MEMOIRS

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Chapter I

Education

Until my academic education was completed, I seemed never to have an opportunity to make a choice in matters concerning my future. I always stepped into the only suitable opening I could see on my horizon. Emerson described this kind of progression: "Each man has his own vocation … there is one direction in which all space is open to him. He has faculties silently inviting him thither to endless exertion. He is like a ship in a river; he runs against obstructions on every side but one, on that side every obstruction is taken away and he sweeps over a deepening channel into an infinite sea." I always thought that somehow, I drifted into the work for which I was best adapted.

I grew up on farm in a Welsh community of northern Pennsylvania where my paternal grandparents settled when they came from Wales in 1831. I received my primary education in the little country schoolhouse where we had good teachers most of the time. Since there was no high school in our rural district, I went to Towanda, the county-seat, to obtain secondary education in one of those private schools called institute or academy that passed out of existence around the turn of the century as high schools multiplied. My class of seven was the last to graduate from the Susquehanna Collegiate Institute. Dreams of going to college were shattered by lack of means.

Because teaching was almost the only profession open to a woman, I had no thought of doing anything else. I taught grades 1-4 two years in the home school, and two years in a neighboring school. Although the personalities of the children provided interest, the subject matter became boring as it was repeated year after year. I was glad when I found a way to escape. I heard about a two-year course given free of tuition to rural teachers at the College of Agriculture of Cornell University. It was the idea of Liberty Hyde Bailey, Dean of the College, to foster in school children a love of Nature by teachers

trained to develop their interest in plants and animals and in the events of the inanimate world. He wanted farmers and their families to enjoy country life.

With my savings from four years of teaching I went to Cornell for the course in Nature study which, during its two years, included the freshman courses in botany and zoology required of the regular students in agriculture. I also studied entomology, ornithology, geology, and meteorology. The lectures were given by the distinguished professors of those subjects, most notable among them being Burt G. Wilder, then approaching retirement. He was a member of the original Cornell faculty, appointed in 1867. Dean Bailey, who was a poet as well as scientist and administrator, taught a course which was aimed to help us to see the beauty of Nature—to surmount our scientific knowledge with esthetic sense. On Sunday evenings Dean and Mrs. Bailey opened their home to students and he read to them, sometimes from his own poems.

When the course in Nature Study was completed, I was no longer interested in obtaining the certificate to which I was eligible. My interest in science had been wetted by the basic courses I had taken, and I wanted to continue the study of science—any branch of biologic science would satisfy me.

In order to develop leadership for the country's expanding agriculture, Cornell's College of Agriculture was accepting to its regular course, free of tuition, students from outside the state. (The College of Agriculture and two or three other colleges of Cornell University are supported by the State of New York, although most of the colleges are endowed and in these colleges students pay tuition.) With no tuition fees to pay, and with the help of a Roberts Scholarship, I was able to complete the requirements for the degree of B. S. in Agriculture during the next two years.

Only one branch of pure science was available as a major subject to senior students in agriculture. They were required to specialize in some branch of applied science such as horticulture,

dairying, etc., or they could specialize in bacteriology. As I was not interested in applied agriculture, I had to take bacteriology, and that was perfectly satisfactory to me.

During the first decade of this century bacteriology was a young science which needed the kind of encouragement that was given at Cornell University by offering it as a major subject free of tuition. My professors were W. A. Stocking in dairy bacteriology, and Veranus A. Moore of the Veterinary College, who taught a course in general bacteriology to students of any of the colleges.

At the end of my senior year Prof. Stocking told me that he had received a letter from Prof. E. G. Hastings of the Department of Bacteriology, College of Agriculture, University of Wisconsin, asking him to recommend a graduating student to be recipient of a scholarship in bacteriology. Prof. Stocking asked me if I would apply for it. Thus, a crack in the door to a career in bacteriology opened unexpectedly.

This University Scholarship was given in alternate years to a graduate student specializing in agricultural chemistry or bacteriology. It had never before been held by a woman. Before I applied for it the information had leaked down to me that the College of Agriculture wanted to develop an instructor for the course in bacteriology to be given to the students of domestic science. In outlining my course of study for the master's degree, Prof. Hastings noted that I had a good background in biology, and he suggested that my knowledge of chemistry should be strengthened. He recommended a course in the chemistry of nutrition and an advanced course in organic chemistry. Thus, although I held the scholarship in bacteriology, I spent two-thirds of my time studying chemistry. It was many years later that physics was added to the curriculum of a bacteriologist. Dr. Elmer V. McCollum, who taught the course in the chemistry of nutrition, was a great teacher. At that time, with the aid of an assistant he was conducting experiments which led to his discovery of Vitamin A, and the class heard much about “fat-soluble A” and "water-soluble B". (The word “vitamin” had not yet been coined.)

A few years previously, Dr. J. McCollum had been a member of the team at the University of Wisconsin which carried out an experiment on groups of cattle to determine whether corn, wheat, and oats were similar in terms of nutrients. Striking differences were found in the nutritive value of corn and wheat, and this result raised other questions. Dr. McCollum became convinced that the problems of nutrition could be solved only by the purified-diet method, and with the aid of an assistant he undertook experiments with rats, which resulted in his announcement in 1913 of the discovery of Vitamin A.

Stimulating and exacting as these experiments were, Dr. McCollum gave generously of his time to his class of four graduate students. He came regularly during laboratory periods to spend some time explaining, less formally than in his lectures, any point on which he thought we might not be entirely clear. Often his teaching was in conversation with individual students.

One day Dr. McCollum said that his experiments were so demanding of concentrated thought that he could not stop thinking about the problems at night, and it interfered with his sleep. Occasionally he showed the strain of his long working hours. I remember that one day while he was lecturing, I turned back a page of my note-book to refer to some note that I had made. He stopped, saying that he would proceed when he had the attention of all the class. At the end of the school year Dr. McCollum asked me if I should like to continue my studies towards obtaining the Ph.D. degree. He said he thought he could obtain for me the university fellowship in chemistry. Although I realized that his offer presented a great opportunity, for several reasons I did not apply for the fellowship. The five years in college had been a financial strain, and a physical strain, too, for each year I had earned money to pay a part of my expenses. I wanted relief from the strains and besides, I did not consider myself so well-prepared for chemistry as for bacteriology, either by inclination or through preparation. Moreover, I had another offer to consider.

Chapter II

I Become a Federal Civil Service Employee

At that time the Dairy Division of the Bureau of Animal Industry (now the Bureau of Dairy Industry), U. S. Department of Agriculture, was expanding its research personnel, although laboratory space in Washington was limited until the east wing of the building occupied by the Department of Agriculture (the white marble structure north of Independence Avenue between 12th and 14th Streets, N.W.) should be completed. Until laboratory space should become available, research was conducted on a cooperative basis in collaboration with several state experiment stations. The Dairy Division paid the salary of the investigators and the state provided laboratory facilities and direction of the investigations. The federal civil service scientist was selected by the professor who was in charge of the project. At the University of Wisconsin, the departments of chemistry and bacteriology were together collaborating with the Dairy Division on investigations into better methods of cheesemaking, an important industry of Wisconsin.

Apparently realizing, after my two terms of study, that I was better adapted to research than to teaching, Prof. Hastings offered me the position of bacteriologist representing the Dairy Division on a team which was working on the improvement of flavor in Cheddar cheese. I accepted the offer and began work on July 1, 1910.

Three more years passed on the campus of the University of Wisconsin. My work was interesting, but not so stimulating as most of my later, more independent investigations. Four papers were published on which my name appeared as joint author with Prof. Hastings, or with Prof. Hastings and Dr. E. S. Hart of the Department of Chemistry.

Each year I took one university undergraduate course in a language, to fill a gap in my education. I began the study of German and continued it through two years, in preparation for reading the

language which at that time covered more completely than any other the current reports of bacteriologic research. (The Germans lost their leadership in bacteriology during World War I.)

Early in 1913 I was informed that the laboratories of the Dairy Division would soon be ready for occupancy. Then an order went out to all of the cooperating scientists to come to Washington to take up their work in the new building on July 1. I regretted leaving Madison, where I had enjoyed the life of the university community, but I wanted to hold my position. So off to Washington I went.

On my way I stopped for a few hours in Chicago, to visit the Department of Bacteriology of the University of Chicago. I was looking forward to further graduate study, and thought I could consider the possibilities at Chicago better if I had seen the laboratories and if I had met some of the professors. Dr. Norman Mcleod Harris, then an assistant professor, showed me around and afterwards we sat down for a little chat. He asked me where I was employed, and I told him I was on my way to Washington where I had a position in the Dairy Division of the Bureau of Animal Industry. “Well!” he said, "I am surprised to hear you say that, for I was there only a few weeks ago and I was told that they did not want any women scientists." I do not remember what my response was, but I think that my demeanor was stoical.

That night, speeding along on the train, the absurdity of my situation did not seem funny. I was on my way to Washington, where I had not wanted to go, and where I was not wanted. I arrived on July 1, 1913, on one of the summer's hottest days.

Although several women scientists were employed in the Bureau of Plant Industry, U. S. D. A., only one had preceded me in the Bureau of Animal Industry. She remained only a year or two, in the Division of Pathology, and left before I came. I wonder how she happed to be admitted. In my case, admission was by accident, for the B. A. I. officials had failed inadvertently to protect themselves against the admittance of women. They had left a loophole in the barrier, and I had entered through it unwittingly. When the arrangement was made for the professor in charge of a cooperative investigation

to select the U.S. civil service employee, the thought had not occurred to them that a woman might be chosen. It was an important matter, for a U. S. civil service employee may not be discharged unless a serious complaint be made against her. According to hearsay, when the bad news broke at a meeting of B.A.I. officials that a woman scientist would be coming to join their staff, they were filled with consternation. In the words of a stenographer who was present, "they almost fell off their chairs.”

Chapter III

Investigations at Dairy Division, Bureau of Animal Industry, U.S.D.A.

Despite the inauspicious circumstances of my arrival, I found the Dairy Division to be a good place to work. My immediate superiors, Mr. B. H. Rawl, Chief of the Dairy Division, and Dr. Lore A. Rogers, in charge of research, seemed not to share the antagonism of the higher officials toward women scientists.

In progress were investigations of the methods of manufacture of butter and cheese, the effect of pasteurization on milk, and other problems of dairying which depended for solution upon a knowledge of basic sciences. This led to investigations of theoretical subjects.

Chief among the scientists was Dr. Rogers, a bacteriologist, whose studies extended beyond the problems of dairying into such subjects as the oxidative degeneration of fats, and the principles underlying the preservation of life in bacterial cultures by the freezing-high-vacuum method.

The bacteriological investigations in progress when I entered the Dairy Division included a search for the sources from which bacteria entered dairy products, and this led to a study of techniques by which strains of bacteria cou1d be identified.

One line of investigation was concerned with finding a method for the identification of bacteria of the coli-aerogenes group. In determining the sanitary quality of both water and milk, it was a matter of great importance to know the origin of bacteria of these types, because the presence of bacteria of the coli type serves as indicator of potentially dangerous contamination, whereas bacteria of the aerogenes type are widely distributed in nature. These studies required the collaboration of a chemist and a bacteriologist. Dr. William Mansfield Clark was the chemist throughout the studies which were

reported in a series of four papers by Rogers, Clark, and the bacteriologist as junior author. My first assignment was to do the bacteriological work on the second and third phases of the investigation.

The chemical aspects of these studies yielded significant findings, which led into a field of investigation in which Dr. Clark became a distinguished leader. Following a suggestion made earlier by Theobald Smith that the ratio of carbon dioxide to hydrogen in the gas produced in fermenting cultures of the coli-aerogenes group of bacteria might indicate fundamental differences of metabolism, Rogers, Clark, et al. found that in the anaerobic fermentation of dextrose, bacteria of the coli type produce the two gases in nearly equal amounts, whereas those of the aerogenes type produce a larger proportion of carbon dioxide varying considerably, but approximately in a ratio of two to one. This sharp differentiation by means of the gas ratio was found to be correlated with the ability of the strains to ferment nine selected test substances.

In these studies, Dr. Clark 's attention was directed to the effect of acid on the growth of bacteria. He found that it is the intensity of the acid in terms of hydrogen-ion concentration that affects their growth. But existing methods of measuring acidity determined the quantity, not the intensity of the acid. Next, with his collaborators, Dr. Clark developed accurate methods for measuring hydrogen-ion concentration. These methods replaced the inaccurate titration method of determining acid content in use in biologic laboratories throughout the world. Also, they were found to be applicable in many industries, and other processes in which they came into wide usage.

Dr. Charles Thom was another colleague in the Dairy Division whose work there opened the way into his chosen field, in which he became distinguished. For a number of years, he studied the effect of fungi, particularly those of the genus *Penicillium*, on the flavors of ripening cheese. He left the Dairy Division in 1914, but he remained in other branches of the Department of Agriculture until his retirement, becoming an authority in matters pertaining to mycology. His final contribution was to

medical science when he played a major role in making possible the massive production of penicillin—a product of the growth of *Penicillium notatum*— during World War II and thereafter.

As my work with Dr. Clark did not fill all my time, I was given another problem, and this one was my very own. I was assigned the task of studying the bacteria that occur in freshly drawn milk—those that multiply within the udder and are excreted in the milk. This study gradually absorbed more and more of my time and interest.

When Dr. Thom left the Dairy Division, I was asked to prepare to take over his position as mycologist. It was a large order, for my training in microbiology had not included a course in mycology. I was encouraged to go to the University of Chicago to devote one term to the study of mycology. I followed that advice, and found time while in Chicago to obtain cultures from samples of milk drawn aseptically from the cows of herds supplying the Chicago area, thus widening the sources of the cultures of my collection.

My memory is dim about the use I made of my newly acquired knowledge of mycology, but I remember that my duties along this line did not interfere greatly with progress in the research in which I was keenly interested. Wisely, Dr. Rogers gave only general directions to his research workers, leaving them free to follow the leads that their own observations detected. Seeing my interest in the problem of the bacterial content of freshly drawn milk, he gave me plenty of time to follow this line of investigation.

Chapter IV

The Beginning of Studies on Brucellae

The bacterial flora of the udder is varied, with several species commonly present. My attention was gradually focused on one particular species, the causal organism of contagious abortion, now designated *Brucella abortus*. For several years it had been known to exist in the udder of infected cows, having been obtained from samples of milk from apparently healthy animals by Schroeder and Cotton of the Division of Pathology, B. A. I., and reported in 1911. Almost simultaneously Theobald Smith and Fabyan obtained *Br. abortus* from cows' milk and reported their findings in 1912. Both reports included a warning of the possibility that these bacteria might be dangerous to human health.

The idea of drinking milk contaminated with bacteria capable of causing disease in animals was distasteful to me. I wanted to know more about these organisms. Especially I wanted to know whether they were related to any species known to cause human disease. The pursuit of this information disclosed problems which required the work of many investigators to resolve.

Deliberating about the nature of *Br. Abortus* I searched for clues that might throw light on its relationship to other pathogenic bacteria. Thinking that Dr. Eichorn, Chief of the Division of Pathology, B.A.I., might be able to give me suggestions, I went to his office to ask if I might discuss my problem with him. I had chosen the right person to consult, for when I asked him if he knew of any apparently healthy animal that excreted in its milk bacteria pathogenic to man, be told me that milk from apparently healthy goats carried the germs of human undulant fever. This fact gave me an idea which added to my zeal for the study of brucellae.

Insofar as I know, only one person in the world ever claimed that he had thought of a possible relationship between the causal organisms of human undulant fever and bovine contagious abortion.

This claim was made by a German, who stated that his work was interrupted by World War I, which prevented the publication of his findings. Shannon, a fictitious character in A.J. Cronin's novel *Shannon's Way* (1948), studied this problem successfully, and was ready to publish his results, when a friend told him that an American woman had recently published the facts that he had labored so hard to discover. The news crushed him, as was befitting in a novel.

There was a reason why bacteriologists had never noticed a resemblance between bacteria from the two sources. Bruce, the discoverer of the causal organism of undulant fever, considered it to be spherical, and placed it in the genus Micrococcus with other spherical species. But Bang, the discoverer of the causal organism of bovine contagious abortion, considered it to be rod-shaped, and he placed it in the genus Bacillus with other rod-shaped species. It would be heresy in those days for a bacteriologist to think of a relationship between a micrococcus and a bacillus, but the fact was found to be that the morphology of the bacteria from the two sources is similar. The cells are rod-shaped, dividing into varied lengths, some so short that under the microscope they appear to be spherical. In some cultures, depending on conditions of growth, these roundish forms predominate.

That *Micrococcus melitensis* and *Bacillus abortus* both infected the udder of apparently healthy animals was a compelling reason, I thought, to compare them and to try to find other characters that might be common. I obtained six strains of *M. melitensis* for study. One was originally obtained from a patient in London twenty-one years previously, and five strains, without record as to whether of human or caprine origin, were dated from ''prior to 1900" to 1909.

Comparing the strains of bovine and human origins I was amazed to find that all behaved alike in the tests available in those days for bacterial identification, except that the strains of human or caprine origin produced a brown discoloration of the culture medium after long incubation. All strains

agglutinated in antiserums produced by strains of bovine origin, and all agglutinated in an antiserum produced by the strain of human origin.

I appealed to the scientists of the Division of Pathology to carry out a comparative test on pregnant guinea pigs. (At that time, I had done no work with animal experimentation.) The animals inoculated with the strain of human origin, which had been maintained on culture medium for years, aborted as promptly as those inoculated with a strain freshly isolated from a bovine fetus. Two days later one animal of each group was killed and brucellae were cultivated from the organs of both animals. Eight days later another animal of each group was killed and the blood serum was tested for agglutinins. It was found that the agglutinin-absorption test could distinguish the strains of different origins. Any strain would absorb a greater amount of agglutinins from the serum of an animal that had been inoculated with a strain of homologous origin than would absorb from the serum of an animal that had been inoculated with a strain of heterologous origin. These findings were reported at the annual meeting of the Society of American Bacteriologists held in Washington, D. C., in December 1917, and they were published in the July 1918, number of the Journal of Infectious Diseases.

Discussing the results of the investigation I commented: "Considering the close relationship between the two organisms, and the reported frequency of virulent strains of *Bact. abortus* in cows' milk, it would seem remarkable that we do not have a disease resembling Malta fever in this country." And I raised a question: "Are we sure that cases of glandular disease, or cases of abortion, or possibly diseases of the respiratory tract may not sometimes occur among human subjects in this country as a result of drinking raw cows' milk?"

The reaction to this paper was almost universal skepticism, usually expressed by the remark that if these organisms were closely related, some other bacteriologist would have noted it. This was not valid criticism, and it did not disturb me, for it would take only a few hours of work for any bacteriologist

who had cultures of brucellae at hand to test the accuracy of my report. I knew someone would do that before long.

With my paper in the hands of a publisher, obviously it would be wise to let the controversial subject rest awhile. Moreover, circumstances almost demanded a change, for World War I was in progress, and everyone was thinking about what he could best do to "help win the war."

Before these memoirs on the Dairy Division close, I want to express my appreciation of the support I received throughout my investigations into the unbelievable with findings of a nature that could be hardly acceptable. My superiors must have foreseen better than I that my findings would lead to resentment among some members of the industry which the Dairy Division was organized to serve, for the B. A. I. was then carrying on its campaign for the eradication of tuberculosis from the cattle of this country. Sometimes there was trouble when an owner could not understand why an animal should be slaughtered when it had the disease in an incipient stage, detectable only by the tuberculin test.

Nevertheless, no restraint was placed upon me; all my needs were cheerfully supplied; even the scientists of the Division of Pathology responded willingly to all my requests for help in carrying out experiments on animals, though they frankly expressed skepticism. My chief, Dr. Rogers, must have been responsible for smoothing the way for the prompt approval for publication of my reports. If anyone in higher administrative office demurred, I never heard about it. Also, the late Dr. [Ludvig] Hektoen, editor of the *Journal of Infectious Diseases*, accepted my controversial paper and published it promptly. (He will appear again in these memoirs as a good friend.) Everyone on whom I was dependent in this work was a true scientist, steadfast in the search for knowledge regardless of how unpalatable it might appear to be.

In transferring from the Dairy Division, I had to resign from the position of Dairy Bacteriologist. A letter from Mr. Rawl, Chief of the Dairy Division, enclosed a resignation blank which I was asked to fill

out and return. I still cherish the final sentence of his letter: "I am asking you to do this with the understanding that when you are ready to return to this division, we shall ask for your transfer." I never requested the return transfer because my interest in brucellae shifted from the causal organism to the disease itself. The Hygienic Laboratory was the place to study brucellosis.

Chapter V

Early Years at the Hygienic Laboratory

On inquiry at the Hygienic Laboratory whether my services might be useful there in work connected with the war, I learned that a position in bacteriology was open. I applied for it and was accepted. The work was with a team of doctors who were working on the improvement of the antiserum used in the treatment of epidemic meningitis, one of the dread diseases of World War I, with a fatality rate of more than 50 percent in some outbreaks.

When I entered in April 1918, the old red brick building at Twenty-fifth and E Streets, N. W. housed the entire institution. Counting everyone from the director all the way down, the personnel numbered less than one hundred. The Laboratory's equipment was simple, although it compared well with that of other laboratories. Thrift in the use of supplies was necessary, for the animal budget was modest. In 1920 a second building was added, and fourteen years later it seemed like a big expansion when two more new buildings came into use.

I was fortunate in being a member of the Division of Pathology and Bacteriology of the Hygienic Laboratory—renamed the National Institute of Health in 1930—during almost nineteen years of the directorship of Dr. George McCoy. These years are generally regarded as a brilliant period of the institution. Since Dr. McCoy was also in charge of the Division of Pathology and Bacteriology, those of us in that Division were doubly fortunate in working directly under him.

In those early years a number of distinguished medical scientists gave luster to the Laboratory, with Dr. McCoy at the top of the list. He was an acknowledged authority on leprosy. While he was Director of the U. S. Plague Laboratory in San Francisco, in a search for the causal organism of plague in the wild squirrels of California, he discovered that the ground squirrel was responsible for the spread of a “plague-like disease", to which the name tularemia was given later. At the Hygienic Laboratory he

continued active research on problems connected with various diseases, in addition to his administrative duties.

Dr. Charles W. Stiles was one of the older scientists at the Laboratory, with his major achievement, the freeing of the southern rural areas of the United States from hookworm disease, already a brilliant, completed chapter of American medical history.

Occasionally, Dr. Stiles' unconventionally added a little spice to the life within the old brick building. The following incident is characteristic: One morning I was walking down New Hampshire Avenue to the Laboratory, and as I crossed Washington Circle I had to wait, as usual, for a chance to pass through the heavy east-bound traffic, for this was before the time of traffic lights on Pennsylvania Avenue. As I stood there, Dr. Stiles caught up with me, and together we waited. He was wearing his Khaki uniform, as was his custom, although other medical officers wore their uniforms only when necessary; and he was carrying a walking-stick. Becoming impatient with the wait, he stepped into the street and held up his cane signaling the traffic to stop. It stopped, and we proceeded.

Although Dr. James P. Leake had been at the Laboratory for a number of years before I came, he was still a boyishly vigorous young man who never walked upstairs, always ran two steps at a time. He was often away, responding to a call for help from a community where disease had assumed epidemic proportions. His studies added valuable information to the understanding of smallpox, cerebrospinal meningitis, tetanus, and typhoid fever. His greatest contribution was to the knowledge of poliomyelitis. Quoting from Williams, (1) "Probably no one before him or since has had as complete an understanding of the total research, administrative, and clinical problems of poliomyelitis."

Dr. Leake contributed more to the Laboratory than his work as an expert epidemiologist. He added a unifying, humanizing influence as he made the newcomer feel at ease in the group. He and Mrs.

Leake extended gracious hospitality in their home, and he was always alert to see when he could be of service to any of us when help was needed.

When I entered the Hygienic laboratory, I found two women well established there. Another woman scientist, Mathilde L. Koch, had come and gone, but apparently her assignment was temporary, for her name was never listed with those of members of the laboratory corps. Miss Koch, a pharmacologist, was a niece of Robert Koch. She published two technical papers in Hygienic Laboratory Bulletin 103, dated February 1917, as senior author in collaboration with Dr. Carl Voegtlin.

Dr. Ida A. Bengtson was the first woman scientist to be listed as a member of the laboratory corps. When she came in 1916, she had nearly completed the requirements for the Ph.D. degree from the University of Chicago. She received it in 1919, with bacteriology as her major subject. She was an adaptable, efficient, indefatigable worker. Of a quiet, gentle, friendly nature she was readily accepted as a member of the staff. During her early years at the Laboratory she collaborated with Dr. McCoy on various problems, her name appearing as that of junior author on their reports. She was a good teacher, and for several weeks annually for many years she had opportunity to exercise that talent as one of the instructors of the orientation class of incoming medical officers.

Dr. Bengtson made a prolonged study of three subjects: anaerobes and their toxins, the etiology of trachoma, and rickettsia. In her studies on anaerobes she experienced the thrill of discovery early in the 1920' s when she identified a new variety of *Clostridium botulinum*, which produces toxin C. She had isolated this anaerobe from the larvae of a green fly. Its toxin was responsible for an outbreak of a paralytic disease (limberneck) of chickens. The sender thought that a study of the material would lead to the discovery of the cause of poliomyelitis.

In 1937 Dr. Bengtson was assigned to the study of rickettsiae, and she remained a member of the "typhus unit" until her retirement. Her work on the tissue culture of the rickettsia of typhus was

important in the subsequent development of the vaccine used for the protection of our troops against typhus in World War II. During the war she worked long hours on problems concerning the production of the vaccine, and on problems concerning the detection and identification of Rickettsial infections. On weekends she found diversion on her big farm in the foothills of the Blue Ridge Mountains of Virginia. Retiring in 1946, she lived only a few years to enjoy her leisure.

During the twenty years that followed Dr. Bengtson's admission to the Hygienic Laboratory, twelve or more women scientists came. In obtaining their positions it was well for all of them that the pioneer woman had filled her position so capably.

Before Dr. Bengtson's arrival, Rose Parrott ("Polly" to all of us) came to the Laboratory as a nurse to assist in a study comparing raw and pasteurized milk as a food for infants. Her name never appeared on the list of Laboratory personnel, but she was so much a part of the life of the institution that she should have a place in these memoirs.

She was young, animated, beautiful, and skillful. With these qualities, of course she was popular. When the study which brought her to the Laboratory was completed, she remained to become an expert technician. She assisted in various investigations until one day in 1944 she became accidentally infected with a culture of *Pasteurella tularensis*. She died of tularemia a few days later.

In 1920 Dr Edward Francis came to the Laboratory to continue his studies on tularemia. Previously, he had been sent to Utah to investigate an outbreak of "deerfly fever." He recognized it in experimental animals as the same disease that Dr. McCoy had described as a "plague-like disease of rodents". The problems involved in establishing *Pasteurella tularensis*, the causal organism of the disease in rodents, as the causal organism of tularemia in men kept Dr. Francis busy for many years.

About 1922 a number of young medical officers came to the Laboratory where they remained for the completion of their careers.

Dr. Joseph Goldberger had already studied pellagra for years, and he had announced his theory that it was a dietary deficiency disease. At the Laboratory he continued the study in an attempt to determine the single dietary factor that was the pellagra-preventive. Eight years after his untimely death in 1929, it was discovered in another laboratory to be nicotinic acid.

Dr. R. E. Dyer's most important work was on typhus and other Rickettsial diseases. Coincidently with his researches he was an administrator, first as Assistant Director of the Hygienic Laboratory, then as Chief of the Division of Infectious Diseases, and finally as Director of the National Institute of Health which became National Institutes of Health under his directorship.

Dr. R. R. Spencer came to spend part of his time while engaged in the investigation for which he became well-known, the development of a vaccine to protect against the highly fatal Rocky Mountain Spotted fever. While working on this project he spent a part of his time at the branch laboratory in Hamilton, Montana, where the entomologist Dr. R. R. Parker collaborated in the development of the vaccine. Dr. Charles Armstrong came to continue his brilliant career as researcher of a number of diseases including typhus, tetanus, and psittacosis. Later he isolated a new virus from a human brain and with others he described the disease, lymphocytic choriomeningitis, which it produces in man. He followed Dr. Dyer as Chief of the Division of Infectious Diseases.

The Hygienic Laboratory had two experiences with rampaging infectious diseases, the influenza pandemic of 1918, and at the turn of the year 1929-1930, an outbreak of psittacosis which was confined within the old brick building.

Williams (1) described the psittacosis outbreak, to which I will add only a few words. The swiftness of its spread through the Laboratory came as a shock to all of us. We knew the high fatality rate in some outbreaks, and the death of Harry Anderson, the first to become ill, warned us poignantly

of what the statistics might mean to us, as ten more of our colleagues became ill within a few days, and the Laboratory was closed for fumigation. We were very thankful for the recovery of the patients.

Perhaps an account of how the epidemic of influenza of 1918 affected one member of the staff of the Hygienic Laboratory will give to readers of a younger generation, whose knowledge of such epidemics is gained by reading, fortunately not by experience, an idea of the disruption that a serious epidemic may cause.

Coming to the United States from Europe and elsewhere, it struck Massachusetts in September and reached Washington in early October. Here war-time conditions favored a rapid spread of the disease, for the city was over-filled with dislocated people, the majority of whom were women who bad come to do the vastly increased war-time work of the government departments. They were crowded in boarding houses with three or more persons commonly occupying one room as a result of the shortage of housing. When someone became ill and a threat to associates, she was apt to find herself very much alone, with no one to take care of her. Some landladies dismissed boarders coming down with the disease.

On October 1 Congress passed a resolution “to enable the Public Health Service to combat ‘Spanish influenza’ and other communicable diseases by aiding state and local boards of health or otherwise… "

Some of the medical officers of the Hygienic Laboratory were sent into the field. Those who remained laid aside their research projects to organize emergency hospitals, or to become practicing physicians caring for sick government workers. Due to the absence of the doctors, and to the illness of many others of the Laboratory personnel, only a few were left to carry on.

I presume that there may have been a demand that the government agency should attempt to find the cause of the epidemic. However that may be, about the middle of the month Dr. McCoy asked me to drop my current problem and turn my attention to the subject of greatest concern.

At the time bacteriologists were considering whether the ''influenza bacillus" of Pfeiffer (*Haemophilus influenzae*), which was found quite constantly in cases of influenza, was or was not the etiologic agent of this disease. In later years it was considered to be a secondary invader.

My first thought was that I would examine the sputum of patients and tissues taken at autopsy to find the dominating bacterial species. That study would require a special culture medium. As our media-maker was ill, I went to the media-room to make it myself. Things were not going well, and I knew that I was not skillfully undertaking this job of making media to which I had long been unaccustomed. Gradually I realized that there was something the matter with me, more than the feeling of helplessness on being assigned unexpectedly to an enormous task. Finally, I guessed it—I was coming down with the flu. I put away utensils and ingredients and went home. A little more than a month later I returned to work.

It was during those weeks when I was confined to my room that Washington experienced a very serious epidemic. Dr. McCoy became a part-time physician, taking care of the Laboratory's personnel who were ill with uncomplicated influenza, in addition to his administrative work. Dr. Leake took care of government workers who were seriously ill with pneumonia in temporary hospitals.

Years later I heard a minister say that during the worst days of those weeks burial services in Washington cemeteries had to be curtailed in order that each funeral cortege could move along promptly to make room for the next.

When I returned to the Laboratory, I resumed my studies on meningococci, for the war was over, the peak of the epidemic in Washington had passed, and conditions were more favorable for clear-

headed thinking. It was recognized that the search for the etiologic agent of influenza was not a problem for a lone investigator. It became a major project of study in more than one institution (but not at the Hygienic Laboratory), and about fifteen years later a team of investigators in a London laboratory discovered that the causal agent is a virus.

Chapter VI

Milk as a Carrier of Disease

Since the pasteurization of milk is an underlying theme of the next chapter, a brief consideration of milk as a carrier of disease during the first decade of this century may help the reader to understand the dissensions in the dairy industry during the following two decades when progress of science seemed to interfere with business.

Between June and November 1906, three separate outbreaks of typhoid fever occurred in the District of Columbia. The following year Dr. M. J. Rosenau, Director of the Hygienic Laboratory, and his collaborators reported the results of their extensive study on the origin and prevalence of the out-breaks. They found that 85 of the 866 cases studied during this time were attributable to the use of infected milk; they described the milk and the conditions affecting it. They stated that the milk supply was “for the most part, too old, too dirty and too warm." It "passes through too many hands and is exposed too many times before it reaches the consumer.”

“Another source of danger is the small grocery store. In several instances we found a close association between the family life of the patient and the business. The same hands that nurse the sick often purvey the milk. The patient is treated in a room adjoining the store. Flies swarm in and out."

It is difficult, now, to realize that the poor quality of milk available to the purchaser in the District of Columbia in 1906 was typical of milk sold in cities and towns throughout the country, where diphtheria and scarlet fever, as well as typhoid fever were common diseases. Their spread was facilitated by unsanitary milk and by other conditions which now seem primitive. Another problem which began to be recognized in the early years of this century was that of milk infected with bacteria

causing bovine disease, especially disease of the udder. Suspicion was directed first against tuberculosis. The story of the web of conflicting ideas that had to be untangled, and the human emotions aroused in the process is relevant to these memoirs because it was enacted again with variations when the question arose of the harmfulness to man of brucellae derived from the udders of cows, and I was deeply involved in the second episode.

Tuberculosis was a common disease of cattle, with as many as 30 percent infected in some localities of this country. Two or three percent of infected cows had lesions of the udder and they gave milk containing enormous numbers of the "tubercle bacilli". But most health authorities did not worry very much about it, for Robert Koch said that the bovine type of the disease was not transmissible to man.

Koch was the discoverer of the causal organism of tuberculosis, and he made major contributions to the knowledge of other diseases, for which he was awarded many high honors including the Nobel prize. But even Koch could make mistakes.

Virchow had taught that human and bovine tubercu1osis were distinct diseases, but in the classical account of his research on the tubercle bacillus (1882) Koch was emphatic in his opinion that there was only one type of the organism which infected man, cattle, and other domestic animals. This opinion was accepted generally by pathologists, veterinarians, and health authorities, but it was contested by a few investigators.

Theobald Smith was the first to present adverse evidence. He reported (1896 and 1898) that comparative studies of cultures of the tubercle bacillus from the two sources, human beings and cattle, showed that they were distinguishable culturally, and by the lesions produced in experimental animals. Smith's observations were confirmed by others, and in 1890 Koch admitted the differences. Then he made another mistake of far-reaching consequences.

The recognition of distinct types of the tubercle bacillus raised the question whether man was susceptible to bovine tuberculosis, and evidence was slowly accumulating to show that bovine tuberculosis was transmissible to man. Ravenel (1900) reported several cases in which a veterinarian or butcher wounded himself while cutting the carcass of a tuberculous animal, and promptly developed generalized tuberculosis. Furthermore, a number of doctors reported small outbreaks in which several members of a family, or several infants in a children's hospital or girls at school developed intestinal tuberculosis almost simultaneously, and the cows providing milk were found to be in an advanced state of tuberculosis.

Interest in the question suddenly became acute with the announcement of his remarkable views made by Koch in London in 1901. He said that if the bovine tubercle bacillus were able to infect human beings, many cases would occur, especially among children. But, he said, most medical men believed that this was not the case. He added that if susceptibility of man to the bovine tubercle bacillus existed at all, infection was a very rare occurrence. He ended his remarks by stating his belief that it was inadvisable to take measures to protect human beings against bovine tuberculosis.

Again, Koch's views were widely accepted, but in England, in Germany, and in this country, investigators were incited to search out the facts. The extensive investigations made in both foreign countries agreed with those of Park and Krumwiede of the New York City Department of Health, who reported (1910) of more than l,000 cases of every kind of tuberculous disease (including tabulated cases with their own data). They found that about 25 percent of cases of tuberculosis children under sixteen, and beyond that age 1.3 percent of cases were of bovine origin.

Before public health authorities became worried about the transmission of bovine tuberculosis to human beings, the dairy industry was deeply concerned about the great economic losses caused by this disease in cattle. Testing with tuberculin which detected the infection even in its incipient stage,

began in 1892. In 1910 a systematic search for all diseased cattle was undertaken in the District of Columbia, and from this beginning grew the concerted effort to test all herds in the country. The campaign of testing, and of slaughtering reactors with compensation to the owner continued for many years. It was "sometimes carried out under actual fire by owners"(2) who cou1d not understand the necessity of slaughtering an animal that appeared healthy but reacted to the test. Finally, tuberculosis of cattle was essentially eradicated from the country.

Simultaneously, with the eradication of tuberculosis from cattle, the number of human cases of tuberculosis of bovine origin diminished until this type of the human disease became a rarity.

With advancing knowledge about the dangers of unsanitary milk, an idea was developing among public health authorities and other interested persons that something should be done to improve the quality of market milk. In December 1910, a meeting was held of the New York Milk Committee, a voluntary organization working to improve the milk supply of New York City. From a list of more than 200 names of men who were prominent in medicine, sanitation, public health, bacteriology, and other professions, the committee selected 20 men of various states, whom they invited to accept appointment on a commission to be known as The Commission on Milk Standards. Of the 20, two declined to serve and one resigned after the first meeting.

The 17 distinguished members of the commission represented every aspect of the dairy industry. Though it was created by and its expenses were born by the New York Milk Committee, it was not the intention that the Commission should have the New York City milk problem solely in mind, but that it should make recommendations that might be adopted by any city in the country.

Several years previously (1907) the U. S. Public Health Service had recommended the pasteurization of milk as the only dependable means of eliminating milk as a carrier of the common communicable diseases. In reports published in 1912 and 1913, The Commission on Milk Standards

recommended pasteurization of milk, but it excluded milk of the highest grade from that ruling. It recognized that bacterial content, using for the purpose the "bacterial count", was the most important factor in grading milk. It recommended that all milk should be labeled and marked with the grade—A, B, or C—in which it was to be sold.

Grade A milk might be either raw or pasteurized. Grade A raw milk should be drawn and bottled under strict conditions of cleanliness; it shou1d be from cows free from disease as determined by tuberculin testing and by physical examinations made by a qualified veterinarian; it should be handled by employees free from disease as determined by a qualified physician, under sanitary conditions such that the bacterial count should not exceed 10,000 per cubic centimeter. The qualifications for Grade A pasteurized milk were somewhat lower. Grades B and C were of inferior quality but were required to meet the standards for those grades.

Thus, the specifications for the production of safe milk failed to include a consideration of the possibility that milk handled with the strictest precautions might cause human disease if produced by diseased animals that appeared to be healthy. A suggestion had been made that septic sore throat might be due to streptococci derived from the udder of cows in cases of mastitis, but the information was too vague to receive the Commission's serious consideration. The warnings given in 1911 and 1912 of possible human brucella infection from the udder of infected cows had made little or no impression. The Commission thought that inspection of the cows by a veterinarian should guarantee against tuberculous infection; adherence to the rules for cleanliness would guarantee against infection with the germs of the common communicable diseases. In 1916 Dr. John F. Anderson, Director of the Hygienic Laboratory and member of the Commission, stated that it was his belief that all milk should be pasteurized excepting the better classes of Grade A.

If the requirements for the production of safe milk appear to be lax, it should be recalled that much pertinent knowledge has been gained during the last half-century. The first tightening of the rules was made in 1917, when the Commission adopted a resolution that all milk should be pasteurized for the protection of the health of troops against diseases commonly carried by milk.

Chapter VII

Brucellar Repercussions

The campaign against bovine tuberculosis had advanced successfully to its final stage, and assurance of the safety of certified milk was generally accepted when a new problem arose to perplex the dairy industry.

The first confirmation of my observation of the close relationship between the causal organisms of human undulant fever and bovine contagious abortion came from Dr. Karl F. Meyer of San Francisco and his collaborators (1920). Within the next four years confirmation came from ten more investigators in seven foreign countries.

Meyer and his collaborators suggested that another bacterial genus, *Brucella*, be recognized, to include the bacteria that designated *Micrococcus melitensis* and *Bacillus abortus*. It was a good suggestion and was readily adopted. About 1930 the name brucellosis came into use for the disease caused by infection with brucellae in man or lower animals. Probably this name arose independently in more than one part of the world. To my knowledge, its earliest use was by a Frenchman, Dr. Laurent, in his thesis presented in 1928 for the degree of veterinary medicine at the University of Lyon. This logical name was much needed. Its use emphasized the animal source of human infection. Furthermore, it replaced many names—undulant fever, Malta fever, Mediterranean fever, and numerous others—that had been applied to the human disease. Because the new terms facilitate clarity of expression, they are used in these memoirs regardless of whether the incident under discussion occurred before the words were coined.

My observations on brucellae were unacceptable to those dairymen who had invested in

expensive equipment for the production of certified milk. They took pride in producing clean milk, and scorned pasteurization, saying that it could make dirty milk salable. Some of them found it easy to believe that the bacteriologist who warned against the use of raw milk was collaborating with the manufacturers of pasteurizing equipment for the promotion of sales.

Opposition in the dairy industry reached a peak about 1930, then slowly declined as public health authorities and physicians gradually became more aware of the dangers of raw milk. An incident helped to turn the tide. A child of one of the officers of the organization of dairymen producing certified milk contracted brucellosis. Of course, the child had never drunk any other than certified milk. When the lesson was presented in this manner, it was understandable.

A somewhat similar occurrence hastened the acceptance of pasteurized milk in England. The story was related to me by one of the directors of the United Dairy Company, whose office was concerned with the education of the public in regard to dairy products. In trying to create a demand for pasteurized milk, he was making slow progress until the news was published that a child of a distinguished family had contracted brucellosis, and a serious bone infection made it a tragic case. The child had grown up on certified milk, which, to make the incident more dramatic, came from the herd owned by a prominent wealthy citizen. This case was given considerable publicity, and thereafter the demand for pasteurized milk increased notably.

Dairymen were fortified in their rejection of the idea that milk from cows that appeared to be healthy could cause human brucellosis, for their point of view was shared by certain bacteriologists who held high positions in scientific or educational institutions and in addition acted as consultants to the dairy industry. They thought that the evidence that brucellae of bovine origin could cause human disease was too meager to commend attention. They could not believe that human brucellosis might exist without being recognized. The fact that infected domestic animals appear to be healthy seems not

to have entered their thoughts. But investigators who read the literature on brucellosis in Mediterranean countries knew that this disease is so extremely difficult to diagnose that even in areas where it is known to exist, acute cases commonly receive an incorrect diagnosis before brucellosis is considered.

The most vocal scientist opposing the idea that brucellae in cow's milk might cause human disease was none other than Theobald Smith, who had been one of the first to warn of this possibility. His antagonism increased throughout seven years, ending in a collision.

For a number of years before my first paper on brucellae was published in 1918, Dr. Smith had been working on brucellosis of cattle, and he must have been studying this subject in 1918 and during the subsequent years when he opposed my point of view, for one of his publications on brucellosis of cattle was dated 1919, and another was dated 1925.

From several sources I heard that he was repeatedly expressing strong opposition. A colleague who was a member of the American Tuberculosis Society, of which Dr. Smith was president, told me that whenever he met Dr. Smith the subject was raised, and the discussion always included a remark to the effect that he was going to refute my work.

In the spring of 1925 matters seemed to be moving to a crisis. Dr. William H. Welch was obviously concerned, and that indicated to me that something unpleasant was about to happen. At the time of these events Dr. Welch was Dean of the School of Hygiene and Public Health of the Johns Hopkins University, having retired in 1916 at age sixty-six from the chair of pathology of the Medical School, a position he had held with great distinction since its beginning in 1884, nine years before it was opened for regular classes. According to Williams (1), Dr. Welch was "the most brilliant star in the sky of American medicine during this heroic age." (During Welch's lifetime, l850-1934)

Dr. Smith's record approached that of Dr. Welch in brilliancy. He began his career in 1884 as director of the laboratory of pathology of the Bureau of Animal Industry, U. S. D. A. Later was professor of comparative pathology at Harvard Medical School. In 1915 he became director of the Department of Animal Pathology, Rockefeller Institute for Medical Research. Most medical historians agree that his most outstanding achievement was accomplished in the B. A. I. when he found that Texas cattle fever was transmitted by infected ticks. It was one of the first demonstrations of insects as carriers of disease germs. Dr. Smith made notable contributions to the understanding of other diseases, particularly tuberculosis. In the words of Zinsser (3) Dr. Welch and Dr. Smith were “the two greatest individual influences that helped to hold the younger men working in the medical laboratories steadfast in the faith of the worthiness of honest effort."

As a member of the Advisory Board, Dr. Welch visited the Hygienic Laboratory in the spring of 1925. I suspect that he may have come, especially to see what kind of a scientist was being harbored there, who stirred Dr. Smith to such fervid opposition.

In his conversation with Dr. McCoy, I imagine Dr. Welch opened the subject with a question of doubt that had been raised in his mind by Dr. Smith concerning the reliability of my published work on brucellae. I feel sure that Dr. McCoy must have told Dr. Welch that he trusted my competence and integrity. Apparently, it was this conflict of opinions that disturbed Dr. Welch. After their conversation Dr. McCoy brought Dr. Welch to my laboratory to meet me. We did not discuss brucellae until they were about to leave, when Dr. Welch said he wished Dr. Smith and I would compose our differences in regard to brucellae. Not knowing what to say, I made no reply.

A few weeks later I met Dr. Welch again at a meeting of the American Society of Tropical Medicine held in Washington, D. C. where I read a paper on the geographical distribution of the serologic groups of brucellae as determined by the agglutinin-absorption test.

The reading of my paper was followed by a discussion in which Dr. Welch joined with kind comments on the paper, but he concluded with the remark that he could not believe that infected cow's milk might be the source of human brucellosis. He then came and set beside me and repeated what he had said before, that he wished that Dr. Smith and I would get together to compose our differences in regard to brucellae. Again, not knowing what to say, I made no reply.

A few weeks later the expected unpleasantness happened, and at this point another distinguished medical scientist, Dr. Ludwig Hektoen, entered these memoirs (for the second time). In 1925 he was Director of Medical Sciences of the National Research Council. The duties of that office did not fill his time, and Dr. McCoy offered his space at the Hygienic Laboratory to work during his spare hours. Re used a vacant desk and workbench in my laboratory which occupied the entire eastern end of the second floor of the old brick building. Almost every day he came for a few hours of work.

One day, without looking up from his microscope he remarked rather casually, "I see that you and Dr. Smith are coming to a clash." I asked for further information and learned that Dr. Smith had been invited to become chairman of the Committee of Infectious Abortion of the National Research Council, and it has been proposed that I should be invited to be a member of the committee. Dr. Smith sent a telegram to Dr. Maynard M. Metcalf of the National Research Council declining the invitation to be chairman, followed by a letter of explanation. Dr. Hektoen told me about the contents of the letter, and on the following day he gave me a copy of it.

Dr. Smith wrote: "I am now studying thoroughly some cultures of so-called Malta fever in man, at least one of which has been ascribed by Miss Evans to the cow. My own results so far indicate that these organisms and the organisms from the cow are identical. You will see that I should be at once with a difference of opinion with one of the members of the committee. This work, however, is not yet completed and will not be published until fall or even later."

This work was never published, for Dr. Smith was going to realize very soon that he was making a mistake in opposing the idea of cow's milk as a source of human brucellosis.

Limited knowledge of brucellae at that time was responsible for Dr. Smith's confusion. In 1914 Traum reported that *Br. abortus* is the causal organism of abortion in sows. This was its specific name until 1928 when Huddleson devised a cultural technique that differentiates bovine and porcine strains. The latter were then named *Br. suis*. The abortus-suis group is readily distinguished from *Br. melitensis* by the agglutinin-absorption test, which does not differentiate bovine and porcine strains.

In October 1922, the first case of human brucellosis to be recognized in this country in which the source of infection could not be traced to goats was diagnosed in the Johns Hopkins Hospital, Baltimore, Maryland. A culture of the infecting organism was sent to me by Dr. Harold L. Amoss, with the request that I determine whether it was of the bovine or of the caprine type. According to the agglutinin-absorption test the strain was classified as *Br. abortus*. The case was reported by Dr. C. S. Keefer in 1924.

In comparing the Baltimore strain with bovine strains, Dr. Smith noted certain pathologic differences in experimentally infected animals. This finding led to further investigation of the source of the patient's infection, which revealed that as a laboratory assistant at the Johns Hopkins School of Medicine, he had been in frequent contact with porcine tissues from a slaughter-house. The strain vas classified as *Br. suis* several years later, with the aid of the new criteria.

The first case of human brucellosis in this country in which a culture was obtained from the patient and the infection was traced to infected cow's milk occurred in Ithaca, N. Y. Dr. C. K. Carpenter of Cornell University, who reported the case, obtained the culture in February, 1925. He sent it to me for the determination of type and I found that according to agglutinin-absorption tests it was of the *abortus-suis* type. If Dr. Smith had had the opportunity to study this culture thoroughly, perhaps the final incident of our controversy might have been averted.

When I learned that Dr. Smith had raised doubt in the National Research Council about the validity of my work, I thought I ought to do something about it. I discussed the matter with Dr. McCoy, and he said he did not know of anything I could do with dignity. He reminded me consolingly that "Truth will prevail." I replied that a man of Dr. Smith's stature could delay recognition of the truth for years.

After thinking hard for a few days, I decided I could ask Dr. Welch to intercede, and I drafted a letter to him. Again, I consulted with Dr. McCoy, for every professional letter written by a subordinate had to be approved and forwarded by the Director. He said that the letter was all right, and that I could send it. But he warned me that I should not expect a reply for Dr. Welch had the reputation of never writing letters. Even during the years when he was on the Advisory Board, Dr. McCoy had never seen a letter written by him. Dr. Welch's technique for the avoidance of writing letters was told amusingly by one of his biographers.

In my letter to Dr. Welch I reminded him that he had said he hoped that Dr. Smith and I would adjust our differences of opinion, and I continued in part: "It has been reported to me from another source that Dr. Smith is strongly opposed to the idea that cattle may be a source of undulant fever in man. I am not personally acquainted with Dr. Smith and cannot very well address him on the subject, but since you appear to be in touch with his work, I am writing this letter in order that if you see fit you may communicate with him when opportunity comes.

"It seems to me that Dr. Smith could not take the point of view that the so-called *Bacillus abortus* is non-pathogenic for man if he knew the evidence that has accumulated in the last few months in Italy and South Africa, as well as in this country."

Two days after I mailed the letter a reply came from Dr. Welch in his own handwriting: "I am very much interested in your letter. I am taking the liberty of sending it to Theobald Smith. I think so

highly of the work of both of you that if I can be the means of bringing about a rapport between you and him in this important study, I shall be gratified.

"If not too much trouble can you give me the reference for the South African cases?"

I answered Dr. Welch's letter by return mail, enclosing a list of six references to articles in recent British or South African medical journals reporting brucellosis in man contracted from infected cows in Southern Rhodesia. Altogether 35 cases were reported.

Dr. Welch sent the list of references to Dr. Smith, and about a week later I received a letter from Dr. Smith, a part of which I quote: “Dr. Welch has kindly sent me your letter bearing upon the relationship between *B. abortus* and *B. melitensis*. Thus far I have not published anything and I did not know that my private talks had any publicity….

"On the whole, I think that the accuracy of your work does not come in question as far as it has gone. Nor do I think that it would suffer if you suspended judgment until the unknown factors responsible for or contributing to the incidence of the human cases have been brought to light.”

The next move took more time. Six months after receiving Dr. Smith's letter I received a letter from the secretary of the Division of Biology and Agriculture, National Research Council, inviting me to serve as a member of the Committee on Infectious Abortion, which had been reorganized recently under the leadership of Theobald Smith. I accepted, and I remained a member of the committee for six years without memorable incident.

The thin file of letters pertaining to the episode preceding my appointment was given to the Library of the Department of Bacteriology, University of Wisconsin. It contains the copy that was given to me of Dr. Smith’s letter to the National Research Council; a carbon copy of my letter to Dr. Welch; Dr. Welch’s letter to me; a carbon copy of my reply to Dr. Welch, with the list of references of reports of

human cases of brucellosis in Southern Rhodesia traced to infected cows; Dr. Smith's letter to me; the letter from the National Research Council inviting me to serve on the Committee of Infectious Abortion a carbon copy of my letter of acceptance.

Although Dr. Smith' s opposition was allayed in 1925, other dairy bacteriologists clung for another decade or longer to the argument that bovine brucellae could not be the cause of human disease because cases of brucellosis were not reported. If they had followed the medical literature on the subject, they would have learned that cases of human disease caused by infection with bovine brucellae were being found in scattered areas of this country, and also in other countries, occurring sometimes as small outbreaks, for when the doctors of a given area were alerted by the diagnosis of one case, other cases would be recognized. But, generally, brucellosis was unrecognized, and to this day many cases are missed, especially cases of the chronic disease, because of the difficulty of correct diagnosis, and because too often brucellosis is not considered.

Only a rough estimate can be made of the incidence of this disease. In 1947 I presented evidence indicating that the actual number of cases occurring in this country was at least 10 times the number of reported cases. This estimate was accepted as reasonable by the *Journal of the American Medical Association* in its *Current Comments*.

It was almost six years after the first case of human brucellosis not traceable to goats was recognized in this country when *Public Health Reports* began reporting regularly the prevalence of "undulant fever”. Two cases were reported in July 1928, and in each succeeding month the number increased, mounting to a peak of 6,321 reported cases in 1947. Since then there has been a steady decline to 636 cases reported in 1961.

Obviously, the rapid increase in the number of reported cases was due largely, if not entirely, to the growing awareness of the disease. The decline may be attributed chiefly to the decreasing number

of infected cattle, with the additional factor of increasing usage of pasteurized milk and other dairy products.

To reduce the enormous losses due to brucellosis in the dairying and cattle-raising industries—the estimated loss was $100,000,000 in 1947—a federal-state program of eradication of the disease from cattle was begun in 1934. The animals were tested for evidence of infection, and reactors were slaughtered with compensation to the owner. About 1940 another supplementary method of combat was introduced—the vaccination of calves to increase the animal's resistance to the disease.

The first series of tests indicated that 11.5 percent of cattle in this country were infected. By 1941 the percentage had been reduced to 2.4. When during World War II the program slackened unavoidably, the percentage of infected animals doubled. In 1947 a nationwide conference on brucellosis was held which sparked a greatly accelerated effort to eradicate the disease. The campaign against bovine brucellosis continues successfully, with a large part of the United States now "certified brucellosis-free" or "modified certified brucellosis-free", which signifies that infection appears in no more than one percent of the animals and five percent of the herds.

The goal of complete eradication is approaching, but the last stages of the campaign will be long and difficult because cattle are susceptible to infection with the porcine and caprine types of brucellae, and because other species of domestic animals and also wild animals are susceptible to infection with the various types of brucellae. As yet, little is known about transmission of the disease from wild to domestic animals.

With the gradual decline of milk-borne brucellosis the human disease is becoming more and more occupational, occurring chiefly in farmers, veterinarians, and workers in slaughterhouses, meat-packing plants, and in other occupations necessitating contact with infected animals, their carcasses or tissues. As the percentage of human cases of infection with *Br. abortus* declines the percentage of cases

infected with *Br. suis* increases, and the disease in swine is of growing concern. In 1961 the U. S. Department of Agriculture began what will become a nationwide program of eradication of porcine brucellosis, in cooperation with the states.

Chapter VIII

Studies on Brucellosis at the Hygienic Laboratory

During the summer of 1922 an outbreak of disease occurred in Phoenix, Arizona, and after several weeks brucellosis was suspected. By the end of the summer 35 cases had been recognized. Some of the patients were convalescents recovering from tuberculosis, two of whom died. All of the patients had drunk freely of raw goat's milk.

In response to a request, Dr. Gleason C. Lake of the Hygienic Laboratory was detailed for duty in Arizona to cooperate with the state and local health officers. He mailed to me for identification cultures from human cases, and from goat's milk. They were found to be *Brucella melitensis*, the variety typical of caprine infection.

While I was studying the cultures from Phoenix, I received the Baltimore culture which has already been discussed. The finding of the Baltimore case of brucellosis, the first to be reported in this country not of caprine origin, and the recovery of the causal organism from this patient gave me valid reason for resuming the study of brucellae. It was the event for which I had been waiting.

One day the subject of human brucellar infection was under discussion with Dr. McCoy and a few other medical officers of the Laboratory. The suggestion was made by Dr. Walter T. Harrison that there was one more experiment that should be carried out which might strengthen the evidence of' close relationship between the causal organisms of bovine contagious abortion and human undulant fever. He thought a pregnant cow should be inoculated with *Br. melitensis*. I agreed that if arrangement could be made, such an experiment should be carried out. Dr. McCoy said he thought he could make an arrangement with the Bureau of Animal Industry, U. S. D. A. He did so and shortly after, he accompanied

me, carrying a culture of *Br. melitensis*, to the farm where experiments with brucellosis in cattle were being conducted. It was located in Chevy Chase not far from the southwest corner of Wisconsin Avenue and Bradley Boulevard. The farmhouse which was used as a laboratory later became a recreation center for the Bethesda-Chevy Chase area, and it is still used for that purpose.

The pathologists of the B. A. I. assisted in carrying out my experiment willingly and cheerfully while frankly expressing their skepticism, as did their younger colleagues six years earlier. One of them said he didn't think anything would happen. I ventured to reply that I expected something would happen.

Forty-six days after a pregnant heifer was inoculated, she aborted. *Br. melitensis* was cultivated from the stomach contents and from various other fluids of the fetus, and from the heifer's colostrum.

My next undertaking was a survey in search of the cases of human brucellosis, made by examining the remnants of 500 samples of human blood serums received at the Hygienic Laboratory by the Wasserman test. One hundred four of the samples came from the Naval Hospital on the grounds of which the Hygienic Laboratory was located. The remainder came from many places in northeastern United States, most of them from veteran's hospitals.

Of the 500 serums 16 (3.2 percent) agglutinated the two test antigens, one prepared with a strain of *Br. melitensis*, and the other with a strain of *Br. abortus*. Fifteen of the serums gave slight reactions in dilutions of 1:40 or lower, too low a titer to be accepted as evidence of present infection. One serum received from the Naval Hospital gave a positive reaction in a dilution of 1:320. This would have been accepted unquestionably as evidence of brucellosis in areas where the disease is endemic. A second sample of the patient's blood was obtained, and by absorbing the agglutinins from the serum with brucellar antigens it was determined that the infecting organism was of the *abortus-suis* type.

The medical officers who had examined the ambulatory patient accepted the diagnosis of brucellosis because they had found no evidence that led them to suspect any other disease. The patient who lived in suburban Virginia, had been in the habit of drinking raw cow's milk. This was the second case of human brucellosis not traceable to goats to be recognized in this country.

From the foreign literature of brucellosis, I had learned that handling brucellae was dangerous work. I was as careful as I knew how to be, but I did not know that the precautions that would prevent a laboratory worker from contracting meningitis or typhoid fever would not guarantee against infection with brucellae. Forty years later, at the time of this writing, the danger of contracting brucellosis in laboratories is well known from wide experience in many laboratories. Brucellae have been found to outnumber by far any other agents the cause of laboratory-acquired infections. A probable explanation has been found to be their unusually high resistance to atomization, together with a sensitivity of the respiratory tract as a route of infection.

With intensive work in progress on several strains of *Br. melitensis* from Phoenix and the strain of *Br. suis* from. Baltimore, it was almost inevitable that I should become infected. It happened in October 1922, with *Br. melitensis* as the agent, proved by cultures obtained in 1923, 1928, and 1931. Many years of ill health followed the infection with periods of complete incapacitation alternating with periods of partial or complete recovery. The last disabling exacerbation occurred in the summer of 1943, almost twenty-one years after the date of infection.

As in most cases of chronic brucellosis, there were no physical signs of disease during long periods of ill health, and I, like very many other brucellosis patients, received the diagnosis

of “neurasthenia” (the term commonly used in those days) with its implication of malingering. In my case, exoneration came in 1928, after years of ill health, through the intervention of another disease which required surgery. Cultivation of *Br. melitensis* from diseased tissue explained the prolonged ill health.

My experience with chronic brucellosis gave me opportunity to observe at first hand this form of the disease, with its implications. In the 1920’s it was practically unknown in American medicine, although Craig had described three cases in returned soldiers who had served in the Philippine Islands during Spanish-American War of 1898.

I had read the British and French literature on brucellosis in the Mediterranean area and learned that this disease was prolonged in mild form in many cases and that the chronic patients were apt to be regarded as neurasthenic. I never doubted that brucellar infection was the cause of my trouble. The disease is better known now with slowly growing recognition of the fact that the tests which are generally reliable for the diagnosis of acute brucellosis commonly fail to detect evidence of infection in cases of the chronic disease.

In recent years, however, with the advent of disability insurance and the acceptance of brucellosis as a compensable occupational hazard, malingering is sometimes suggested in the literature on brucellosis as a reason for the failure of recovery, or it is suggested that a diagnosis of psychoneurosis prior to infection be made. The injustice of such damaging judgements based on faulty evidence was obvious when in one sweeping indictment it fell upon sixteen chronic brucellosis patients who failed to recover after contracting the disease in the laboratory at Fort Detrick. I was incited to take up my brucellosis pen again after sixteen years of retirement, in an attempt to stimulate a more zealous search for knowledge of this obscure disease.

To be ill and regarded as an imposter is to be in an almost intolerable situation, and a damaged reputation is not readily repaired. The rule of law that the suspected should be considered innocent unless guilt is proved ought to be applied also in medicine.

A hypersensitivity to brucellar antigen ended my work with cultures of brucellae, and I turned to other subjects of study. In 1936, however, I returned to a final investigation of a brucellar problem in which I handled no living cultures. I planned and coordinated the work of a team of three young doctors who carried out field investigations to determine the prevalence of chronic brucellosis. We included in this study an evaluation of the various laboratory procedures in use at the time for the diagnosis of brucellosis.

The areas selected for the surveys were three widely separated cities of the United States where a large percentage of the milk was consumed raw, although the herds supplying the milk were known to be infected with brucellae. Through the courtesy of the local physicians a search for evidence of brucellar infection was made among chronic patients in whom satisfactory evidence of disease was lacking.

The survey which gave the most information was in a city of about 107,000 inhabitants, in an area where a simultaneous survey of bovine brucellosis was being carried on as a part of the national program of eradication of the disease from cattle. This city was selected for a study of the prevalence of the human disease because 23 percent of cows were found to be infected and 81 percent of milk was sold raw.

Among 325 cases studied, 22 were found in which brucellosis appeared to be the logical diagnosis. In 5 cases brucellar infection was proved by cultivation of the organisms; in 9 cases clinical considerations were supported by positive agglutinative reactions with brucellar antigens; in 8 cases a diagnosis of probable of probable brucellosis was made despite the fact

that specific tests failed to confirm the clinical evidence of brucellar infection. In general, the contacts of the 22 bucellar patients were limited to the ingestion of milk and other dairy products.

Chapter IX

Southern Rhodesia Leads in the Recognition of Brucellosis of Human of Bovine Origin

The first person to make practical application of the new knowledge of the relationship between *Br. melitensis* and *Br. abortus* was L. E. W. Bevan, a veterinarian of Southern Rhodesia. From the time of his arrival in 1905 he had encountered outbreaks of contagious abortion in cattle. Also, a mysterious infection of man which defied diagnosis became a matter of great concern in that area. A Dr. Appleyard, an old but keen clinician, suspected that the disease might be a form of Malta fever, which he had seen elsewhere.

In 1921 when Bevan read about the relationship between *Bacillus abortus* and *Micrococcus melitensis*, he began to "think furiously". He studied hospital records and was struck by the fact that many cases of the obscure disease came from farms where there was no contact with goats. Finally, a patient came from an area that Bevan knew the cattle were infected. He prevailed upon the doctor to obtain a sample of the patient's blood and he found that the serum gave a positive agglutinative reaction with the standard suspension of *Br. abortus* in dilutions as high as 1:200. The case of reported in the January 1922, issue of *Transactions Royal Society of Tropical Medicine and Hygiene*. Soon afterwards, Bevan studied other cases of unidentified disease which he diagnosed as brucellosis by means of agglutinative test.

Working in the same area with Bevan was the bacteriologist Orpen. In the July 28, 1923 issue of *South African Medical Record*, he described his method of obtaining blood cultures from patients with brucellosis, and in the February 23, 1924, issue of the same journal he reported that by means of the agglutinin-absorption test he identified the Southern Rhodesian strains as *Br. abortus*. Since Orpen did

not give the data when he first obtained brucellae from a patient, it is questionable whether his proof of human infection with brucellae of bovine origin antedated the cultivation of brucellae of porcine origin from the Baltimore case in October 1922, which was reported by Keefer in January 24, 1924 number of the *Bulletin of the Johns Hopkins Hospital*. The records show, however, that the first recognized case of human infection with brucellae of non-caprice origin was found in Southern Rhodesia the year before the Baltimore case occurred.

In 1925 Bevan wrote that 35 cases of brucellosis not traceable to goats had been recognized in the United States, three of which were of porcine and two of bovine origin.

Chapter X

Studies on Streptococci

For many years I carried on studies of streptococci, chiefly the beta-hemolytic streptococci belonging to group A, in various aspects of their relation to disease.

Before the discovery of the two therapeutic agents, sulfa compounds and penicillin, which came into general use about 1940, the chief incentive for investigations of pathogenic streptococci was the acquirement of knowledge which might lead to the development of therapeutic antiserums. When the cures were found, it seemed that the dream had come true, and that further streptococcal investigations could be postponed indefinitely.

I had begun my streptococcal studies a number of years before 1940 and I continued them until retirement in 1945, for I had done much work, and much more needed to be done before conclusions could be drawn in the attempt to determine whether an immunologic relationship exists between strains of heterologous agglutinative type.

In 1919 Dochez, Avery, and Lancefield reported their study of a limited number of strains of four biologic types and a few untyped strains. They found that agglutinative reactions were correlated with the results of protection tests; they observed no cross-protection between agglutinative types. A theory of the unity type in streptococcal immunity was formulated, which did not include the possibility that the bacterial cells might have components other than the type specific antigen which could give rise to protective reactions. Their theory, based on a study of a few strains, remained unchallenged for more than two decades.

When I began my studies the number of recognized types has multiplied to about thirty, and it continued to increase. The time had come, I thought, to re-examine the immunologic relationship between these many agglutinative types.

In the first phase of my study I selected strains of streptococci that were distinguished from the majority by lack of ability to ferment lactose. They were chosen for the practical reason that inability to ferment lactose was correlated with high virulence for mice and marked the ability to produce potent antiserum in rabbits. The agglutinative reactions of these strains placed them in types 14, 15, 18, and 19. I prepared protective serums with one strain of each type and tested the potency of the antiserums in mice inoculated with diluted culture. I found that three of the four antiserums gave cross-protection, one against 1 of the three heterologous strains, one against 2, and one against all 3.

The study was continued using antiserums prepared with the four strains used in the preliminary study together with additional antiserums, two of which were prepared with a mannite-fermenting strain, one of type 6, the other of type 23. Antiserums of other types were limited to five because in most of the types no strain suitable for production of antiserum could be found.

I found that the ability of the streptococci to ferment mannite, possessed by a relatively few strains, was correlated with serologic grouping, and that those, as well as the lactose-deficient strains, possess high virulence for mice and produce in rabbits antiserums of high potency.

The 11 antiserums were tested in mice for protection against 56 strains of streptococci of 20 agglutinative types. One of the serums failed to show protection against any strain of heterologous type; ten gave protection against one or more strains of heterologous type. The mannite-fermenting strains of types 6 and 23 were found to produce antiserums of wide range potency, that of type 6 producing antiserum which protected against 29 strains of 15 heterologous types, and that of type 23 producing an antiserum which protected against 20 strains of 13 heterologous types.

The strains of type 23 which produced an antiserum with wide range of potency had been studied by Aronson four decades earlier. He found that the antiserum produced by this strain protected against many strains of streptococci from various disease sources. His observations were confirmed within a few years, and the interesting strain was kept alive in several laboratories. But Aronson's prophetic observations on the properties of this strain were forgotten, while the theory of the unity of type-specificity was generally accepted.

In 1956 D. G. Fleck of London reported a study of one strain to streptococcus of each of four types, 1, 6, 18, and 23, in which I had found strains which produced antiserums capable of giving protection against strains of heterologous types. In his first series of tests he found no cross-protection, but the antiserums of types 6 and 1 seemed to delay the death of the mice infected with strain 18. He prepared another antiserum with the strain of type 6, and this gave "significant protection against infection with type 18 streptococci." The cross-protection obtained with the one antiserum type 6 was confirmed by its enhancement in the blood of the mouse of phagocytosis of the streptococci of type 18, but no other type. Also, in a precipitin-absorption test a relationship between type 6 and 18 was shown.

During World War II the first report to be made by the epidemiologist on group-specific immunity against streptococcal infection was published by Rantz and his collaborators. They reported (1945) that during a food-borne epidemic in a military hospital caused by streptococci of type 1, more than 250 cases of clinical infections were discovered. A number of men previously infected by other types of group A streptococci were hospitalized at the time of the epidemic and were exposed to the infection. None of them reacted to the presence of the new type more than a minimum of clinical signs.

For years the late Dr. Schwentker was engaged in studies on the epidemiology of streptococcal infections in Europe, in the U.S. Army, and in the U.S. Navy during World War II. In 1943 he wrote: "All our observations point towards a conclusion that resistance to streptococcal infection is due, at least in large part, to some kind of type-specific immunity". In a later report (4) Dr. Schwentker stated that he

undertook his naval research with the belief that immunity to streptococcal infections was type specific. But before making this report he changed his mind, after carrying out an experiment in which a group of men were immunized with vaccines prepared with a single type of streptococcus. This group showed 33 percent reduction in total number of streptococcal infections, as compared with a control group. He wrote: “To our surprise, the protection was not type specific”. (I failed to find a detailed report indicating the type of strain he used to prepare the successful vaccine).

Holmes and Williams (1958) observed streptococcal infections for three and a half years in a house near London with an average population of more than 400 children. Among the children with tonsils, but not significantly in those without tonsils, the percentage of cases of illness was substantially lower in the groups with a history of heterologous type illness or exposure than in the unexposed group.

Apparently Dubas had in mind the streptococci when he wrote in 1945: "The obvious importance of the type-specific antigens, and the ease with which they can be studied, has led to the comparative neglect of the other components of the bacterial cells which can also give rise to protective reactions. There is, however, much evidence for a type of acquired immunity which transcends the limits of type differentiation and which is effective for a whole bacterial group.”

Dubos (5) raised a question concerning the validity of the experimental approach which separates “phenomena and structures into their elementary particles… to study these by highly refined analytical methods… Everything in nature partakes of organized complexity, and the scientist must, therefore, learn to study experimental situations involving a number of mutually integrated systems”. This principle derived from reflections on biologic science in general, emphasizes what the limited data on streptococcal group-specific immunity points out, that the immunogenic properties of the entire bacterial cells should be considered. To future investigators of the epidemiology of streptococcal infections, this offers an interesting field.

Chapter XI

Streptococcal Bacteriophage

One of the first features of streptococci to attract the attention was their susceptibility to bacteriophage, and I became interested in the possibility that host susceptibility to the various types of streptococcal phage might serve as a useful character for identification of strains. A good start had been on phage-typing of the *Enterobacteriaceae*, but this method of classification had not been utilized appreciably in studies of other bacterial families, although the phage phenomenon had been discovered about two decades earlier.

My first concern, however, was with an idea of that time that since phage destroys bacteria, it should be a good remedy for bacterial infections. The results of my experiments in mice and rabbits showed that the phage inoculated together with sensitive streptococci did not protect the animal. Further, I found that in test tube experiments the action of streptococcal phage was inhibited by animal fluids. This might offer an explanation of its impotency as a therapeutic agent. There was some evidence that phage might activate a sublethal dose of streptococci in mice.

Two type of streptococcal phage had been described before my work on this subject began, neither of which was capable of lysing strains of group A. I searched for phage with an affinity for strains of group A and found two serologically distinct races of differing affinities, and my assistant, Elsie Sockrider, found another race of phage with an affinity for certain strains of group A not lysed by the races previously found. It was noted that sensitivity to various types of phage was correlated with physiologic and serologic characters of streptococci. This strengthen the belief that phage-sensitivity patterns might be of useful character to aid in the determination of relationship between strains, and in the differentiation of closely related strains. Lysogeny appears to have potential significance in epidemiological studies.

The most interesting observation that resulted from my studies on streptococcal phage was that at the moment when the phage was destroying the bacterial cells of a sensitive strain, collateral lysis of certain cocci that were resistant to the filtered lysate would occur. I called this the “nascent” state of phage. A similar enhancement of the potency of streptococcal phage in the presence of a growing strain had been noted previously by Gratia.

With the lapse of years before recognition, it seemed as if my observation of nascent phage would sink quietly into oblivious. Then, twenty-three years after my first publication on the subject, Maxted of London and Krause of Rockefeller Institute published reports on studies of nascent phage at about the same time.

Maxted, Krause, and investigators in the other laboratories have extended several lines of study on this phenomenon. Results indicate that it is not the phage that causes collateral lysis of resistant strains, but a labile lytic factor with enzymatic properties which is produced by the infected strain at the time of lysis. The enzymatic factor may be used to release various antigens from the cell walls of group A streptococci. Following another line of study, it has been found to produce protoplasts and L forms of group A streptococci. This opens a new approach to investigations of changing morphology.

Epilogue

The passage of eighteen years since retirement gives a point of vantage from which to review with considerable objectivity the events that shaped my career. In reliving them and reviving old memories, some almost forgotten, my impelling interest was in searching the background which determined the course of events, and in examining the present status of problems on which I worked, to appraise the results of my efforts in the light of subsequent trends. These probings became a part of the memoirs and crowded out comments that might have been made on projects that demanded less of my time and thought.

Omitted also are memorable incidents which absorbed temporary attention. If this pen was more facile, I might dwell on some of the richly endowed personalities, and the beauties of nature and art that entered my life and passed out leaving their imprints.

Certainly, there are regrets over difficult situations that I might have handled better. But the course that was open for my ship to sail was on the whole gratifying. The going was rough at times, and there were stretches of clear sailing too.

Washington, D.C.

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