HERBERT W. CONN

Photograph taken about 1915

PROFESSOR HERBERT WILLIAM CONN AND THE FOUNDING OF THE SOCIETY[1]

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Geneva, New York

In December, 1898—that is, just 50 years ago this coming winter—three members of the American Society of Naturalists, at a New York meeting of that Society, met together informally to discuss the question whether a society specially dedicated to bacteriology as a science was called for in this country. As a matter of fact, bacteriology as a science was a new idea in those days; it was chiefly thought of as an adjunct to pathology. The public had heard about "germs", but if you told a layman that you were a bacteriologist, you would get a blank look in return, whereas if you had said chemist, botanist, or zoologist, there would have been some comprehension of your occupation. Of the three men in question, one was a general biologist, the other two pathologists. The chief question they discussed was whether an organization was needed which would appeal both to medical and non-medical men who were interested in bacteriology *as such*—not merely as a phase of pathology. These three men realized they were interested themselves in these broader aspects of bacteriology and felt that there might be others similarly minded in this country, enough eventually to form a society of perhaps 100 members.

The three men were A. C. Abbott of the University of Pennsylvania, E. O. Jordan of the University of Chicago, and my father, Prof. H. W. Conn of Wesleyan University. Of the three men, I naturally know the last-named best; and it is of him I wish to speak today.

I judge from comments of others, even more than from my own recollections, that he was in many respects an unusual man. He was an energetic organizer, yet suffering much of his life from what we now call an inferiority complex. He was an inspiring teacher, yet retiring in his ways and not at all gifted socially; in other words, he was unwilling to talk much unless he thoroughly knew his subject, and then he could be eloquent. He was an outstanding advocate of personal and public hygiene, although in his own ways he often failed to practice what he preached. He had force of character sufficient to put across those policies in which he was interested; yet he so hated argument that he would often get up and leave the room if conversation became argumentative. Although he was inspiring to his students, he was apparently so afraid of influencing his son's choice of a profession that he almost never mentioned bacteriology to me when I was a boy, and it was not till many years later that I realized he had wanted nothing more than for me to follow in his footsteps. He had a complex character, and I sometimes think that, seeing him in his daily life, I knew less of his character than his students who saw him only at class and in the laboratory.

[1] Amplification of Presidential Address delivered before the Society of American Bacteriologists at its Forty-eighth General Meeting, Minneapolis, Minn., May 12, 1948.

Years ago I was urged by one of his students to write his biography, and I replied that I did not know him well enough. Indeed, I have undertaken this subject for my address with some trepidation; the only reasons I now feel capable of handling it are that certain diaries and reminiscences of his, which had long been lost, came to light a few years ago, and in addition to reading them I have been having considerable correspondence of recent years with former students and associates of his.

Probably most of us are familiar with the famous story of Pasteur, telling how at a dinner he called attention to the importance of washing the germs off the skins of grapes before eating them, and then later absent-mindedly drank the water in which he had washed them. I do not think I ever knew my father to do that; but he would lecture repeatedly about the dangers of the common drinking cup—and then promptly use one himself the next time he wanted a drink while in a public place. He used to hate screens and screen-doors worse than flies, and my mother was forever having to shut them after him; so, when the connection of flies to typhoid was brought out, I think he was upset mostly because it gave my mother an unanswerable argument as to why he should not leave a screen door open. As a matter of fact he was somewhat fatalistic where matters of his own health were concerned, and many a recollection we members of the family still retain indicates that he regarded the rules of hygiene he taught as applying to others but not to himself.

But it was an entirely different attitude that made him a good teacher. It was partly because he had very human ways that endeared him to his students. Back in the days when college professors were supposed to be distant, dignified and immaculately dressed individuals, he was anything but that. Had he lived in this generation he would have delighted in the informal attire that is permissible so much of the time; but he did his best, for those days. His unconcern about the appearance of his clothes was a matter of much distress to my mother, but I do not think his students thought any the less of "Herbie" (as they used to call him, sometimes almost to his face) on that account. She was particularly mortified one morning when, after he had been gone an hour, one of his students came to our house; "Prof. Conn came to work this morning" he informed her "without his collar and necktie, and he sent me here to get them for him." I can imagine her feelings. With the stiff shirts of those days, the lack of a necktie alone would have been conspicuous; presumably too he had been to chapel, as the college day at Wesleyan always began with chapel services!

So the boys must have thought of him as a much more human person than the average professor. Also he could be inspiring. He might come into the laboratory some afternoon where three or four students were working, perch on one of the desks with one foot up under the other knee, and begin giving a very informal lecture. The boys would usually stop their work because they knew they were going to hear something interesting; and besides they suspected that in "Herbie's" courses less stress was laid on the amount of work done than on the student's interest in the work. Perhaps he might have just returned from some meeting, and, if so, he was always full of anecdotes about the men he had seen and points of

technic he had picked up from them. Almost invariably he would pick out of his pocket a small pad of paper with loose sheets torn off and attached to it by a rubber band. You see, he had the habit, whenever he heard anything that might specially interest someone, of writing that fact on a slip of paper from this pad under the name of the person to whom he intended to tell it. He kept the slip till he had done so, and then would throw it away. This pad with its attached slips became well known to his students and associates.

Another strong feature of his as a teacher was a way he had of driving a point home. In a lecture, he would pass rapidly over the details which he knew would be promptly forgotten even if his students took the trouble to learn them; but would stress and reiterate the fundamental points, even though they were so elementary many of his hearers knew them in advance. Most often he would repeat himself in different language to avoid monotony; but sometimes he would even repeat the words. Professor Buddington of Oberlin relates something about one of the lectures which he says amused him greatly. He was an associate of my father's for several years and used to sit at the back of the room through all the lectures so as to know where to take up a subject if he were suddenly called on to substitute for the regular lecturer. In one lecture on foods, the different values of proteins and carbohydrates had been discussed and he drove home his conclusions by coming out emphatically with the practical advice: "So gentlemen, remember, always eat *meat* with your *potatoes*! *Meat* with your *potatoes*!" Professor Buddington says he was much amused to see that every boy in sight of him, however little attention he had been paying, wrote in his notes "Eat meat with potatoes". Probably they had done that every day of their lives; but he had emphasized it in such a way they were unlikely to forget the underlying physiological principle.

This tendency to be repetitious undoubtedly helped him in his teaching; but in his writings it sometimes became tiresome, and my mother who edited most of his books, had to cut out a good deal of it. Occasionally it would even provoke a smile on the faces of his students, as when he would describe cocci as "little round, spherical balls".

By the time Professor Buddington was at Wesleyan, my father's courses had become very popular; he not only knew how to talk with students in an inspiring way, but he also had a somewhat original theory as to a teacher's function which was brought to my attention by two things he said to me while I was in college.

Once, for instance, I asked him why elementary physics was required of all but classical students; but no courses in biology. Didn't he think biology as important as physics? "I would not want any required course in biology," he answered. "You may notice that the elementary classes in biology are almost as large as if they were required subjects. I try to make them interesting enough so that most of the students will elect them. And I think they remember more of what they learn in my classes than they would if they *had* to take the courses." Some years later the faculty voted for a required course in public health, with Professor Conn as teacher. He began those lectures with some trepidation, I think, for he wondered if he could keep his audience interested in a required

course. He managed to; and he told me, with much delight, what rapt attention the faces registered and how he was usually kept 20 to 30 minutes after class answering questions by men who came up to the desk when the lecture was over.

Another time, I gave him a piece of information which I had picked up from my friends in college, and which I thought would surprise and shock him. "Do you know," I asked, "that your lecture courses in evolution and elementary bacteriology are selected by the men who are hunting for 'snap courses'?" "I don't doubt it" was his calm reply. "I make those two courses easy intentionally. I put into them ideas I think all students ought to know, and I want as many to take them as possible. Even though they take the courses just to avoid hard work, they learn something; and I think that does more good than it would if I made the work so difficult only the best students would elect the courses."

That, in a nutshell, was his theory of teaching. The elementary courses should be given by the best lecturer in the department and made attractive to everyone; the advanced courses, however, should be adapted to the best students only, and those electing them should be expected to do hard work.

I have tried so far to give some idea of his character; I do not think this the occasion to go into detail about his career. I do feel, however, that I should say a little about his early life to show how he became specially interested in bacteriology, and hence in founding this Society.

Although he seemed to be a healthy and very energetic man during the prime of his life, he had been a sickly boy—with what complaint no one knew; and considering that he finally died of heart failure, it is not impossible to conclude that his comparatively short life may have been due to the then unrecognized condition of rheumatic fever in his boyhood. However that may be, his early illness had quite an effect on his life because he was soon taken out of public school, and at the private school where he did go, he failed to get the contact with other boys that was more normal for one of his age. He liked school well enough, and the harder he had to work, the better; the one thing which bored him most at school was not having enough to do, since he could learn a lesson so much more quickly than any of the others. His greatest pleasure was reading. He read nearly everything he could get hold of in the small public library of his home town (Fitchburg, Massachusetts), and used to take such books to school to read during idle moments under cover of textbooks he was supposed to be studying but which were too elementary to interest him. This was back in the 70's (he was born in 1859) and not many good books for boys' reading were available.

The one subject he never could master was spelling, nor did he acquire that accomplishment to his dying day. Many years later in some reminiscences of his he wrote:

"I have often wondered why I was so unable to get my spelling lessons while all others were so simple and easy for me. . . . Arithmetic and Geography and even Grammar had some meaning and some reason for being, and the facts stuck in my mind. . . . But Spelling had no meaning whatever. The same letters had different sounds in different places and the same sounds had various fancy ways of being spelled . . . and my mind refused to hold such

a meaningless jumble as the illogical arrangement of letters. Nor has it ever consented to do this, for even to this day, after years of writing and proof-correcting, I am never sure of the spelling of an unusual word. . . . I well remember an embarrassing moment when writing on a blackboard an examination set of questions one of which contained the word *discuss*. Ordinarily I could have spelled it right, but not there in the presence of the class. I felt my cheeks turning red, and finally wrote it with a single *s* at the end and a peculiar little flourish which I hoped the class would take for a second *s* provided the word had two of them and would regard as a flourish if the word had only a single *s*. . . . I like to flatter myself that the reason why I never could make a good speller was that my mind was too logical to grasp such a nonsensical, unreasonable subject as the spelling of English words!"

It is, in fact, a little amusing in reading one of his journals about our family journeying in Europe to learn that we *rowed* along a quiet country *rode*, but on another occasion *road* a boat on a river. He seems to have used all three of these homonyms interchangeably.

He told me once that an important thing for any scientist was to learn to write well, "And I don't mean good hand writing or good spelling", he added. "You can get people to correct your spelling and to typewrite your manuscript. But you must know how to express ideas clearly."

He went to college at Boston University. That proved a good place for work, but with very little college life. Such a college suited him pretty well, but it did not help him in learning to become a good mixer. He joined a fraternity, but did not seem to have cared much for the associations that brought him. One of the experiences, however, which he did look back to in later life as of much help to him was an informal attempt at teaching. In anticipation of a difficult examination in physics, the whole class decided to get together, without any faculty member, and to study for the exam. He was voted chairman, and began asking questions of the others. He soon discovered that he had to answer most of the questions himself; and was not contented with answering them, for he felt that each one called for an explanation. As a result the class pronounced him a better teacher than their professor, and he began to think that perhaps teaching should be his career.

Certain events at Boston helped him decide on biology. One of these was his attendance on a series of lectures by Professor Farlow, not at the college, but at the Lowell Institute. His lectures were on Lower Forms of Life, and Father could never forget how he began his first lecture by uncorking a beer bottle, and remarking: "You have all heard of 'popping the question'; we will now proceed to 'question the pop'!" the young listener became so interested in these lectures that he considered seriously "questioning the pop" as his particular specialty in life—only remember, there was no such subject as bacteriology known in those days.

This fact is well illustrated by an anecdote he used to tell of something which took place a few years later (in the early '80's). He was then at Johns Hopkins, studying for his doctor's degree; and in later years used to recall how a tall, lanky young man appeared on the scene and announced he wanted to study bacterilolgy. According to Father, he was not given much encouragement, and was informed that only a genius like Pasteur could study bacteriology; so he went

elsewhere. Some years later, my father had a chance to meet one of America's most distinguished bacteriologists, and was surprised to recognize him as the young man who had been unable to find any bacteriology at Hopkins. He was Theobald Smith. Considerably after my father's death, I had a chance to tell this anecdote to Dr. Smith himself, and he was somewhat amused; his recollection of the matter was that he went to Hopkins to get one particular course, and when that course was over he left. Whichever version of the story is correct, it emphasizes the fact that the nearest any student could come in the 70's and early 80's to specializing in bacteriology was to major in general biology.

The young man we are most concerned with began taking biology in his junior year at Boston University, and after the first lecture he was interested enough to come back the following Saturday to look at microscopic animals under the microscope. This interest led to closer contacts with teachers in that field, and finally to an invitation to spend some time at a small marine laboratory that had been started privately by Professor Hyatt at Annisquam, Mass. This was the beginning of many summers that he spent at marine laboratories, most of them later on at Cold Spring Harbor where he was Director for several years.

His graduation, in 1881, was apparently more or less of an anticlimax to him. He graduated second in his class and felt that some recognition of that fact should be made in the graduating exercises. No attention was paid to him, however, and that rankled in his mind for most of his life. About 30 years later he was mentioning his college days in a letter to me, and added: "My graduating day was a very unhappy one, for I felt that I had not been treated fairly, and I have never had any interest in going back to college since." Undoubtedly this disappointment contributed to his inferiority complex; nevertheless, by this time his career had taken shape and from then on he followed it to a logical conclusion, and few if any of his associates ever suspected he suffered from a sense of inferiority.

The first real progress in his career came during his years of graduate study at Johns Hopkins where he was located from 1881 till summer in 1884. There he seems to have fallen into congenial surroundings. He majored under Dr. W. K. Brooks, one of the leading zoologists of the day, roomed at Dr. Brooks' residence in Baltimore, and seems to have made a good impression on him almost from the start. Dr. Brooks was in charge of the Chesapeake Zoological Laboratory which met some summers at Beaufort, N. C., and once or twice at Hampton, Va.; he took my father with him to this laboratory every summer while he was a student at Hopkins. He also had my father give four lectures a week and assist in laboratory instruction during his last year at Baltimore. The first publications (18 to 27) signed by H. W. Conn (dealing with invertebrate zoology) were the result of his work at Baltimore, Beaufort, and Hampton.

It was while at Hopkins that he became engaged to a girl who was his sister's best friend and whom he had known for a good number of years. When she died a few years ago I found in her possession what was probably the first letter he ever wrote her, dated at Boston soon after he started his college work there, and while she was at Smith College. It is written in formal style, copybook

handwriting, totally unlike his usual scrawl, and is interesting enough so that I trust I will not be violating the secrets of the departed by reading it:

"Julia:

I wish to find out whether there is a young lady in Smith College, by the name of Hattie Herrick, and thought that perhaps you would be as kind as to inform me. The lady to whom I refer was a schoolmate of mine at Ashburnham. She entered college last year and I wish to know for especial reasons whether she is there now and how she stands in her class. If it would not be too much trouble I would be greatly obliged to you if you would find out and let me know.

I am enjoying myself very much this term. I am engaged in studies in which I am especially interested and have tickets to courses of lectures upon these very same subjects. Don't you wish you lived in Boston, with a library of 250,000 volumes, 15 or 20 free lecture courses, and other advantages too numerous to mention?

Excuse the liberty I take in writing to you, for I wish to know about Miss Herrick very much.

Ever,
H. W. Conn"

I think it quite safe to assume that his real reason for writing this letter was *not* to get information about Miss Herrick! I also think it equally safe to guess that his real reason was attained; for Julia replied promptly enough so that he could answer her letter two weeks later (the second letter of his being still in existence); in this second letter he thanks her profusely for the information concerning Miss Herrick, but then goes on to other things. I presume their correspondence flourished after that, but no more letters are in existence except one which he wrote in 1882 (his second year at Hopkins) just after he had asked her to marry him. His diary mentions writing a letter of proposal, which is no longer in existence, nor do we have her reply; but I do have a lengthy letter of his written a little later showing that she had not yet given a definite answer. This letter I will not read. I will only remark that although he was presumably trying to add a few more words to win her over, he actually filled the letter mostly with comments about his own lack of prospects and his presumption in asking her to marry him. "The life of a scientist" he writes "is at best not enviable. It is sure to be filled with trials and sacrifices."

That may have been the way to win his particular girl, however; for they became engaged that fall, although they were not married until 1885, three years later.

From things he wrote in his diary those days, we know he was full of doubt as to whether he could succeed in life. How much of this was the natural feeling of any young man just beginning his career and how much was due to the sense of inferiority which had haunted him since his boyhood, I do not know. But I do know that he had another source of mortification, about which he was so sensitive that he did not confide it, except in barest hints, even to his diary: he was so youthful in appearance that he thought people laughed at him. In those days, when almost every college student encouraged a full beard, my father could hardly persuade more than a mere down to appear on his lip—even three years after graduation. After making up his mind to teach, he must have won-

dered if he could hold his own with students, most of whom would undoubtedly look older than he did. He succeeded apparently; but he had his moments of mortification—as for example when the expressman, who was moving his belongings into the apartment where he and my mother were to live right after they were married (when he was 26 years old), made some caustic remarks about college *freshmen* who brought wives to college with them!

Herbert W. Conn, 1886. Photograph taken during his second year at Wesleyan

He was then at Wesleyan University, where he spent the rest of his life. He had accepted a position there promptly on getting his doctor's degree in 1884; he was to be in the Department of Natural History, with the idea of heading a new department in Biology as soon as that could be arranged. He was thus the founder of the Biology Department at Wesleyan, and remained its head till he died in 1917. As I have implied before, he was a born teacher; and in spite of his numerous outside activities, he never neglected his college work, but built up a strong department and turned out numerous students who made themselves famous in their chosen fields. At first his chief interest was in evolution; but during the 90's his major attention came to be directed toward bacteriology, in

which he made his greatest reputation. He began teaching bacteriology around the turn of the century, and had a big lecture class in that subject; but his laboratory classes in that subject had to be kept small, largely from lack of equipment. His laboratory instruction in bacteriology was most informal. The student was first expected to make up his own media; then he was told to plate out water, milk, or whatever else he wanted, count the colonies, and pick up a number of

Herbert W. Conn, about 1905

pure cultures. Most of the rest of the course he was allowed to play around with those cultures and to see what he could find out about them. The scheme certainly took little of the professor's time and worked fine for the brilliant students; it was not so effective in case of the less brilliant men, but my father tried to weed them out of his laboratory classes before they had gone far enough to begin bacteriology.

His outside activities during this period were almost too numerous to mention: He was Director for several years (1891 to 1897) of the marine Biological Laboratory at Cold Spring Harbor; he wrote 22 books, many of which went through several editions; he was in charge of bacteriological research at the Storrs Ex-

periment Station from about 1890 till 1906 and taught a class in bacteriology at the Agricultural College there (now University of Connecticut); he was very active in public health work, having a lot to do with the founding of the Laboratory Section of the American Public Health Association, and serving for over ten years (1906–1917) as Director of the Connecticut State Board of Health Laboratory; he was a popular lecturer in scientific fields, making lecture trips each year of quite an extended nature; he was for some 20 years superintendent of a Sunday School in Middletown; and lastly, as we all know, he was one of the three founders of this Society.

His growing interest in bacteriology during this period, of course, most concerns us. His first outstanding work in that line was in the field of dairying. When he began studying bacteria in milk, no one realized the great importance of the lactic fermentation in milk; and it was his work, with Esten, at Wesleyan, (37–46, 49, 52, 56–60, 63–67, 71, 84, 85, 89, 92, 97) that showed how the lactic acid bacteria predominated over all other types initially present. This led to various practical applications in the ripening of butter and cheese as well as in preventing spoilage of fluid milk. It is hard for us to realize today what a revolution there was, between 1900 and 1920, in the methods of milk production and distribution, during which period the change took place from the unsanitary, rapidly souring milk of the last century, to the relatively safe milk of good keeping quality which we know today. A large contribution to this revolution came from the research of the Wesleyan investigators in the 90's—followed up by the stress on sanitary control in which Professor Conn took a leading part after 1900 (62, 83, 86, 87, 89–92, 101–103).

Another important activity of his in the 90's (which in fact continued till his dying day) was in giving popular lectures. In the days before movies and radio, entertaining lecturers were in more demand than they are today; and when any member of a faculty showed talent in that line his college used to be glad to send him on lecture trips for the sake of the advertisement which it gave the institution. Wesleyan had several gifted lecturers on its faculty, some in classical and literary fields, some in the sciences; Professor Conn used to be considered one of the best of their scientific lecturers. It was for this reason that he needed to have a substitute (like Professor Buddington) present in his classes to supply for him when he was suddenly called out of town. He knew how to present a subject in an interesting way; and he also had the rare gift of knowing how to be brief. He told me, in fact, how on a trip to Schenectady he had been asked to address about 700 scholars at a high school and planned to give one of his stock lectures on bacteria which ordinarily took an hour or more but *could* be crowded into 45 minutes. Just before beginning he asked the principal how long he could have and was told *15 minutes*. He told me later it made him "hot for a minute" to think how to get 45 minutes crowded into 15. He added: "Well, I did it, and did not take more than the time suggested; judging by the tremendous applause that followed I judge I did it all right."

He seems to have driven himself hard all his life. Social life did not appeal to him and he avoided it as much as he could. His chief way of relaxing was to

turn from one form of work to another. His outside interests were many, but they were nothing that could truly be called hobbies; when he really became enthusiastic over one of them, it later became a main part of his life work. That was the way he worked into bacteriology.

His interest in bacteriology was given a tremendous impetus in 1894 when there was an outbreak of typhoid at Wesleyan, which he was successful in attributing to oysters fattened under unsanitary conditions (55). This was the first time oysters had been blamed for typhoid; and the logic he used in eliminating all other sources of infection and in definitely incriminating the oysters from one particular source made a great impression at the time in the scientific world. It not only added to his reputation, but firmly established his interest in bacteriology. It led in later years to activities in the public health field, although at first his chief interest was in dairy bacteriology.

Further stimulation was given to his interest in bacteriology by a trip to Europe he made in 1897–8, when he was granted a leave of absence from Wesleyan. I was 12 years old at the time, and remember the trip well, although I was naturally too young to understand what his chief interests were in taking the trip. I can see now, however, that he went as much as anything to take courses under distinguished biologists and to meet others in that field, but that he shortly became most interested in his contacts with German, Danish, and Swiss bacteriologists—Koch, Ostertag, Bang, Freudenreich (see 70). His trip was equally worth while to him in other ways, as he was a keen observer and much interested in the ways and characteristics of the people among whom he traveled. He wrote a day-by-day journal of the trip in which he recorded not only his doings but his impressions of the people and things he had seen; to me it is one of the most interesting things he ever wrote, more so than any of his published books. One of his comments on the German character, written nearly 20 years before the first World War is so prophetic I want to quote it, although it has nothing to do with bacteriology:

"The German makes a first-class citizen. He is willing to obey without knowing any reason for it except that it is the law. With good leaders the stolid, phlegmatic, unimaginative German can be led to almost anything, and they thus make the very best soldiers when things are all planned and go as planned. Their wars have been successful, since they have been carried out as planned. But I have an idea that if they met with reverses the whole army would lose its head and be utterly unable to readjust itself to any new condition. All this is surmise, however, but their slowness in grasping an idea makes one wonder what they could do in an emergency. As long as things go as they plan they can be relied upon, but when they once get their plans upset, then—good-by to their great power—this is my opinion."

I do not know whether or not the idea of a society of American bacteriologists occurred to him on this European trip. It is not impossible that something may have been said, in his conversations with European bacteriologsts, to put in his head the idea that America needed an organization dealing with bacteriology as a science. At all events it was only a few months after his return—December, 1898—when he had the historic conference at New York with Abbott and Jordan,

and one of the three broached the idea which led to the formation of this Society. Apparently, at that meeting, Dr. F. P. Mall, then Professor of Anatomy at Johns Hopkins, referred to either Dr. Jordan or Dr. Abbott as a "lost soul", because there was no existing section of the Association devoted to his interests, and someone suggested that the bacteriologists ought to organize a section of their own.[2] Jordan and Abbott thought the idea worth while; and the former immediately set out to find Conn. They proposed the formation not of a new section of the A.A.A.S., but of a distinctly new society. They decided, according to a statement they published a few years later that there was no society "whose nature was such as to bring together the large and growing number of investigators who are studying bacteriological topics. It was felt that the rapid development of this subject along biological, agricultural, industrial, as well as hygienic and pathological lines, is creating a special branch of science; and it was believed that an association of investigators in these various lines would be mutually helpful."

They corresponded about the plan during 1899 (See Appendix, p. 291), and I imagine the idea was broached informally to various friends of theirs during the

[2] In a letter written June 8, 1922, Dr. Abbott recorded his recollections of the founding of the Society in these words:

"At the 1899 meeting of the 'American Association for the Advancement of Science', held at Yale University, New Haven, Connecticut, Mall (the late F. P. Mall, Prof. of Anatomy, Johns Hopkins University) and I had been lunching together when Jordan (E. O. Jordan) joined us while we were having an after luncheon smoke. Mall exclaimed—'here's a lost soul', or 'an unattached wanderer' or some equivalent expression, meaning that Jordan and a number of others like him, did not find themselves at home in any of the existing sections of the Association; and there was no apparent way for this group of men to become active in the association without the organization of a new section or Society to which the bacteriological contributions could appropriately be made. He said he and Jordan had been talking the matter over and asked me if I would cooperate with certain other interested men and organize a special society into which bacteriologists as such, could be admitted and be encouraged to develop the phases of the science that made the most appeal to them.

I gladly embrace the opportunity to be of what seemed to me real service and soon found myself one of a self-appointed Committee of three, (Jordan and the late Prof. Conn of Middleton, Conn., being the other two), who took it upon itself to call a meeting of those men in the country who were regarded as most likely to look favorably upon the project and become active workers for its success."

Some time latter this comment was shown to Dr. Jordan, and in a letter of June 24, 1935, he wrote as follows to the Society's Archivist:

"My recollections of the first conversations with Abbott and Conn are a little different from those of Professor Abbott. Let me give them here.

Our first conversation took place at the meeting of the Society of American Naturalists in New York in 1898 (I think the enclosed correspondence bears this out). I was coming down the steps of some building where meetings were being held when I fell in with Mall, whom I had known well both at Clark and Chicago, and stopped to chat with him. He said, 'Why don't you bacteriologists have a Society of your own? I just met Abbott also wandering around like a lost soul.' I suppose the meeting at luncheon to which Abbott refers must have taken place a little later because I distinctly recall talking with Abbott after seeing Mall and then going with him to look up Conn."

year, but no formal step was taken until October when a letter was sent out to about 40 American bacteriologists, which ran as follows:

My Dear Sir:

An attempt is being made to organize a society of American Bacteriologists upon the lines followed by the Society of American Physiologists, the Society of American Morphologists, and the kindred associations that meet yearly as "affiliated societies", with the Society of American Naturalists. It is thought that such an association will conduce to unification of methods and aims, will emphasize the position of bacteriology as one of the biological sciences, and will bring together workers interested in the various branches into which bacteriology is now ramifying.

It is hoped that you will be willing to aid in this undertaking. Should you be willing to cooperate in the formation of such a society, will you kindly communicate with Prof. H. W. Conn, Wesleyan University, Middletown, Connecticut, before November 1st.

The first meeting will be held with the affiliated societies at New Haven, Connecticut, during the Christmas vacation. You are requested to send the titles of papers to be presented to Prof. A. C. Abbott, Hygienic Laboratory, University of Pennsylvania, Philadelphia, Pa. Titles of papers should be in Professor Abbott's hands not later than December 10th. A program giving details as to time and place of meeting, titles of papers, etc., will be issued later.

A. C. Abbott
H. W. Conn
E. O. Jordan

This letter, I think, was neither duplicated nor mimeographed (those processes being hardly perfected in those days) but was either handwritten or done individually on the typewriter; neither did college professors have stenographers at their call at that early time. Because of pressure of other duties, Dr. Abbott did not send out any of these letters; the other two members of the self-appointed committee apparently apportioned the job between them. I definitely remember quite a number of those letters going out from our house. The replies, as directed, came to my father, and in many cases there was follow-up correspondence. According to the statement published by the three men later, "The responses to this letter were immediate and emphatic. With the exception of three, all to whom the letter was sent responded and in every case was expressed a feeling that the organization of such a society was not only feasible, but eminently desirable."

The meeting for organizing was held at 2 P.M., December 28, 1899, at the Medical School Building of Yale University, with 30 in attendance. Prof. W. T. Sedgwick of the Massachusetts Institute of Technology was invited to take the chair, and he presided at all the sessions that year. Dr. Abbott had prepared a program, of which seventeen papers were presented at this meeting; including one by H. W. Conn on "Natural varieties of bacteria"; eight other papers were listed on the program but not presented. At the third session, December 29th, the committee to draw up a constitution reported and the first constitution of the Society (perhaps 200 words long) was adopted; Dr. Sedgwick was elected President; Dr. Abbott, Vice-President; and Dr. Conn, Secretary-Treasurer. It was decided that the original membership should be those responding to the

letter of October 1899 and any others who had attended the first meeting. Thus computed, there were 59 original members.

The New Haven members of our Society have long taken pride in being located at the birthplace of our Society. A 30th anniversary meeting was held there in 1929, and a 40th in 1939, and hopes were expressed that the 50th anniversary would also be held there. It is a matter of much regret to the New Haven members that the Society has now outgrown the facilities afforded by their city, so that we cannot meet there next year. The best we can do is to meet in Baltimore in 1950, just 50 years after the *second* meeting of the Society was held in *that* city.

At the original New Haven meeting it was decided to limit active membership to 100; but the Society grew much faster, even in those days, than its founders had thought possible, and it proved necessary to raise the limit occasionally to avoid a long waiting list. When my own application for membership came up in 1910, the limit was either 200 or 250, and there was just one vacancy. There was another name besides mine being considered for membership; and to avoid having to choose between us two, the Council made Father an honorary member, thus creating one more vacancy on the list of active members. After that it was a joke of his that they dropped him from the active list to put me on. Of course, a few years later, the Society removed all limit to the membership, the better to support the Journal of Bacteriology, and we all know how rapid its growth has been since. If my father, in 1898, when he first conceived the idea of the Society, could have pictured this meeting, 50 years later, at which is son is presiding, with a registration of about 1000, and a membership of about 3,500, he would have been decidedly surprised!

But to come back to the early days. The membership dues were $1.00 a year, and the Secretary-Treasurer was allowed to use the *tremendous* sum thus realized for clerical help. It would not have gone far toward hiring a stenographer, but I believe he hired my mother to type his form letters to the members. He held that office till the 1901 meeting at Chicago (the 2nd meeting of the Society having been held in Baltimore in 1900); at Chicago he was elected President, and Jordan became Secretary. His influence in the Society continued for many years after he was out of office, however, and he was a perennial member of the nominating committee. Officers were elected quite informally in those days: a nominating committee was appointed by the President at the first session, which reported at a later session, with a single candidate nominated for each office. This put so much responsibility on that committee that serving on it became embarrassing to my father eventually, and I can remember how in his last few years (1914 to 1917) he used to stay away from the first session so that he could not be put on that committee. He lived just long enough to see the Journal of Bacteriology started and the membership limit removed, but not to see the Society begin its rapid growth in members.

Such were his activities, or at least all I know of them, in connection with this Society in its early days. To round up this brief account of his life, however, I should say something about the nature of his activities during his last 15 years.

The only one of his earlier activities which he gave up at this time was his connection with the College and Experiment Station at Storrs. He gave this up at about the time he became director of the Board of Health Laboratory; so the result was a decided shifting of his chief interests from the agricultural to the public health field. This Board of Health laboratory was somewhat unique at its time. About the only practical methods of laboratory diagnosis then known were for diphtheria, pulmonary tuberculosis, and typhoid, and facilities for making such tests were available only in large cities. Connecticut was one of the first states to realize that the telephone and more rapid mail service made it possible to extend such a service to small communities located some distance apart. It was decided that Connecticut was small enough in area but sufficiently densely populated to make statewide service of the sort practical. The venture proved a success, and directing that laboratory occupied much of my father's time for the rest of his life.

It also brought him into closer contact with other workers in the public health field including Dr. C.-E. A. Winslow and Dr. C. E. North, who are still with us, and brought about very close relationships with Dr. William H. Park of New York City. One of his first dealings with Dr. Park in this matter was in 1900 when he and Dr. North made a visit to the laboratory where Dr. Park, with one assistant (Dr. Anna L. Williams, I believe) was undertaking for the first time to make bacterial tests of milk. They found him installed in one small room over a horse stable in East 15th St., not even knowing how to make the tests required. My father showed him on that occasion how to make plate counts on ordinary gelatin and on lactose litmus gelatin—the latter medium being one of my father's own devising and which he had used as the basis of his important work on the milk flora.

That was how the outstanding work of Dr. Park's Board of Health Laboratory in New York City started—with very humble beginnings. It was also the beginning of the association of these three men in their fight for the sanitary control of milk. They conceived the idea of sanitary milk standards, and pushed the idea for all they were worth (See 101, 102, 103). By the time my father died in 1917, the battle was practically won, but it had been a hard fight. For a man who always hated a fight, and had started out in life with an inferiority complex, he certainly obtained an amazing reputation as a fighter in those days—for there were strongly intrenched interests in the dairy industry then who were not at all anxious to see such standards enforced, and who managed to secure other prominent bacteriologists to fight their battles for them. It required many a hard-fought battle at meetings of the A.P.H.A., in legislative bodies, and at court trials, before such standards were generally adopted and enforced. The men who were associated with my father in these activities have been amazed when I told them of his inherent timidity and hatred for agrument; they insist he kept those qualities well hidden from them!

I have nearly finished my subject; but I would like to say a few words of a more personal nature—concerning my own relations with him. In some ways I never felt as close to him as I might have been—as my only sister was, in fact;

but there was never any serious misunderstanding between us. Quite the contrary, I think he handled very well the problem I posed for him. It must have been quite a blow to him when he realized that his son, whom he knew to have certain qualities that made for success, and on whom he had undoubtedly been pinning hopes for the future, was becoming hard of hearing. Yet he never once made the mistake of showing undue sympathy or of letting me assume that success would be impossible because of the handicap. On the other hand he encouraged me to take advantage of any other sense or facility I possessed to overcome the difficulties I faced. I think it was largely due to him that I avoided the error so common among deaf people of becoming over-sensitive, and at the same time assumed from the beginning that the handicap was not going to keep me from success in life. On looking back now, I think I owe him more gratitude for instilling that attitude in me than for the indirect way he steered me into bacteriology—although I also appreciate the latter.

Perhaps I cannot end this account better than by quoting my sister. As I have already remarked, she was closer to him than I was in many ways; and long after he died, she wrote to me in reminiscence about him:

"Although he did not often speak of intimate things, what he said once he meant always. He never spoke until he was sure, and then not again. Indeed like most New Englanders he was blunt and not given to flattery. Like them he did not want to begin saying 'nice things', for as he explained, he would then have to keep saying them. If he liked something or someone and said so, he meant it, and there was no need of repeating.

His odd ways, you say, sometimes bothered you; while they tickled and delighted me. His indifference to so much that others held as important; his impatience with 'small talk'; his equal impatience with fancy desserts ('sweetened nothings' he called them; the only desserts for him were apple pie, mince pie and squash pie); his love of hearing rain on the roof; his horror of being late or of being kept waiting himself; his quick hurt at being interrupted or not listened to while he was talking; his dislike of dressing up to go out for the evening—these and many other traits gave him a peculiar charm to my eyes as I began growing up."

That sums him up better than I could in my own words; and all I need add relates to his passing on. In 1909 or 1910 (when he was about 50) he first realized that his heart was weak and that he must cut down on physical acitivity. Doing so was a hardship to him as he had always been energetic and so very active mentally as to be most unhappy when doing nothing. To stay at home because he had no way of going anywhere except walking, and to be dependent on taxis or other conveyances for local transportation on all his trips was very annoying to him. Even so he kept going, and a little before he died he wrote me a letter telling about his numerous engagements on a trip he had just taken which made me feel tired when I re-read it recently. I suspect such activities tired him also.

His death came suddenly, although we had known for years it might come at any time. He died on April 18, 1917, the very day that my daughter (who is now a member of this Society) was born. The suddenness was hard on those he left behind; but I am sure he would have had it that way could he have chosen. I cannot think of a better epitaph for him than one remark he made in the letter I just mentioned: "I don't seem to be put on the shelf yet."

APPENDIX

CORRESPONDENCE RELATING TO THE FOUNDING OF THE SOCIETY OF AMERICAN BACTERIOLOGISTS

Between

H. W. CONN, E. O. JORDAN and A. C. ABBOTT

Chicago, Ill., April 6th, 1899.

My dear Prof. Abbott:—

I enclose a list of names as a starting-point for our proposed Bacteriological Society. Doutless names that should be added to the list will occur to you and perhaps some names that I have included should be stricken out. After we have once organized other members,—and I think there are many desirable ones, —could be chosen.

I would suggest that a circular letter be sent to the men we finally decide upon as most available in this connection, and I will write out a rough draft of what I have in mind and send it to you for revision in a few days. I have sent a copy of the list to Prof. Conn, asking his opinion in regard to desirable additions and subtractions. Should we send out our circular letter this Spring or wait until October?

Very truly yours,
EDWIN O. JORDAN

Middletown, Conn., Sept. 27, 1899.

Mr. E. O. Jordan,

Dear Sir:

I have waited about returning the enclosed list, thinking that the best time to send out the circular letter would be October. I have added a few names to the list, but have not thought wise to omit any.

It seems to me that you have included only desirable men in your list, and that all should be invited to join the proposed section of the Naturalists' Society.

I think that a circular letter should be sent out now as soon as possible inviting men not only to join, but to present papers at the meeting to be held in New Haven next Christmas. I hope that Prof. Welch can be prevailed upon to act as President for the first year, at least.

Let us now push the matter as much as we can for a month or two, and I feel confident that the section will then be a success.

I should be very glad to see a rough draft of the circular letter, as you suggest, although I have perfect confidence in your ability without submitting it to me for revision.

Trusting you can now push matters to a successful issue,

I am, respectfully,
H. W. CONN

Chicago, Illinois, October 9th, 1899.

My dear Prof. Abbott:—

I enclose a draft of a circular letter concerning the formation of our proposed Bacteriological Society. I have also sent a copy to Prof. Conn and he will have it sent out to the gentlemen named on our list. If you wish to add any names to the list, will you kindly forward them to Prof. Conn. Both Prof. Conn and myself have added a few since the list was sent you last Spring, but there may still be some that we have overlooked. Would you be willing to undertake what I have suggested in the circular letter—namely, the arrangement of the program for the New Haven meeting? Prof. Conn, who is near New Haven, will arrange details as to time and place of meeting.

Would it be possible for you to see Prof. Welch and gain his consent to preside at our meeting of organization? If so, will you kindly communicate with Prof. Conn, in order that some statement to that effect may be inserted in the circular letter.

Trusting that we may now push matters to a successful issue,

I am,
Very truly yours,
EDWIN O. JORDAN

Philadelphia, Oct. 12th, 1899.

Prof. E. O. Jordan,
Chicago University,
Chicago, Ill.

My dear Prof. Jordan:

I must confess that your letter received this morning brings me face to face with a very serious matter,—namely, the organization by us of the new Society. During our conversation in New York last winter I agreed to support it, and shall keep my promise, but I say frankly that I have so many irons in the fire that I do not know at this moment how much support I can give it. At all events, if you think I can be of assistance in arranging the program I will be very glad to undertake it, but I must look to you and to Prof. Conn and to every one else interested in the Society to aid me in getting up a decent program for the first meeting. I have just written to Prof. Welch, asking him to preside at the meeting of organization, and as soon as I hear from him I will let you know concerning the matter.

Very truly yours,
A. C. ABBOTT

Middletown, Conn., Oct. 18, 1899.

My dear Prof. Jordan:

Your letter is received and I am glad to undertake my share of the work. I have prepared a letter similar you sent, with some additions, and have already sent a copy to each person in the list of addresses.

I will let you know later what responses I receive.

Yours truly,
H. W. CONN
(J. M. C.)

PUBLICATIONS BY H. W. CONN

Arranged Chronologically

I. Books

1. CONN, H. W. 1886 Evolution of Today. G. P. Putnam's Sons, New York.
2. —— 1891 The Living World. G. P. Putnam's Sons, New York.
3. —— 1897 The Story of Germ Life. D. Appleton and Co., New York;
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   in England, 1909, *as* Germ Life. Hodder and Stoughton, London.
4. —— 1899 The Story of the Living Machine. D. Appleton and Co., New York; 1915 *2nd edition*;
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   in England, 1912, *as* Story of Life's Mechanism. Hodder and Stoughton, London.
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6. —— 1901 Agricultural Bacteriology. P. Blakiston's Son and Co., Philadelphia; 1909 *2nd edition*.
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8. —— 1903 An Elementary Physiology and Hygiene for use in Schools. Silver, Burdette and Co., New York;
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9. —— 1903 Bacteria in Milk and its Products. P. Blakiston's Son and Co., Philadelphia.
10. —— 1904 Evolution of Today. G. P. Putnam's Sons, New York.
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12. —— 1907 Practical Dairy Bacteriology. Orange Judd Publ. Co., New York.
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14. —— 1909 Advanced Physiology and Hygiene for use in Secondary Schools. Silver, Burdette and Co., New York.
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26. —— 1884 Life history of Thalassema. (*Doctor's thesis*). Id. **3**, 351–401.
27. —— 1884 Method of formation of trochosphere in Serpula. Johns Hopkins Univ. Circs., **4**, 15–16.
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