking water. After Sternberg taught his laboratory colleagues how to find the malaria parasite in stained blood smears in March, Councilman began to study the plasmodium in earnest. At this time, malaria was endemic in Baltimore during the summer months, and he had no difficulty obtaining blood specimens for his work. At the inaugural meeting of the Association of American Physicians in Washington in mid-June, Councilman presented "Certain Elements Found in the Blood of Malarial Fever." Once he had finished his remarks, Dr. William Osler voiced his skepticism because Councilman had not verified all of Laveran's claims. Osler, then at the University of Pennsylvania Medical School, was becoming a leader in the world of clinical medicine and one of the most experienced physicians in microscopical studies of blood. He stated he had studied a handful of malaria cases and believed the amoeboid bodies to be nothing more than vacuoles in the red blood cells. His words carried significant weight and authority. As Councilman's own doubts about the cause and effect of malaria then became apparent, Sternberg rose from his seat in the audience. He stated his hearty support for Laveran's work, and—without pretense or arrogance—pointed out if Osler had stained his blood preparations, he, too, would be convinced that the vacuoles were malarial parasites. After further investigations, Osler saw the error of his observations and stated later that at the time of the

meeting he had spoken "in the fullness of his ignorance."[41]

The Johns Hopkins Laboratory, dedicated primarily to original medical research, was now a reality. However, not all of the American medical research visionaries resided in Baltimore. Other laboratories were becoming established, but they were significantly influenced by Welch's ideas, methods, and actions. The pathological and bacteriological laboratory he created at Bellevue had stimulated the alumni association of Sternberg's alma mater to create a facility of its own under the direction of Dr. Francis Delafield and his assistant Dr. T. Mitchell Prudden. Dr. Frederick Dennis asked Andrew Carnegie to give $50,000 to build a pathological teaching laboratory in New York City. This facility opened in the spring of 1885 under the direction of Dennis, Dr. Edward G. Janeway, and Dr. Hermann Biggs. The idea for another purely bacteriological research laboratory evolved in the mind of Cornelius N. Hoagland the same year. Sternberg played a key role in the design, development, and success of this Brooklyn facility.[42]

Cornelius Hoagland, a physician, gave up practice after the Civil War to become a millionaire, along with his brother, Joseph, producing baking powder in New York City. He probably would never have thumbed through another medical journal had diphtheria not killed his oldest grandson—whom he adored—in December 1884. Jolted from a life of leisure, Hoagland was determined to put money and energy into a medical endeavor with the potential to reduce—perhaps even eradicate—childhood mortality from infectious diseases. With advice of physicians in New York, such as Dr. Joseph H. Raymond, Hoagland convinced Long Island College Hospital to accept sponsorship of a bacteriologic laboratory. In recognition of Sternberg's standing in the field, he also was determined to recruit him as director.[43]

Hoagland was favorably impressed and directed Raymond to draft a proposal that would make Sternberg reconsider staying in Baltimore. Their initial correspondence has not survived, but Raymond's proposal was apparently robust enough for Sternberg to indicate a definite interest. In a letter dated November 14, Sternberg explained what he could provide the laboratory and the compensation he expected, but career desires and Army politics kept him from immediately accepting the position:

> "This much…I can promise. I will give you a course of ten lectures on bacteriology during the winter of 1887–1888 for $500 – paying my own expenses and if I am still stationed in Baltimore or Washington, will go to Brooklyn for this purpose at such times as you may arrange. I will also accept the position of director…in the laboratory and will give as much time as I can to the students who wish to take a practical course in bacteriology. I could have an assistant upon the spot who could be instructed by me there or could come here for a practical course (four weeks or more.) If I should be stationed in New York Harbor I would be able to give more time to the laboratory work and I think you ought in some way give me a salary of $1000 at the outset to be increased if the school is prosperous and if my connection with it should prove advantageous to it."[44]

As in the fall of 1883, Sternberg was again trying to gain access to the inner circle of the Surgeon General's Office as the administration changed. Surgeon General Murray retired in August. Colonel Jedidiah H. Baxter became the acting surgeon

general during the ensuing political and highly partisan struggle by candidates for that office. The internecine strife raged for three and one-half months until President Cleveland abruptly ended it by appointing Lieutenant Colonel John Moore to the post on November 18, 1886. Regrettably, neither Sternberg's earlier biographers nor his personal papers indicate what position he sought in Washington. It was one that he believed he had an excellent chance of securing if Moore was selected as surgeon general and one that would allow him to continue bacteriological research either at the Army Medical Museum or at Johns Hopkins. Immediately upon assuming office, Moore announced that Baxter would remain in place and two other "strong and remarkable" assistants, Majors Charles Greenleaf and Charles Smart, would join him "for the upbuilding of the medical service."[45] If Sternberg was vying for one of these positions, his friendship with Moore and his political acumen were inadequate. In late November he informed Raymond if Hoagland accepted his terms he would assume the directorship from Baltimore. Four days later, Raymond responded affirmatively and requested Sternberg's advice on floor plans and laboratory apparatus.[46]

The Hoagland Laboratory would consist of four departments: (1) bacteriology under Sternberg, (2) physiology under Raymond, (3) histology and pathology under Frank F. Ferguson, and (4) photomicrography under Hoagland. All of these directors, except for Hoagland, needed assistants to ensure practical laboratory demonstrations were appropriately prepared for the students. Sternberg especially required a man well qualified in bacteriologic techniques because of his long-distance teaching. Hoagland suggested one man could assist both Raymond and Sternberg for the meager salary of $600 per year. Hoagland did not become a millionaire without a bit of parsimonious penny pinching, but his expectation of obtaining a competent and diligent physician to serve two masters was ludicrous. Sternberg argued that two assistants were required because it would be nearly impossible to find a man qualified in both fields. He suggested that since pathology and bacteriology were overlapping fields to some extent, Joshua M. Van Cott, assistant director of histology and pathology, could assist Ferguson and himself, and another man could be found for Raymond. Unfortunately, Van Cott balked at the idea. Bolton, one of Sternberg's assistants at Johns Hopkins, was mentioned as a very qualified candidate, but Bolton had accepted a position at a southern medical school for $2,500 per annum. Sternberg then considered Mall, another one of his associates in Baltimore who was then a fellow in pathology at the university and had an outstanding background in pathology, physiology, and bacteriology. Mall was interested, but not for the pittance Hoagland was offering. As 1887 arrived, the frustrating matter remained unsettled. However, medical issues of a more immediate national and international concern pulled Sternberg away from Brooklyn and Baltimore once again.[47]