Henry Pickering Bowditch and the Development of the Harvard Laboratory of Physiology

FREDERICK W. ELLIS, M.D.

PAUL L. McLAIN
SCHOOL OF MEDICINE
UNIVERSITY OF PITTSBURGH

Reprinted from
THE NEW ENGLAND JOURNAL OF MEDICINE
Vol. 219, No. 21, pp. 819-828, Nov. 24, 1938

HENRY PICKERING BOWDITCH AND THE DEVELOPMENT OF THE HARVARD LABORATORY OF PHYSIOLOGY

FREDERICK W. ELLIS, M.D.

NEWTON CENTRE, MASSACHUSETTS

IN THE year 1929 a great physiological conference was held at the Harvard Medical School. As the writer stood before the magnificent group of buildings and was jostled by the crowd from the four corners of the earth, he was saddened by the thought that there was probably not more than one other member of that International Physiological Conference who could cover with his own recollection the modest beginnings of physiological research in the Harvard Medical School, and its ascent, at first slow, and then rapid, to its present commanding position in the institution. The story of that ascent is an inspiring one, but I fear that most of the scientists who labor in the marble palace now devoted to the pursuit of physiology in the institution where systematic teaching of the science as a university function began in this country are ignorant of that story, which should teach them a useful lesson, and create an appreciation of their good fortune which may not now exist. What is true of Harvard University is true of other institutions in our country. Physiology has come into its own in America in the last fifty years. In no other part of the world has there been such a spectacular change in its status and material resources. The medical profession has finally recognized the truth that physiology is the basis of scientific medicine.

It was in Massachusetts that physiology began its rise in America. We might illustrate that rise with a curve beginning with a time abscissa corresponding with 1871, when Henry Pickering Bowditch, just home from the laboratory of the world's greatest teacher of physiology, began his career as a full-time assistant professor in the Harvard Medical School. Physiology began then to be appreciated in our country as a basic medical science, as well as a subject of academic interest; but that recognition came very slowly, even from the most enlightened medical contemporaries.

The writer well remembers that while he was riding on a railroad train in the early eighties, Reginald H. Fitz, then a professor of pathology, sat down beside him and asked what he was doing at the time. When he was told that this recent graduate of Harvard Medical School was occupied with physiology he expressed the opinion that physiology had done about all that it could for medicine. This opinion came from one in thorough sympathy with science, and from one who was destined to be known soon as a great discoverer in medicine. Fitz thought then, as most well-educated physicians in America thought at the time, that physiology had little real promise for medicine, although he was one of the few medical men of that period who evinced an intelligent interest in the young man's scientific work. But what have physiology and physiological methods not done for medicine since that interview with Fitz on a commuters' train to the North Shore? At that time Fitz's science was an anatomical one; now it is largely a physiological one. Subtract the physiological discoveries that have been appropriated by clinical medicine since that ride

with Fitz, and what is left of modern medicine? Bacteriology and the sciences which have been annexed to it are largely physiological branches of biology. Ductless glands were then an enigma, and hormones and vitamins were unknown. Biochemistry was just appearing above the horizon, and physical chemistry, which has become an indispensable tool of physiological and clinical research, had not been named. Clinical research has become applied physiology, and many men in the best modern hospitals are working on what are essentially physiological problems with methods and apparatus borrowed from the physiological laboratory. The scientific aspects of medicine have been turned topsy-turvy, for some of the most important contributions to physiology in recent times have been made by pathologists and clinicians.

A few years ago an old friend of mine, a university professor, was desperately ill from an attack of pernicious anemia, a disease that was uniformly fatal in the elder Fitz's day. He was practically bedridden, and could not cross the room unless supported, and then only at the risk of fainting. About two years later I saw him while he was on a vacation at his son's farm. It was a hot summer day, but he had been seeking physical and mental refreshment by running a mowing-machine in the hayfield. It is unnecessary to state who were responsible for this miraculous change in his condition. Minot and Murphy did this great thing, and back of them was Whipple, who forgot that he was a pathologist, and trespassed on the domain of physiology. It is astonishing how many scientific discoveries of the first magnitude are being made by people who

have no business to make them, for they make conspicuous the paucity of the output of those who ought to make them and do not. Insulin and diabetes naturally occur to us in this connection. A young man with slight claim to the title of physiologist, and with a limited experience in medicine, with the able assistance of others makes one of the greatest of medical discoveries, and one that is a physiological discovery.

The few men now living who entered the Harvard Medical School sixty or more years ago have seen a wonderful revolution in medicine, and I, who am of their number, have seen things equally wonderful happen in physiology and to physiology since that time. I became a student in the Harvard Medical School in 1877, and made my acquaintance with Bowditch's attic laboratory on North Grove Street about the time that the present Higginson Professor was six years old. The school was little changed from what it was when Bowditch joined the staff in 1871, and the faculty was nearly the same as it was then. Bowditch was a full professor, but I suspect that the emoluments of his post could be cared for easily. The policy which had been inaugurated in the school, in 1871, by President Eliot was in successful operation. The institution had been pretty thoroughly inoculated with German science, which had begun to take, and had already produced a good deal of a blush. The ambitious graduates of the school no longer flocked to Paris for their professional polish, but Germany and Austria were their objectives, and it was German science and methods that Bowditch brought home with him for his pupils. While Bowditch was a student in Germany and for a generation later, German science overtopped that of the rest of the world, and

German physiology attained its high-water mark at that time. President Eliot was a chemist, and was well aware of the merits of German science. He, no doubt, set his trap for Bowditch early; in fact, I think that we may say that Bowditch was predestined for the position which he attained. In those days the best in human values was thought to be located in Boston, or in its contiguous territory, and Bowditch originated in that territory. Although President Eliot was quite capable of making a mistake, a fact which I am unaware that he ever publicly acknowledged, he did the best possible thing in making Bowditch the connecting link between the German laboratories and the Harvard Medical School. Germany was then the acknowledged leader of the world in physiology, and Bowditch became the representative of that leadership in the United States.

By starting our imaginary curve at the year 1871 we do not imply that there had been no notable achievements in physiology in this country prior to that date; but it was then that the science first received definite and intelligent recognition from university authorities, and began to be taught and cultivated in an organized and efficient way. This momentous beginning was due to President Eliot, who was probably the first president of Harvard who had an adequate appreciation of the scientific needs and deficiencies of his institution. The medical school was one of the first departments of the university to which he turned his attention and especial interest, and that interest never flagged. I am of the opinion that the greatest work which he did in the university was done there.

The base line of our imaginary curve is not a zero line. Very creditable work in physiology

had been done in this country before Bowditch established his laboratory, but the great significance of that laboratory was due to the fact that it was a research laboratory open to qualified medical scientists who had been bitten by the bug which compels a man to search for new facts in a particular branch of his profession. In addition to his German training Bowditch had another eminent qualification for the position to which he was called, for he belonged to a devoted and intelligent family in easy circumstances that was ready to finance his scientific career. In those days the recompense which one received from following such a career consisted largely in allowing someone else to pay his bills. In honoring Bowditch for his part in the rise of physiology in this country we should include in that honor the family that made it possible for him to accomplish what he did. Now the circumstances under which a physiologist works in the United States are so changed as to seem almost incredible to the older generation that remembers the lean and Spartan years. Science and money are now in alliance, and to a certain extent are interchangeable. Some people have expressed a wish to "soak the rich," a class which for a generation, and until recently, has been inundating this country with every description of educational, artistic, scientific and charitable institution, until we have become the envy and despair of other nations, but I do not expect to hear any loud denunciation of this class from the physiologists who are now enjoying the comforts of a real home. If we should construct a curve of the rise of physiology in this country during the last fifty years, and another curve of the increase of our national wealth in that period, I think that we should find them to be almost exact replicas of each other.

At the time that Bowditch became a professor of physiology it was a gracious Harvard custom to allow an incumbent of a new chair of science to furnish his apparatus, and so he purchased and brought home from Europe instruments that he knew that he would need. In thus providing himself with facilities for work he had predecessors in the university. President Eliot's first teacher of chemistry, Josiah P. Cooke, was appointed professor of chemistry in 1850, but it was necessary for him to illustrate his lectures with apparatus from a juvenile laboratory which he had established in his father's house, and his first laboratory in Harvard was in a cellar. Formerly when a newly appointed professor of science timidly approached the authorities for a place in which to work, he was led to the entrance of some subterranean locality and was told that he might establish his laboratory there. He was fortunate if he did not need a ladder to get into it, and rubbers after he arrived at his destination. Bowditch needed no rubbers in his first laboratory, for it was situated just below the rafters at the top of the building in which it was located. I have a vivid recollection of that laboratory although I never worked in it, as all my research work with Bowditch was done in the Boylston Street laboratory during the first three years of its existence. The main room of the old laboratory, which was not a large one, had for its principal furniture a table for experiments, a sink, a workbench and foot lathe and a tub for frogs. A smaller room which was shared with Quincy, who had just been appointed an assistant in histology, contained cases for instruments and a few books. This was the laboratory and equipment that Bowditch used for twelve years. I do not think that

he had an official assistant for the first five years of that time, but George M. Garland became his assistant in 1877. The appropriations for the physiological laboratory were certainly meager. On one occasion in my undergraduate days I experienced a desire for a frog for some scientific purpose, and I ventured to approach Bowditch for a donation from his store. He very gravely and courteously informed me that the funds at his disposal did not warrant a reckless extravagance of that kind, and then the natural goodness of his heart overcame him and he went to his tub and seized the most diminutive batrachian in it, with which I departed with mingled feelings of exultation and remorse. This little incident illustrates the conditions in the physiological department of Harvard in 1878. Five years later Bowditch moved into his Boylston Street laboratory, which was considered palatial, and was probably the best in America at the time, and which to me was one of the most attractive spots on earth. The physiological curve made a large jump upward at this time, and the gradient has been a very steep one ever since.

During my undergraduate days the medical school was housed in a dingy building at the end of North Grove Street amid decidedly humble surroundings that had probably seen better days. The morgue was near at hand, the jail was not far away, and the appearance of the plain brick structure was that of an impecunious relative of the university to which it belonged. I gazed upon it for the first time with the subdued emotion which the tourist feels when he first sees the Pyramids. The building was then only thirty years old, and was in good company, for the Massachusetts General Hospital was just behind it—

an institution that has always been so closely knit to the medical school as to seem to be an integral part of it. The class to which I was added had ninety members, and of these only thirty-five had academic degrees; but this fact should not be considered as evidence that the school was functioning on a low plane. Nearly all the students were on a par so far as essential premedical education was concerned, except that a few of the college men had had general chemistry. For the most part the students with whom I was associated, whether college graduates or not, worked hard and were a credit to Harvard, and to the profession in which they afterward achieved success, and some of them distinction. If anyone believes that medical education in Harvard in 1877 was at a low ebb, he is mistaken; but it must be remembered that scientific medicine had just begun to climb upward then, and that the greater part of it that is now familiar to us was then unknown.

When I entered the medical school there was much there to remind us of the past, and especially of the great discovery with which our institution was so intimately connected. Figuratively speaking the atmosphere of the school was permeated with sulfuric ether. Not many rods away was the amphitheater where Warren demonstrated to the world that Morton's method of annulling consciousness in surgical operations was "no humbug." Another John Collins Warren, the grandson of the first, and a genial friend, scholar and gentleman, was a young instructor in the school. Oliver Wendell Holmes, who coined the word "anesthesia," and who joined the faculty in 1847, a year after the ether demonstration, was the idolized professor of anatomy who lectured from

one to two in the afternoon, and could make us forget the pangs of hunger at that unconscionable hour. He became at that time another Orpheus charming the inmates of Hades with his lyre. He could even make osteology delightful by embellishing it with poetry, and interlarding it with anecdote. Our professor of surgery was the distinguished and picturesque Henry J. Bigelow, with a patriarchal beard that made him look like Michelangelo's Moses, who was a survivor of those present at the first ether operation in the adjacent hospital. It was he who first proclaimed the discovery of anesthesia. We had the privilege of hearing him speak of his acquaintance with Morton, and of Morton's frequent and prolonged visits to promote enthusiasm in the subject that engrossed his thoughts. We also had another and agreeable reminder of Morton and his ether in my class in the person of an unassuming and popular young man who was Morton's son.

The discovery of ether anesthesia was the greatest that had been made in practical medicine up to the time that it was announced by Bigelow in the *Boston Medical and Surgical Journal* a few weeks after the memorable operation on October 16, 1846. That message astonished and stimulated the whole surgical world, and it came from the Harvard Medical School. We should not forget that Bigelow was a product of the school, and was soon to become one of its teachers, like his father. John Collins Warren had long been a distinguished professor there. Morton, who administered the ether, was then studying in the school, and Charles T. Jackson, who is said to have suggested ether to him, was a graduate. It was the prestige and authority of the Massachusetts General Hospital, which was manned largely

by the officials of the Harvard Medical School, that gave ether anesthesia to the world. This is the truth to remember in all debates on the priority of the discovery. That discovery was a physiological one, and it also became of the greatest service in physiological experimentation.

When I began my course in the medical school Bowditch was a stalwart young man of thirty-seven, who seemed to be the embodiment of health and energy, and walked with an eager, springing step, as if he were anxious to get somewhere and had no time to lose. The instruction which he gave in the school was by lectures illustrated with excellent demonstrations, and by quizzes and conferences. A number of the students were given subjects for study in the available literature, and then reported their findings at meetings in which they were discussed. This was an excellent way for broadening and deepening the information which they obtained from the lectures. The course in physiology in the Harvard Medical School was a complete refutation of the view that real instruction in science cannot be given to students without individual work by them in a laboratory. I have less hesitation in saying this because I have been an incurable addict to laboratories of various kinds for more than sixty years. The elementary laboratory is desirable and useful, but its results depend largely upon the kind of student who enjoys its advantages. It is the student who enjoys his work there, and leaves it with reluctance, that derives a benefit from it commensurate with its cost. Knowledge obtained in the laboratory may leave as easily as it came. However, our class did have an infinitesimal amount of practical instruction in the attic laboratory. We were divided into

squads, which were summoned to the laboratory twice during the first year. I remember that on the first occasion that I enjoyed this privilege Garland showed us how to make the classic nerve-muscle preparation from the frog's leg, and that on my second visit Bowditch was on hand to initiate us into the mysteries of graphic registration on the smoked drum. This was the tiny germ from which the present luxurious growth of laboratory instruction in physiology in the Harvard Medical School was derived.

While I was an undergraduate student Bowditch did considerable research work in co-operation with his assistant, Garland, or an advanced student or outsider. He also contributed independently a few notes or articles to the English *Journal of Physiology*. During the first twelve years of Bowditch's experience in the faculty of the medical school his work was a grind that he no doubt enjoyed, but all the while he was dreaming of better things, which came with startling swiftness after they once started. After he moved into the new building on Boylston Street a new era began, and his dreams came true.

During my three years' absence from the medical school, after I graduated, I had no very direct knowledge of what was transpiring in its physiological laboratory, but I have no reason to think that the investigations carried on there during this period were numerous. The English *Journal of Physiology* was established in 1878, and immediately became the vehicle for the publication of the most important physiological work in Bowditch's laboratory. In the years 1880 to 1883 two important articles were published: one by Bowditch and Southard, and another by Bowditch and

G. Stanley Hall. Bowditch seems to have been the only physiologist that had observations published from his laboratory in the years that I have mentioned. The above is not a fair summary of the work done there during this period, for the research on the vasomotor nerves by Bowditch and Warren was commenced, I believe, in the old building on North Grove Street, and it is possible that Ernst was beginning his bacteriological work there in 1883, before the new medical building was completed, but it certainly had not advanced far. The three or four years after Bowditch moved into his new laboratory spanned the golden period of his scientific research in America, and it was my good fortune to see much of that work, and to be intimately associated with him in his laboratory. After 1890 I am unaware of any major scientific work which he published under his own name. Probably his main ambition had been to give physiology standing and resources in this country, and when he saw these grand objects largely achieved he was content to lay down the oars, and let the boat glide along under its own momentum. He had been a good physiologist, and did important work that may well serve as a good example to those that follow him. He was a good and faithful teacher, but I think that that kind of occupation irked him after a while, but he had well earned the satisfaction which he must have felt when he saw the magnificent group of buildings arise on Longwood Avenue, due largely to him, and when he knew that Boston had become one of the great medical centers of the world.

In the year 1883 Harvard Medical School celebrated its centennial anniversary, and dedicated its new building on Boylston Street. I went back

to attend the celebration three years after leaving school. I was especially anxious to see the new physiological laboratory, and when I saw it I was captivated by it, and worked in it for three years thereafter. Bowditch had designed it with loving care, and had utilized every spot and crevice in it. It was almost regal in comparison with the Spartan quarters in the grim building on North Grove Street from which he had just emigrated. No child was ever more delighted with his new toys on Christmas morning than Bowditch was with his new laboratory. A few weeks after I first entered it, as I was talking with Wallace, the mechanic, in the dusk of the late afternoon, we could hear Bowditch wandering from room to room in the gathering gloom, until he came where we were located and said that it was hard for him to leave the place because it was so attractive. He had great reason for his satisfaction, for he now had the best physiological laboratory in the country, and had just been appointed dean of the medical school that was generally thought to be the best in America. The building was not only attractive in itself, but was conveniently located in the newer part of Boston, in full view of Copley Square, as no buildings intervened at that time, and was surrounded by the Victorian architecture of the Back Bay, which was then unsmudged by bituminous coal. The building is sometimes called the "old" one by those who were sheltered by it at a later date than myself; but to me it still has in recollection all the youthful charms that it exhibited as a parvenu in the Back Bay.

When the new building was dedicated Oliver Wendell Holmes made the principal address, and in his final burst of eloquence he expressed the hope that as time rolled on it would stand

where it was, foursquare and upright, as a monument to Aesculapius, and for the use of future generations of his disciples. I may not have reproduced the exact words of the eloquent doctor, but I have, no doubt, expressed his wish and expectation. The building is still there, but twenty-three years later the school picked up its effects and moved into the magnificent group of five other temples of Aesculapius, where it is now located. That simple statement tells most emphatically with what marvelous strides medical education, with physiology in the van, was advancing in the United States in that short period. The step from North Grove Street was a very long and important one. The year 1883 marked the beginning of a new epoch in the Harvard Medical School, and that same year, and a few more in the same decade, formed the culminating point in Bowditch's scientific career. He was then in the prime of life, and had become an acknowledged leader in his science. His new laboratory was probably the most important scientific center in Boston, and was the focal point for many visitors from Europe as well as the United States. He was filled with enthusiasm for research, and appreciation of the great opportunities that had come to him and his institution. Some of the best scientific work of his career was done during the first few years that he passed on Boylston Street, and then he merged himself in the work of his pupils and of the school as a whole.

Bowditch's administrative duties as dean of the medical school must have absorbed a good deal of his time and energy during the first few years of the existence of the new building, which had then many distinguished visitors who wished to see both him and the attractive edifice which

was his pride and joy. William Osler attended the dedication of the new building, and I think that it was a day or two later that I met him in the physiological laboratory, and was introduced to him by Bowditch. I had never seen him before, although we had had a slight correspondence on a subject of mutual interest. At that time he was a little known young professor of a miscellaneous aggregation of subjects known as the Institutes of Medicine, in McGill University, Montreal. The institutes in his case meant histology and pathology, with physiology thrown in gratis. Although he lectured on physiology his practical work was largely microscopic, for it was recorded that he was somewhat diffident in the presence of a kymograph. During his visit to the laboratory a friend remarked to him that it was impossible for a laboratory worker to engage in practice. There was a large element of truth in this statement, as anyone who has tried the combination of practice and research will be willing to admit, although some resolute and indefatigable individuals like John Hunter have succeeded with both, even though Hunter sometimes vented his displeasure in a pungent remark when he was obliged to go forth to earn his guinea.

But it is frequently possible for a man to exchange his laboratory for the general public, and become a conspicuous success in practice, and we have no more shining example of this kind of man than Osler. For ten years he broadened the foundations of his future work by the study and teaching of physiology and pathology in a medical school, and during this time the public passed him by, and very little private practice came his way. For a part of these ten years he was a visiting physician to a hospital, and a hospital ward may

be a kind of laboratory. In less than a year after I met Osler in the Harvard laboratory he was called to a professorship of medicine in Philadelphia. Five years later Johns Hopkins had him, and not long after that the whole world knew him, and physiology and pathology had their triumph.

For a little time after I became an inmate of Bowditch's laboratory he was fully occupied with his new duties as dean, and as I had no very definite ideas regarding a problem to work on, I was obliged to wait for suggestions from him. I began some work in the pathological laboratory of William F. Whitney, and continued it for a while, but dropped it after I became fully occupied with physiological activities. Bowditch's assistant at this time was Joseph W. Warren, who became associated with him in 1881. Warren was born in Springfield, Massachusetts, and received his medical degree in Germany. I do not think that he ever engaged in practice. He gave some instruction in elementary laboratory work to the undergraduates, and ably seconded Bowditch in his researches, but did very little independent investigation. Bowditch and Warren had evidently begun their well-known work on the vasomotor physiology of the cat in the old medical school, and they brought their apparatus to the new one, where they were soon ready to continue their plethysmographic research.

At the suggestion of Bowditch I undertook an investigation of the vasomotor physiology of the frog. I began with a microscopical study of the circulation in the web of the curarized animal when the peripheral part of the cut sciatic nerve was stimulated in various ways. A small artery of the web was observed under the microscope, and the diameter of the vessel measured

with a micrometer eyepiece while the nerve was being stimulated. The observations were made at regular intervals, and their duration measured with the metronome, and then a graph of the results was made. Satisfactory results were obtained in these experiments, but the work was very fatiguing, and I soon resorted to the plethysmograph, with which permanent and continuous records were easily made. The frog soon proved to be a most convenient and satisfactory subject for this kind of experimentation, as it could be kept for days under curare, and required no artificial respiration. As the piston-recorder for air transmission had not been invented then, I used very small and delicate Marey drums for recording the changes in volume of the hind leg. The results of my vasomotor experiments were very similar to those obtained by Bowditch and Warren in their work with the cat. Contraction of the blood vessels was easily excited with tetanic stimulation, and dilatation was frequently obtained with slowly repeated single-induction shocks of appropriate strength, but dilatation after stimulation as the effect of degeneration of the nerve was doubtful. I found on microscopical examination that the frog's sciatic nerve degenerated very slowly, if at all, after cutting. I also made a very careful study of the cut nerves of the cats used in Bowditch and Warren's experiments, and found advanced degeneration after the third day, and my report of my findings was published as an appendix to their paper.

Although Bowditch and his assistant discovered nothing entirely new in their research, they systematized and corroborated what had already been discovered regarding the vasomotor nerves of the limbs, and introduced an accurate method of study-

ing and recording their activity. The apparatus for recording was practically the same as that employed by Mosso in studying the pulse, and described and illustrated by him in his monograph, *Die Diagnostik des Pulses,* published in 1879, but was modified to control the temperature of the water in it. The great value of the research was largely due to the deliberation and meticulous care which Bowditch and his assistant devoted to it, for they did not publish their results until four years later. That research became a classic, and the only criticism to which it is open, and one that applies to my own as well, is that the secondary coil of the induction apparatus should have been calibrated with a ballistic galvanometer in order that more exact data regarding intensities could have been given. The ubiquitous dry cell had not come into use in those early days, and whenever I needed an electric current I was obliged to go to the battery closet in the gallery of the laboratory and set up the Grove cell which supplied the central station on the floor below. Bowditch was proud of that little wooden tower, which housed the variable interrupter, and where all the electric wires of the laboratory converged, but its use did not contribute to easy working. There were no electric street currents for the distribution of light, heat and power in those days, and consequently there were no electric lights, and none of the convenient little electric motors that are now so useful in laboratories of every description. Bowditch's artificial respiration apparatus was run by a water motor with water supplied by a tank in the upper story of the building, but he loved that apparatus and showed it with pride to the numerous visitors whom he conducted about his premises. He had a regular itinerary, which

included a visit to my frog, — when I had one working, as I usually had, — and I am inclined to think that most of the visitors were more thrilled by the sight of that insignificant animal recording its secrets on the smoked drum than by anything else that they beheld in the establishment. I recall a French count who spoke no English, and about whom it was whispered that he was a great duelist, who was especially moved at the spectacle.

As Bowditch was dean at the time about which I am writing, it fell to him to receive the visitors who wished to see the new building. He did double duty then, both as dean and as a physiologist who was decidedly proud of his laboratory. I think that there were only few of such visitors who were not shown the laboratory. I remember seeing there Sir Robert Ball, the Astronomer Royal of England, who was no doubt attracted to the place from his desire to meet the grandson of the distinguished Nathaniel Bowditch, the mathematician and navigator. One day Lord Rayleigh, the eminent English physicist, and Lady Rayleigh were conducted about the laboratory by Bowditch. John C. Dalton, the physiologist, appeared there on one occasion, and Bowditch brought him to my table and introduced me to him. I had studied his textbook before I entered the medical school, and I regarded him as a sort of physiological patriarch, although he was then twenty-two years younger than I am at the present moment. Any awe that I may have felt was soon dispelled by astonishment when I saw him take a cigar from his pocket, and with the permission of Bowditch light it, and saw for the first time in history that laboratory desecrated with tobacco smoke; for smoking was strictly forbidden there.

Bowditch's personal friends were frequently seen in his laboratory, and some of them called him Henry. William James and Stanley Hall sometimes appeared, and professors in other departments had business there. Two visitors who came several times, and who made a particular impression on me, were John S. Billings and S. Weir Mitchell. There seemed to me to be an especial bond that drew Bowditch and these two men together, and I think it possible that the bond was their war experience, but I never heard Bowditch speak directly of that experience. Once in a lecture to my class in my undergraduate days he stated that he had often slept while he was traveling on horseback. This statement no doubt referred to the time when he was a cavalry officer in the Civil War, but he did not say so, and I never heard him allude directly to the Civil War. I remember that a military procession passed through Boylston Street on one occasion, which was probably Memorial Day. As Warren and I watched the soldiers through the laboratory window, Bowditch stood near by and looked at the company without any comment. He could be reticent, and was always modest. Not long ago I consulted the full and careful notes of his lectures which I made in 1877 and 1878 to find what he said about the all-or-none properties of cardiac muscle at this time. What he said was brief. He stated that the extent of the contraction of voluntary muscle corresponds within narrow limits to the intensity of the stimulus, but that contraction of the muscle of the heart under constant conditions does not vary in this way. He did not mention that it was he who made the interesting discovery regarding the heart.

Weir Mitchell was interested in the work of the

len to zero; he did not breathe; there was no pulsation of the mercury in the manometer. As some sort of action seemed called for I suggested that we give the inanimate body a hypodermic of ammonia, which was done. I do not know just how strong that ammonia was, for it was taken from a reagent bottle, but it was sufficient. After some patient waiting the animal gave a little sigh, and later began to breathe more vigorously; the heart commenced to beat, and the mercury to go up, and after a while we had a live dog again, although the blood pressure attained only about two thirds of its original height. Bowditch was summoned from his library, and took his place at the table without any comment on the transformation that had taken place, although I had witnessed one of the most interesting and instructive experiments of my experience. I have had some difficulty explaining to myself just what the ammonia did to that dog while it was devoid of respiration and circulation, but I have always retained a great respect for ammonia as a cardiac stimulant.

At the end of the academic year of 1884 Bowditch left his laboratory for a year's vacation in Europe. Warren delivered the lectures on physiology during the next year, but with what success I do not know, as I did not attend any of them. As he had charge of Bowditch's notes he no doubt made good use of them. I continued my work in the laboratory, and, as formerly, made it my headquarters when in Boston. I think it was at this time that Warren engaged in his independent research on the action of alcohol, in which I sometimes rendered a little manual assistance, but I hasten to say that Warren always drank the alcohol.

In the spring of 1886, after I had completed my work on the vasomotor nerves, Bowditch asked me to engage in experiments on the action of ether on the sciatic nerve of the frog. I was occupied with this research for a considerable time, and the results that I obtained are hard to reconcile with the present all-or-nothing theories of nerve conduction and muscle activity.

Bowditch was not a general biologist like H. N. Martin, who commenced to teach biology and physiology in Johns Hopkins University in 1877. Martin's early experience at Johns Hopkins duplicated closely that of Foster at Cambridge, England, for both were responsible for the teaching of a number of subjects which are related or basic to physiology, as well as physiology itself, and both founded schools covering the whole field of animal biology. This Cambridge characteristic of Martin's department at Johns Hopkins was in decided contrast to that of Bowditch's department in the Harvard Medical School. Bowditch was only concerned with physiology, and he was even something of a specialist in that science. His work and interest was in that division of physiology which was in the closest contact with that of the master in the Leipzig laboratory. He is said to have studied histology with two of the best French and German teachers of that science, but although he made some necessary allusions to microscopic structure in his lectures, I cannot recall, in my three years of intimate association with him in his laboratory, ever seeing him making any independent use of the microscope. He was obliged to advert to some chemical matters in instructing his class, but he unblushingly disclaimed any interest in chemistry, and attributed that indifference to the fact that he was color blind, and could not dis-

tinguish the colors of precipitates. This excuse always impressed me as decidedly thin, for John Dalton, the founder of modern chemistry, was color blind and gave his name to that defect. In physiology Bowditch stuck closely to his last, and never attempted to do anything but peg his kind of shoe, and having finished the shoe to his satisfaction, he was even willing to turn the hammer over to others and let them peg the other shoe. But the work which he did perform was that of a thorough and well-trained workman. The number of researches in which Bowditch personally engaged were not very numerous, but they were conducted in a very conscientious manner, and might well serve as patterns to some of the younger generation who are sending to market too much immature veal, instead of the heavy beef for which the scientific public is yearning. There is too much bread-and-butter research, at the present time, on the part of those working for a position, and too little work that is actuated by an irrepressible urge to question Nature.

After 1886 I did no more work in the Harvard laboratory, although I sometimes saw Bowditch there or elsewhere, and sometimes heard from him by letter. I commenced practice in 1887, and became so thoroughly immersed in ophthalmology that I had little time for anything else. My physiological work became more and more restricted to the physiology of vision and physiologic optics and closely related subjects.

In the summer of 1887 I received a letter from Bowditch stating that a movement had been started to establish a physiological society, and inviting me to join in it. I remember that it was hot weather then, for I asked that the place for the meeting be one where cooling drinks could be

obtained, but I heard nothing further of the matter until the first meeting of the Society was called in New York, in December of the same year, for organization and the election of officers. I attended that meeting, of which Weir Mitchell was made chairman and Martin secretary. Seventeen men were present, and all were either actively engaged in physiological work or in closely related sciences. This was the beginning of the American Physiological Society, whose semicentennial anniversary was celebrated in Baltimore last April. These seventeen men represented the twenty-eight original members, eleven of whom were not present at the first meeting. Of the original members only five survive; all of them attended the organization meeting in 1887, and four were present at the recent celebration in Baltimore. The United States and Canada were pretty thoroughly combed to make the original group of twenty-eight members, and only about half this group were professional physiologists. The other original members were not actively engaged in physiological research, but regarded it as very essential for the advancement of biology and medicine. The society founded then has become one of the great scientific associations of the world, and the parent of the American Society of Biological Chemists, the Society for Pharmacology and Experimental Therapeutics and the Society for Experimental Pathology. The combined membership of the parent society and of the organizations which parted from it, but are still closely affiliated with it, numbers more than twelve hundred. The leap from twenty-eight to twelve hundred demonstrates most vividly what has been accomplished in physiology and scientific medicine in America in the last fifty years. Bowditch was one of the most active found-

ers of the society, and lived long enough to enjoy the satisfaction of knowing that he had contributed much to a second great factor in the development of biology and medicine in this country.

I continued to visit Bowditch's laboratory for a while after the foundation of the American Physiological Society, and sometimes met him at its meetings, but our ways diverged more and more until I lost my health and gave up practice. As I was recovering from my protracted and very severe illness I learned that Bowditch had resigned his professorship, and I knew that he too was ill, and as a scientist had faded from the picture. A flood of memories of the happy times I had spent in his laboratory impelled me to write to him. I was not able to write myself, so I dictated a letter as I lay on a couch. I told him how much the three years that I spent in his laboratory had meant to me, and how much I regretted that he could no longer pull the stroke oar in his institution. He sent back a very kind, but a pathetic letter. He seemed to be philosophically resigned to his affliction. He typed the letter, he said, because his shaking hand would not allow him to write. He seemed greatly pleased that the authorities of the medical school had given him a room in the new building which he had helped to erect, and he hoped to haunt the institution for a while longer. He expressed a desire that we should meet again in Boston, but we never did. When he departed, a fine gentleman of the best New England type left the Harvard Medical School greatly his debtor, and also left medical education in this country on the high plane that he used to dream about under the rafters of the old medical school on North Grove Street.

Henry Pickering Bowditch was extremely fortunate to be born in the time and place to which fortune assigned him. When he began his work in the Harvard Medical School the resistless tide of modern science was about to overtake medicine, and to urge it forward with ever-increasing speed. The virgin resources of our country were being rapidly converted into wealth which would soon be at the disposal of the Harvard Medical School, where it could be used intelligently and effectively for the good of humanity. The hope of the world now lies largely in scientific medicine, and the Harvard Medical School is still devoting its high ideals and great resources to its advancement.

Copyright, 1938, by the Massachusetts Medical Society.

Printed in the U. S. A.