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BIOGRAPHICAL MEMOIR WILLIAM GILSON FARLOW  
1844-1919

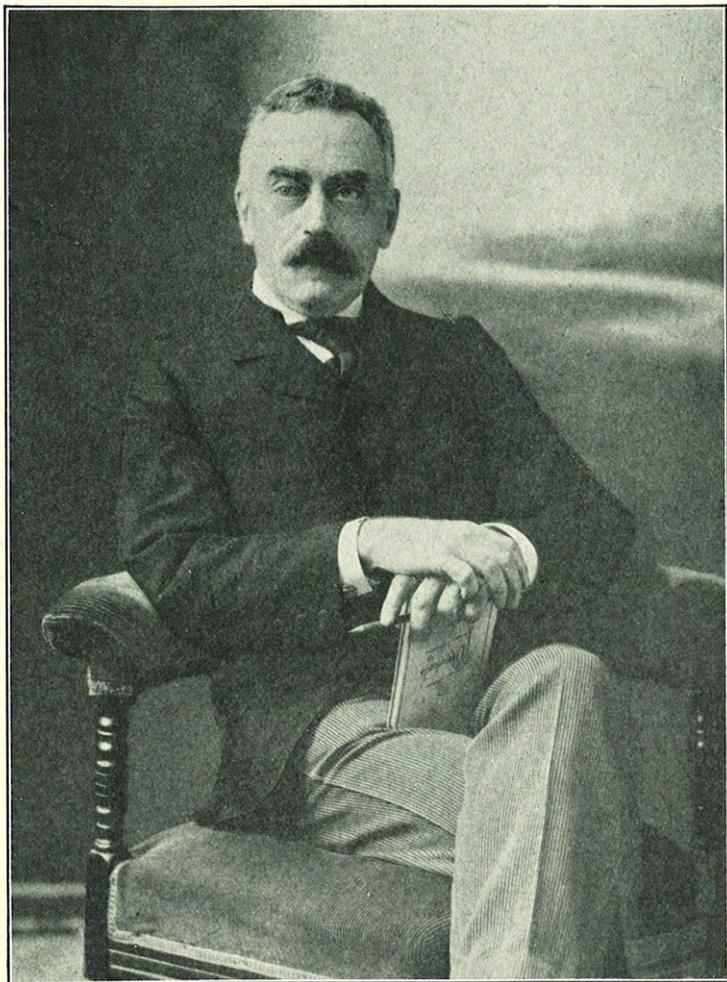
BY

WILLIAM ALBERT SETCHELL

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*W. G. Harlow*

# WILLIAM GILSON FARLOW

1844-1919

By WILLIAM ALBERT SETCHELL

William Gilson Farlow was born in Boston, Mass., on December 17, 1844. He was the son of John Smith Farlow and Nancy Wight (Blanchard) Farlow. He received his early education in the public schools, both of the grammar and high school grades. He entered Harvard University in 1862 and received the degree of B. A. in 1866. He entered the Harvard Medical School in November, 1867, and received the degree of M. D. in May, 1870. He was appointed assistant to Asa Gray, Fisher professor of natural history in Harvard University, in July of 1870, continuing in this position for two years. In June, 1872, he sailed for Europe, where he traveled and studied for somewhat over two years, returning to America in the summer of 1874. In the same year he received the appointment of assistant professor of botany at Harvard, with the particular field of cryptogamic botany, giving instruction both at Cambridge and in the newly established Bussey Institution at Jamaica Plain. In 1879 he was appointed professor of cryptogamic botany in Harvard University, with teaching entirely at Cambridge. He continued to teach until 1896, at which time he withdrew from all work along this line except as to advising and assisting certain graduate students. He married Miss Lilian Horsford, daughter of Eben Horsford, in 1900. He died June 3, 1919, having served in the faculty of Harvard University as assistant professor and as professor for 45 years and having advanced to the position of senior member.

As a boy and undergraduate student, William Gilson Farlow seems to have had strong inclinations toward music and botany. In these respects he resembled his father, John Smith Farlow, born in Boston in 1817 and educated there, who, besides being a successful man of business, member of the Legislature of the State of Massachusetts, president of the Massachusetts Reform Club, for many years president of the Newton Public Library, etc., was also for a time president of the Handel and Haydn Society of Boston; and, although with no critical knowledge of botany, was very fond of plants, was a member of the Massachusetts Horticultural Society as well as that of Newton and was awarded many prizes at their exhibitions. During his boyhood and youth, William Gilson Farlow followed his father's likings for science and the humanities and gave many evidences of the same alert and active mind as well as capabilities for comprehensive grasp of fundamentals which characterized his later life. He was twice awarded the Franklin medals for scholarship in the Boston schools. In college he was a member, and secretary for one year, of the Pierian Sodality, acting as pianist and several times soloist at its public concerts. His unusual musical ability attracted the attention of his instructors in music and J. K. Paine, professor of music at Harvard, urged him to take up music as his chosen profession. He was an inimitable story-teller, even in his younger days, and likewise took part in amateur theatrical performances. He was also secretary and treasurer of the O. K. Society in his junior year. He was a member of the Harvard Natural History Society and curator of its herbarium, and his scientific attainments were held in high esteem by his classmates and fellow collegians as well as by his instructors. He was elected secretary of his class at senior class election. He was accustomed to explain his habit of casting quick glances from side to side and slightly upward by saying most humorously that when he was a freshman he was much smaller than he was later on in life and that the sophomores used to throw water out of the second-story windows on the freshmen as they passed the dormitories. Consequently, in watching out for the sophomores at their windows he acquired a lifelong habit. At graduation, filing answers to questions asked by the class, he stated that he had "no definite plans for life."

During the year following his graduation he continued his botanical interests and, taking the advice of Gray and following in the footsteps of most of his botanical predecessors in this country, resolved to proceed to the doctorate in medicine as a preliminary and possibly also as an alternative to entering the field of botany. He spent a portion of this year in studying anatomy under Dr. Jeffries Wyman, in Cambridge, himself an enthusiastic naturalist, and entered the Harvard Medical School in Boston in November, 1867. He entered upon and carried through his medical studies with the zeal and thoroughness characteristic of him, and at the end of his third year he won a coveted appointment as surgical interne at the Massachusetts General Hospital in Boston under the distinguished surgeon Dr. H. J. Bigelow. He obtained his medical degree in May, 1870, and his duty done, his anchor cast to windward, he relinquished the favorable opportunity of advancing in medicine as he had earlier that of entering upon a career in music.

His medical education finished and with no intention of practicing, he returned again to Cambridge, studying with Asa Gray and even helping Gray with his classes. He was formally appointed to an assistantship with Gray in July, 1870, in which position he continued for two years. He succeeded Horace Mann, who had died during Gray's absence in Europe, and at Gray's request attempted to give more instruction in cryptogamic botany than had hitherto been given. With such an inspiring and enthusiastic principal as Gray, he undoubtedly absorbed and otherwise gained an extensive knowledge of the flowering plants and vascular cryptogams, but his chief attention seems to have been directed toward the marine algæ, of which Gray had obtained a considerable collection for an American botanist through his friendship and connection with William Henry Harvey, who had written the *Nereis Boreali-Americana*, the first account of our American marine algæ, and who with J. Whitman Bailey had worked over and reported on the algæ of the United States exploring expedition, from Charles Wright, from J. G. Agardh, of Lund, and others. He used to tell, with some amusement and for the instruction of those who later were studying in this same field, of his lack of the sense of the importance of certain numbers noted on some of the specimens and how he nearly lost for future generations the valuable specimens distributed by Harvey from Ceylon, from the Friendly Islands, and from Australia. At the same time, another of Gray's pupils, Daniel C. Eaton, of Yale University, was occupying himself with marine algæ and both cooperated with the United States Fish Commission under Spencer F. Baird in work on the southern coast of New England. Farlow spent the summer of 1871 at Woods Hole, on the southern shore of Massachusetts, with the wonderful corps of naturalists which Baird had assembled there. Eaton afterwards joined with Farlow and Dr. C. L. Anderson, of Santa Cruz, Calif., in issuing a series of dried specimens in fascicle form of the marine algæ of North America, but soon relinquished the algæ into Farlow's exclusive charge. During this period of his life, Farlow came into correspondence with J. G. Agardh, of Lund, in Sweden, sending him many rare specimens and receiving determinations, criticisms, and specimens in return. He began during this period of his assistantship to prepare and publish his earlier papers on the marine algæ. During his assistantship he introduced the study of the lower cryptogams into the Harvard curriculum, a novelty in American educational practice.

It will be of interest as well as instructive to glance for a moment at the botanical situation in the United States at the period of Farlow's assistantship to Gray (1870-1872). Gray himself, at 60, was meditating retirement from teaching and administrative duties and was negotiating with Charles Wright as to the work in the herbarium. He was also preparing for the addition of a lecture room and laboratories (completed late in 1871). Sereno Watson was with Gray at that time, whither he had proceeded (1870) to complete his account of the plants of King's expedition. George Lincoln Goodale, who joined Gray as assistant, was destined (1873) to take over the subject of "Vegetable physiology." The corporation of Harvard University had started (1870) the organization of the Bussey Institution, a school of agriculture and horticulture, for which plans had been made by the founder as early as 1835 in a will proven in 1842 and funds turned over to Harvard University by the trustees of the founder in 1861. John Torrey was still alive and professor of botany in Columbia University, although in the last

years of his life (†1873). Daniel C. Eaton, pupil of Gray and grandson of Amos Eaton, was professor of botany in Yale University, with his specialty in ferns, but publishing, at this period, his few papers on marine algæ. Sullivant was still alive but was not publishing. Lesquereux and James had taken over the mosses and C. F. Austin the hepatics. Edward Tuckerman was professor of botany at Amherst College and was writing his classical papers on American Lichenology. Charles H. Peck, at Albany, was acting as State botanist and beginning to publish on fleshy fungi. T. F. Allen was beginning to publish on American Characeæ. In Europe, Hofmeister, although still active, had passed on his mantle and task of preparing a comprehensive *Handbuch der Botanik* to Sachs and to De Bary, and the oncoming generation was deeply immersed in what has been designated the "vegetations-punkt" type of investigation. J. G. Agardh, at Lund, was veteran in phycology, as Elias Fries was in fungi, while Müller, at Geneva, was working on the *Flora Brasiliensis*, with lichens as his hobby. Schwendener (1860–1868) was publishing the series of papers on the algal types of lichen-gonidia and was bringing about the fundamental and spirited discussion as to the possible dual nature of the lichen-thallus, which was to be prominent for so many succeeding years. Bornet and Thuret were the foremost exponents of algal morphology and reproduction. Of great importance to Farlow were all of these, but possibly foremost in influence for his chosen profession was the fact that Sachs had produced the second edition of his epoch-making *Lehrbuch der Botanik*, which had not yet been translated into English and which had not, at the time, made the profound impression outside of Germany which it later created. It seems worth while to mention the situation outlined above, since it, as limited, had a direct influence on Farlow and his work. Even the oldest of our prominent American workers of the present day and even those of Europe were, at this time, not advanced beyond the grades below the university. There was no strictly botanical periodical in America except the *Bulletin of the Torrey Botanical Club* (1870– ), botanical articles being few in production and published in the *American Journal of Science* or the *American Naturalist*, but mostly in the proceedings of the few learned societies of that era, such as the American Academy of Arts and Sciences of Boston, the Boston Society of Natural History, the American Philosophical Society of Philadelphia, the Philadelphia Academy of Sciences, the St. Louis Academy of Sciences, and the California Academy of Sciences. The National Academy of Sciences was not founded until 1863.

From the point of view of the condition of botanical science and teaching in America, where the "college" point of view still held the most considerable place in higher education and where "research" was not, as yet, spelled with a capital "R," it is little to be wondered at that Farlow, having followed the botanical tradition of preparing himself in medicine, having associated himself with teaching in Harvard University, following his own natural bent, and in accordance with Gray's sympathetic advice and desire for extending the bounds of botanical instruction and specialization at Harvard, should have been attracted toward the lower cryptogams and have turned his attention to northern and central Europe for the assistance he needed for his further training and orientation in this field as well as in general. As Farlow himself says later on in his life (1896): "It certainly now seems ridiculous that one who had only just finished his medical studies and knew nothing about cryptogams beyond what he had read in leisure moments or had picked up in the field should attempt to teach the subject. But the young are courageous, not to say audacious, if they are not learned, and, it must also be admitted, the demands of students for information on the subject were easily satisfied at that time." Consequently we find him leaving Cambridge and Gray at the end of the second year of his assistantship and setting sail for Europe in June, 1872, where he spent the next two years in study and travel. He burdened himself with specimens, particularly with algæ, many of them from the Oregon and California coasts, collected by E. E. Hall, and C. L. Anderson. Landing in England, he proceeded at once, via Copenhagen, to Lund, in Sweden, to consult and absorb wisdom from J. G. Agardh, the founder of phycological taxonomy. He has left us a glimpse of his visit and experiences at Lund in the charming and characteristic letters to Gray, of which only too little was published in the *American Naturalist* in 1874. Among the west American marine algæ submitted to Agardh at that time were the specimens upon which he founded the

genus *Farlowia*. From J. G. Agardh, during this visit and by his correspondence, Farlow was assisted in fixing determinations of his later lists (1875 and 1876) of the algæ of the United States, as well as many which he never published but passed on to his own disciples. From Lund he went to Stockholm and on to Upsala, where he met Elias Fries and his son Th. Fries, authorities in taxonomic lichenology and fungology. Farlow tells in his biographical notice of Edward Tuckerman how the elder Fries recalled the visit of the American lichenologist whose sharp eyes detected, as they strolled on the famous avenue near the University of Upsala, a species of lichen which the elder and most famous lichenologist had never seen there. It is to be recalled also, in connection with the visit to Upsala, that the younger Fries was bringing out his comprehensive work on Scandinavian lichens (1871-1874).

From Sweden, Farlow went to Norway for algæ and then on to Petrograd (St. Petersburg) to examine the collections at the Imperial Academy of Sciences collected by the Lütke expedition (1823-1827), and those collected later by Wosnessenski for the Imperial Academy, on the northwestern coasts of America. These had been studied and reported upon by F. J. Ruprecht and were apparently in the same condition and arrangement as when left by him. From Petrograd, he went to Moscow, thence on to Berlin and Cologne, and finally to Strassburg, to the laboratories of Anton de Bary, in the newly established German university in the territories recently wrested from France.

Anton de Bary was at that period easily the first and foremost plant morphologist in the world, and his students were drawn from all countries. In his laboratories Farlow met many of the future leading botanists of central and northern Europe. Two of these, both Poles, J. Rostafinski and E. Janczewski, became his especial intimates, and after their work at Strassburg was over they journeyed on into France together, or at least met again at the Villa Thuret. De Bary and Sachs were both associated with Hofmeister in his plan for issuing a comprehensive *Handbuch der Botanik*. De Bary had already published his remarkable work on the morphology and physiology of the fungi, lichens, and myxomycetes (1866), in which it is noticeable that the bacteria were not included as they were in the revised edition (1884), and was at work on his comparative anatomy of the vegetative organs of the flowering plants and ferns (published 1877). Farlow found at Strassburg a master and his disciples deep in the work of testing and advancing botanical knowledge in extensive fields. Of the three associated more closely, Rostafinski gave the world a monograph of the Mycetozoa (1873 and 1875), Janczewski elucidated the development of the ascogonium in *Ascobolus* (1871), and Farlow investigated and described the first known case of apogamy in ferns (1874). In De Bary's laboratory Farlow learned and practiced the microtechnique of that day and learned much as to methods of instruction, literature, and the work of his contemporaries. Since De Bary paid much attention to the parasitism and saprophytism of fungi and the reactions of host plants to their parasitic forms, we may readily infer that Farlow received much inspiration for the work he instituted on his return to America on phytopathology. It was at this time, as he related later, that he became acquainted with Sachs's textbook (second German edition, 1870), for which his admiration never ceased.

Farlow fully occupied his stay of two years abroad. Besides his work in De Bary's laboratory, he visited Switzerland, becoming acquainted with its Alpine flora, both as to flowering plants and cryptogams, especially the lichens. He settled down for a while at Geneva, where Johann Müller-Argoviensis assisted him in his study of the rich lichen flora of that locality. From his notes as to this part of his stay, we learn that he did not neglect the fungi in his collections and studies. During the stay abroad, Farlow found opportunity of spending some time at the Villa Thuret at Antibes, with G. Thuret and E. Bornet, in phycological studies. Rostafinski and Janczewski were also there. The two French phycologists were foremost in the study of the morphology and development of the algæ. Thuret's masterly series of papers on the zoospores and antheridia of plants, with their superb illustrations (1850-1853), his researches on the fertilization of the Fucaceæ (1855-1857) and, in connection with Bornet, the solution of the cystocarpic development in the red algæ (1867) had marked a new epoch in such study, and his taxonomic work, although he published little in this line, was

based on a thorough knowledge of both the morphology and development of the living plant as well as on the work of his predecessors. Bornet, his coadjutor, was fully his equal and was destined to become Farlow's most beloved and revered friend and correspondent for the rest of his life. Farlow used to say that Bornet was the only botanist he knew who made no real mistakes. Bornet was at the time making the famous study of the algal nature of lichen-gonidia, soon after published (1873). The Bornet and Thuret publications, *Notes Algologiques* (1876 and 1880) and *Etudes Phycologiques* (1878) are the most outstanding in the realm of phycology. As a result of their work at Antibes, Janczewski published most important papers on the propagula of the Sphacelariaceæ (1872) and on the methods of growth of the thallus of the brown algæ (1875), as well as papers on the structure of *Porphyra* (1872) and the development of the cystocarp in certain red algæ (1877). Rostafinski also published several papers on algal structure (1875-1877) and was inspired to begin a revision of the Laminariaceæ, which never came to other than preliminary publication, but which nevertheless had its influence. Farlow, while publishing nothing as an immediate result, was influenced most profoundly in his later work on the algæ, particularly in the studies leading up to his *Marine Algæ of New England*.

It is impossible to follow all the wanderings of Farlow during his two years abroad, but it is sufficient perhaps to say that he visited Paris and various places in Germany, Italy, England, and Ireland, to examine type specimens, to visit and consult with various botanists, and to familiarize himself with their floras, both phænogamic and cryptogamic. He returned to Cambridge late in the summer of 1874, well equipped in every way to take up the work in his chosen field. He brought with him many authentic specimens, much in the way of literature and notes, and had annexed a host of sympathetic correspondents to assist in developing exact knowledge of our lower cryptogamic orders.

From 1874 to 1879, Farlow was attached particularly to the Bussey Institution, although he gave a certain portion of his time to cryptogamic instruction at the Botanical Garden in Cambridge. I have previously mentioned the Bussey Institution, the idea of which was in the mind of its founder, Benjamin Bussey, of Roxbury, as early as 1835, but which, because of the conditions of the bequest, did not come into active existence until 1870. The workers here were at that time F. H. Storer, dean, and in charge of agricultural chemistry; D. D. Slade, in charge of applied zoology; and C. S. Sargent, at the Arnold Arboretum (established 1872), in charge of arboriculture. It is to be borne in mind that the continent of North America possessed few agricultural colleges or agricultural courses in universities, the majority of the older of these institutions being founded in the late sixties or early seventies. Of stations for agricultural experimentation there were few in Europe, the first, that of Moeckern, near Leipzig, having been organized in 1851, the Rothamstead station, under Lawes and Gilbert, having started somewhat later, and at the time of the inauguration of work at the Bussey Institution between 30 and 40 in Europe all told. The first strictly agricultural experiment station in North America (Connecticut) came into existence in 1875. Storer, with S. W. Johnson, of Yale, and E. W. Hilgard, of Mississippi, Michigan, and finally of California, were developing agricultural chemistry in this country, Storer being particularly interested in the chemistry of fertilizers. The Bussey Institution was intended for several classes of students, both for those not intending to proceed to a degree and those who were candidates for one. We may not wonder, then, that Farlow's work in connection with the Bussey Institution was primarily directed toward the fungi of economic interest and that he laid there, firmly and efficiently, the foundations of what has come to be known as phytopathology. The papers published by Farlow in the *Bulletin of the Bussey Institution* and elsewhere, between the years 1876 and 1880, show by their titles and content the trend of his interest toward the taxonomic, physiological, and pathological aspects of the fungi, although he still paid very considerable attention to the algæ. His papers on potato rot, diseases of oranges and olives, the downy and powdery mildews, particularly of the grape, the black knot, onion smut, the reddening of salted codfish, the deteriorating effect of certain lower algæ and related organisms in water supply, all are models of their

kind and indications of his activity in connection with the work laid out for the Bussey Institution and the Massachusetts Society for the Promotion of Agriculture, the latter furnishing the plates for Farlow's articles.

During his connection with the Bussey Institution, Farlow also gave instruction in cryptogamic botany at Cambridge two days a week, in a primitive laboratory in Lawrence Hall, and also summer-school instruction at Cambridge and in the marine algæ at Woods Hole, Mass., in what he calls an "improvised laboratory." He had certain advanced students, the first of whom, Byron D. Halsted, later professor of botany at Rutgers College and botanist of the New Jersey Experiment Station, took for his thesis subject: "A classification and description of the American species of Characeæ" (Boston Soc. Nat. Hist., Proc., vol. 20, pp. 169-190, March, 1879).

In 1879, as he tells us (1896, p. 2), the diminished income from the Bussey funds caused a suspension of his instruction at the institution and he was transferred to Cambridge, with the appointment to a professorship of cryptogamic botany, the first recognition of the equal standing of the lower plants with the higher, "cryptogamic" being adopted as a portion of his title, "in order," to quote his own words (1896, p. 9), "to point out the existence of this branch of botany as a proper field for study in this country." Farlow was now 35 years old and had established firmly cryptogamic botany as a worthy branch of university instruction and attention. He was free to devote himself to the building up of his own branch of botany as Asa Gray had in his time, and from even less beginnings, built up his wonderful structure and equipment of phænogamic botany. A room was assigned for laboratory and herbarium in the building of the Lawrence Scientific School, whence it was removed to the attic of Boylston Hall, later to the lower floor of the east wing of the Museum of Comparative Zoology, then to the third floor of the Agassiz addition to that building, and finally to the upper floor of the central or botanical section of the museum building, where it met other divisions of botanical instruction. The botanical establishments at Harvard University have always been scattered and are scattered even at the present day, but during the last years of Farlow's life, economic botany, histology, and physiology were housed in the same building with cryptogamic botany, while the Gray Herbarium and the Arnold Arboretum were more or less distant from them. The period in Farlow's life extending from 1879 to 1896 represents the time of his active teaching of larger as well as of smaller classes and of graduate students. Among his earlier advanced students and assistants of this time was William Trelease, and somewhat later Roland Thaxter, the former soon becoming immersed in work on the morphology and taxonomy of the higher plants, particularly after becoming the first director of the Missouri Botanical Garden, and the latter continuing on with the fungi and becoming Farlow's successor, to carry on the work of placing the great Farlow Herbarium and Library on a permanent basis for growth and influence.

About the year 1885 there came into Farlow's laboratories George Howard Parker, Benjamin Lincoln Robinson, Robert Paine Bigelow, William McMichael Woodworth, and James Ellis Humphreys, who brought with them a true biological spirit and introduced some innovations in botanical methods. Some of this group brought with them from the zoological laboratories the method of embedding in paraffin, and used this technique in their cryptogamic research, probably the first application of this method in any botanical laboratory. About 1887, A. B. Seymour was appointed assistant to Farlow and began his long association with the cryptogamic herbarium and preparation of indices of species and host plants of North American fungi. In the fall of 1887, began my own four years of connection with the cryptogamic laboratories, first as Morgan fellow and later as assistant in biology, and with me, in the laboratory, besides Seymour as assistant, were Kingo Miyabe and W. C. Sturgis. From this time on the cryptogamic laboratories became the shrine toward which the pilgrimages of the cryptogamic students of the United States and Canada were directed. There may be mentioned H. M. Richards, G. J. Peirce, C. L. Mix, T. W. Galloway, L. M. Underwood, E. A. Burt, R. A. Harper, B. M. Duggar, Hermann Schrenck, George T. Moore, and others, most of whom finished up one or more short papers with Farlow or began research work to be reported on later.

In 1883, Farlow began to issue the important series of papers entitled "Contributions from the Cryptogamic Laboratory of Harvard University."

In 1891, intending to relieve himself of routine teaching and to take a trip to Europe, he gave over the teaching of cryptogamic botany to Roland Thaxter, who was called from his position as botanist of the Connecticut Agricultural Experiment Station at New Haven, Conn., for the time being, resuming only graduate instruction in 1892. This, also, he finally relinquished in 1896, in his fifty-second year, although he remained helpful in matters of advice and reference to the end of his life. After 1896, however, the younger generation did not come into intimate contact with him or share to any considerable degree the benefits arising from his direct suggestion and criticism.

After 1896, Farlow devoted himself largely to furthering the projects which had been in his mind, in building up the material basis for his subject, devoting his time to clearing up undetermined and current specimens, preparing material for a future distribution, pushing forward the work on the bibliographical index of North American fungi, and to answering the multitudinous letters asking for advice or assistance on critical points in cryptogamic taxonomy and literature. He was compelled also at this period to assume certain large responsibilities in the business affairs of his family, which made serious inroads on his time and energy. He carried through all these matters with his usual energy and thoroughness, shaping his affairs so as to leave all in orderly fashion when his end might come. Fortunately, he continued able to go on with his work of all kinds until a few weeks before he passed away quietly, conscious and calm until his last moments. There passed away at the close of this last and by no means least active period of his life the dean of American botanists, one who had created more than one subdivision of botany, pure and applied, in North America, who had led, generally directly, but at least indirectly, to the highest goal of attainment practically all of the surviving botanists of his country. He left behind him a sorrowing wife, a host of ardent pupils and followers, and, as a further heritage, collections of books, specimens, notes, drawings, and indices unequalled for work along the lines of cryptogamic botany. His memory remains green and will continue to live with us, his pupils and associates, and his example will continue for the inspiration of generations to come.

The character of William Gilson Farlow was too many-sided for any one person to appraise, record, and attempt to make plain, especially to those who have not had the privilege of prolonged personal contact. To those of us who knew him well little need be said as to his personality and accomplishments. To those who knew him only from his writings or from the treasures of specimens and books which he brought together there is some fair indication of his energy, wisdom, and farsightedness. For the coming generations there is desirable some expression, feeble and inadequate though it must necessarily be, as to his loveliness, his kindness of spirit, his regard for truth, and straightforwardness. I am thoroughly conscious of how far short any attempts of mine may be in attempting to summarize the qualities and accomplishments of such an outstanding personality as that of William Gilson Farlow; but, having passed in review the main periods of his life, it seems best to undertake some general exposition of certain of numerous manifestations of his personality and his pursuits.

In stature, Farlow was decidedly below the average, a matter concerning which he was somewhat sensitive, especially when associated with one who was tall. He seldom, however, made reference to it except through some witticism. In referring, as he did on rare occasions, to his college days, he used to remark that at that time he was even smaller than at maturity. In the one room on the third floor of the Alexander Agassiz section of the Museum of Comparative Zoology, which served for cryptogamic laboratory and herbarium in my own first years at Cambridge—a lofty room piled high with materials—he was accustomed to ask me, the tallest of the workers, to get something from the top of one of the cases, with the usual after remark, "Now, please touch the ceiling." His own worktables and desks were made so low and his chairs so high that no one else could work at them comfortably.

His figure—erect when younger and slightly bowed in his latest years—passing from his house on Quincy Street up through Divinity Avenue to the museum with short, rapid steps,

always with books or manuscript under his arm, was distinctive and could easily be recognized as far as it could be seen. His downward, sidewise glance, seemingly furtive but really diffident when one came to realize its significance, was keen, and there was little that escaped it. The beginning of his conversation was often abrupt, but passed on into a monologue when discussing a problem or recent occurrences in the botanical world, which ended usually with some interrogation, often disconcerting as to whether an answer was demanded or not. Often some query on the part of others was greeted with a laugh or chuckle, which frequently placed his listeners more or less hors de combat and demanded further explanation or discussion. His ejaculations of surprise or incredulity were characteristic. Very commonly he would say: "Mercy! Bless my soul! I wonder where we are coming to when so-and-so puts forth such a view." Occasionally when he had some puzzling plant before him he would come over to our laboratory table and, laying down the specimen, say: "I will give any one of you 5 cents if you will tell me what this is." Many such a problem was placed before us, and we wrestled with it mightily, but seldom were we able to win the munificent reward, although at times we were given what we were much the more anxious to obtain, viz, his recognition of merit in our suggestions. This recognition was difficult of complete affirmation, since his critical mind interposed every possible objection, and the attainment of even partial approval was the result of a strenuous elimination of all that could not be sturdily and properly maintained. This method begot caution about accepting evidence unless of the most definite and pertinent variety. The alternative views he presented during such discussions, the keenness with which he detected flaws in the arguments presented, or the merciless fashion in which he carried some point raised to its logical and usually absurd or irrelevant conclusion, all these characterized the workings of his mind and made a profound and, if viewed properly, a most profitable impression on his associates. Many there were who misunderstood his extremely critical attitude, his witticisms, and his lack of acceptance of any pronounced opinion, even of his own, but those who came in daily contact with him soon learned to estimate them at their true worth and to welcome them as leading to the truth as nearly as it might be possible to approximate to it. To his students Farlow, while critical of their endeavor, was always sympathetic, even to those who least appreciated his efforts. Many a student received material aid, either directly or indirectly, and found him most embarrassed, seeming even cynical, when he attempted to express his appreciation. I remember the case of one assistant who married during vacation time and chose for his wife a young woman as poor as himself. Farlow was much excited and said to me: "Mercy! Bless my soul! What do you think has happened? I have just been informed that X has been married. He only receives \$500 for the next year and no prospect of any more for I don't know how many years. What are we coming to?" This was accompanied by a look which showed his concern and despair. X, however, seemed to manage and soon passed on from Harvard to a position yielding at least more than \$500, but Farlow gave no sign of having intervened. This case is typical.

In spite of differences of opinion as to the value of certain methods of work and the kind of results obtained, Farlow was always willing to look up points in literature and material for others and spent much of his time and energy in doing so, although often ill repaid in the final outcome. He was earnest rather than outwardly enthusiastic, but the attention he gave to details and larger points for those who desired to do good work was more significant than any amount of outward approval or compliment. He was a kind friend and counselor, although he seldom gave direct advice and his assistance, other than in direct line of his subject, was indirect and unobtrusive. As a host he was perfect, and at the gatherings at his rooms, or later in his own home, he knew how to draw out even the most diffident to join in the conversation and to feel at ease. He made the treasures of his library and his collections available, but always with circumspect reserve, to his students and visiting specialists, ever with due respect to their care and preservation. On the treasures of his mind, which were enormous, one could always draw and no one ever came away from a visit to him without added profit and comfort. His retentive memory and the breadth of his reading and acquisitive instinct made his knowledge encyclopedic in extent and his mastery of detail, without loss of coordination, was simply marvelous. At no

time did he show greater control of his mental balance and wisdom in meeting a particular situation than in the last weeks of his life, when, knowing as a physician that his end was certainly approaching, with calmness and deliberation he arranged his various and very considerable affairs and consulted with those who were to carry on his work and those for whom he desired to provide. During the last several years of his life, in fact, he had devoted himself to preparing for this end, which came peacefully to him, still in possession of his mental faculties. A word as to Farlow's health may not be amiss in this account. It may be said that, although never robust in the commonly accepted sense of the term, and although subject through much of his life to distressing and nerve-racking headaches, he lost little time from his work through illness and spent more than the ordinary working hours of the day in his pursuits. In later years he was less subject to these interruptions of his work and was amazingly cheerful as well as industrious.

As a field naturalist, Farlow was keen and untiring, although few of his later students had the opportunity of observing him in this capacity. As to his earlier trips and methods I know little except from casual remarks. He was wont, at times, to compare the condition of the neighborhood of the time with what it was earlier, when, judging from his reminiscences over some specimens, he lamented the intrusion of asphalt pavements and garbage heaps in select localities and called to mind that Rev. Prof. A. P. Peabody, then an elderly man and college pastor, could remember back to the time when *Arethusa bulbosa* grew in one corner of the college yard. Even in my own day (1887-1891) at Cambridge there remained some traces of good collecting places, such as "Norton's Woods," a small patch of woodland to the north of the museum, "Glacialis," near Fresh Pond, etc., but the tracks of progress were already blotting them out, although it was still possible to obtain a considerable number of both algæ and fungi from them. Our few excursions with Farlow, especially those to the seashore, opened our eyes to the possibilities of keen-eyed and experienced collecting. Every form of plant life had its point, or points, of interest, and we returned home from such a trip laden down with specimens and our minds stored with information concerning them. His first collecting was undoubtedly in the vicinity of Boston, Cambridge, and Newton. He early visited the seashore of the north coast of New England and the White Mountains of New Hampshire. These remained his principal collecting places, but in his early years of teaching he collected on the south shore of New England and proceeded on the north shore as far as Eastport. During his two years abroad he collected, probably extensively, in some favored localities. He mentions Switzerland particularly for the lichens and flowering plants. He was zealous also in his search for fungi, since he realized, as he intimated again and again in his writings, that little was to be obtained from American sources as to type or even reasonably authentic specimens of any kind, and an acquaintance with the traditions of mycology was one of the first points to be gained for future progress. His collections of marine algæ at Woods Hole and Gloucester, Mass., and at Eastport, Me., supplemented by his considerable collections at other places along the northeastern coast of the United States, were the foundation of his Marine Algæ of New England, and supplemented by his experiences along the Florida coast in 1875 and the California coast in 1885, both trips in company with Asa Gray, formed the personal experience basis of his broader work on the marine algæ of the United States. Farlow made trips to the Bermuda Islands in 1881 and 1900, collecting all sorts of cryptogams, but especially algæ, fungi, and lichens. He detected during these visits several species not noticed by any of the other botanists visiting the islands.

While Farlow's trips to Florida and to Mexico, California, and the Bermudas were general as to interest, yet marine algæ were the principal feature. His mycological collecting was largely done nearer home and almost exclusively in New England. Owing to his attraction and more or less of propulsion toward phytopathology, the parasitic fungi are more prominent in his published writings, yet it must be emphasized that he was a great collector and student of the fleshy fungi and that he left unpublished a considerable series of magnificent colored plates (already printed) of our American species. His studies on the Gymnosporangia or Cedar-Apples of the United States (misprinted "The Gymnosporangia or Cider-Apples of the United States" in first proof) is classic and was the forerunner of such monographic work on our fungi.

His other published work on the Rusts or Uredineæ shows his interests and insight into this difficult group of plant parasites. He went so far as to have prepared and even lithographed figures of the spores (telia) of the species of some of the more critical genera, but the text was never prepared. In regard to the perplexing synonymy, he used to remark that it was very likely that Adam may have named all the flowering plants, but that Eve must have named the Uredineæ. Eastern Massachusetts and New Hampshire, particularly the White Mountain region, were his field for fungi, as well as other cryptogams, nor did he pass unnoticed the flowering plants. His friendship with such inveterate collectors and students as the Faxon brothers, led him even into other New England territory. In later life his summers were usually spent in New Hampshire, either at Shelburne, where he found so many rarities, or, after his marriage, in his summer residence at Chocorua, overlooking the lake, where the field for fungi of all kinds was of the richest. He himself has told the very interesting story of how, while resting on a couch on the veranda of his place at Chocorua, he heard a pattering noise and, looking, saw a squirrel with some object in his mouth. A movement alarmed the squirrel, who dropped what it was carrying and fled. On examining the object, Farlow found it to be one of the hypogæous fungi which are so seldom collected and which, without this contribution from the friendly animal, he might never have seen. It brought also to his mind the larger suggestion of the dispersal agency we are now realizing so well in California, concerned in connection with hypogæous fungi in general. In his honor, one of the shoulders of Mount Chocorua, running from the peak along the ledges to the "Brook Trail," where he did much of the collecting of his last years, has been named Farlow Ridge. The last years of his life, Farlow spent much of his time putting the various specimens he had collected into condition, and since his death some of them have been sent out under the title of "Reliquiæ Farlowianæ." While realizing that the "closet-botanist" was a very important and helpful member of the profession, his various expressions as to fear of his being classed strictly in that ilk gave evidence of the importance he attached to field studies.

As a collector in the field, Farlow was very keen and successful, and his herbarium is full of results of his activity in this line. The influence of the great collections accumulated by Asa Gray, the foundation of the Gray Herbarium of to-day, rich in variety and in type material of the flowering plants and the vascular cryptogams, and poor, but not entirely lacking, in representatives, and very valuable ones, of the lower cryptogams, as well as the influence of Asa Gray himself, by example and by practice, led Farlow very early to the task of bringing together a similar authoritative and working collection of cryptogamous plants, particularly of lichens, algæ, and fungi. Farlow's earlier experiences in attempting to put into order and availability the cryptogamic portions of Gray's herbarium and to arrange and classify his own collections were augmented by his many and extremely valuable purchases and exchanges. The first considerable collection to be purchased was the fungus herbarium of Rev. M. A. Curtis, of Asheville, N. C. This was acquired for Farlow by Asa Gray while the former was studying in Europe. The Curtis collection is rich in specimens from Schweinitz, in those collected by the various exploring expeditions, and in duplicate specimens retained by Curtis from sendings abroad for identification and publication by such European authorities as Elias Fries, Berkeley, De Notaris, Desmazieres, Duby, and others. This collection was purchased in 1872. Through "friends" of Harvard University, there was purchased in 1898 the collections of Prof. Edward Tuckerman, of Amherst College, the founder of American lichenology. These collections were rich in types and other authentic specimens of Tuckerman and all the lichenologists of his day. The Tuckerman collections contain most of the older and rarer lichen *Exsiccati* as well as the unrivaled series of North American specimens collected by the founder and his correspondents. It has also a representation of the lichens of the various exploring expeditions undertaken by the United States. To these collections of fungi and lichens, Farlow added enormously through his own collecting and by those received through his pupils and correspondents. The marine algæ are due to his own efforts and those of his correspondents, the only collection of any size acquired by purchase being the small De Alton Saunders collection. In the collection of marine algæ, however, are specimens from every then living phycologist of note as well as from those who preceded them. I am not in possession of any exact

numerical estimate of these various collections which Farlow brought together, but figures give only a very inadequate idea of the value of the assembled material. In 1896, however, Farlow made the statement that the cryptogamic collections (in largest degree due to his own efforts) must number several hundred thousand.

Farlow early appreciated the value of published sets of specimens ("Exsiccati" or "Exsiccatae") and diligently sought out such as might be purchased. His success in this direction was most extraordinary, so that in his *Sketch of Cryptogamic Botany* (and kept together as sets), states that between 1872 and 1896 there were brought together (and kept together as sets), not including those complete or partial sets whose numbers were scattered through the general herbarium, 75 different series, including 64,000 specimens representing about 23,000 distinct species. From 1896 to 1919 he continued to add to this series, both of older and of current issues. It is to be remembered that each of these specimens is a datum of reference, and it is doubtful whether any such considerable collection of fundamental specimens exists anywhere else. In connection with this unique collection of published specimens of the lower cryptogams, it seems very desirable to note Farlow's attitude toward their preservation and arrangement. He kept each series of specimens with their printed labels, title pages of the fascicles, etc., together and intact, while the more usual method is to separate them from one another and distribute them through the general collection. By the latter method, the relation of the series, date of issue, etc., is lost. The specimens cease to be integral parts of a "published" series and are often difficult of location in the general collection because of shifting views as to synonymy, etc. Under Farlow's method the specimen, usually quoted by number, is readily located and all data as to details of publication may be readily ascertained. To facilitate access, Farlow indexed all these specimens and even made the proper cross references, so that the existing status of a specimen might readily be ascertained, or all published specimens relating to a certain species might readily be found and comparison made. Farlow left these collections, both general and published, to Harvard University under certain conditions. It is to the credit of those concerned in carrying out the trust that the conditions have been fulfilled and that the Farlow Herbarium is now lodged in a fireproof building, arranged and cared for as a basal unit, for the benefit of cryptogamic botanists of the present generation as well as of those to come.

Under the present disposition of the Farlow Herbarium, the Farlow Library is housed in the same building and in convenient juxtaposition to the specimens. During his lifetime, Farlow used to lament the impossibility, in his estimation, of bringing the two together, the herbarium having been located in the museum building, while the library, in the later years of his life, occupied a fireproofed addition to his own residence. The necessary books had to be carried back and forth between the two locations or else consulted separately. As in the case of the series of published specimens, Farlow sought out and purchased rare publications relating to his specialties, bought current periodicals and books, acquired separates, and all that were of interest or importance. His eye was keen over book catalogues, and his library was as complete as an expert with means at his disposal could make it. Farlow was extremely careful of his books and rarely could be induced to loan one, and consultation was chiefly in his study and under his own eye. In this way he kept his collection intact and uninjured. He was an omnivorous reader through the whole field of botany, keeping track, largely through the original articles, of progress in special fields—others as well as his own. His memory was exceedingly retentive, and he provided a fund of information to his students, his botanical visitors, and his correspondents.

Associated with the work of accumulating two such fundamental adjuncts to accurate work on the lower cryptogams as a satisfactory herbarium and an adequate library, came the matter of making both and the results of coordinated labor in the two available. That was accomplished by a series of indices. References and cross references were made, both in connection with the literature and the published specimens. The species were carefully attended to and their host plants (or animals) in case of the parasitic species. The synonymy, not only from the published data but from critical research, was carefully worked out. The result was not only indices to facilitate the work of Farlow himself, his students, and his correspondents, but for publication.

Several of these did come to the point of publication, such as a list, followed by a supplementary list, of works on North American fungi (1887 and 1888), a host index of the fungi of the United States (1888 and 1891), and finally the first part of the magnum opus, the Bibliographical Index of North American Fungi (1905), which included the fungi only as far as Badhamia, the rest remaining still in card form (approximating 350,000 references) awaiting funds to make it available to the many to whom it would be of the greatest benefit. These indices have been of inestimable influence in the work on North American fungi, both as to those published and those unpublished. Information and criticism founded on the data contained in them has been freely given, especially in correspondence, and has tended to keep down errors, unnecessary publication, and constructively to keep accuracy up to a high level. One of the greatest boons to our current work on fungi would be conferred by the publication of this last great index and adequate provision for its continuance from Farlow's farseeing and most admirable inception.

While the number of titles of the writings of William Gilson Farlow is ample, while the variety of topics he touched is very large, and while the new facts and considerations brought forward by him are very considerable, yet his critical knowledge of the various groups upon which he worked was so enormous and so detailed that we turn from what he has left us to that which we feel that he had to give with a sense of most serious loss. His very early publication on the apogamy in certain ferns was clearly a student publication, a happening in a laboratory where its importance was realized by an able instructor of wide experience. Farlow's main interest, however, did not lie in that direction and he did not follow up that lead, although he retained a deep interest in apogamy and related phenomena, as I well remember from experiences somewhat over a quarter of a century later while a student in his laboratory and in connection with pteridophyte apospory. His earliest papers concerned themselves with the marine algæ, taxonomic and critical, and these led up to what many of us, and it seems to me justly, consider his most characteristic and outstanding publication: viz. his account, really manual, of the marine algæ of New England and adjacent coasts. In arrangement, in content, and especially in critical and explanatory remark, this small volume is a model, refreshing, instructive, and intriguing to personal effort on the part of reader or student. Farlow's matchless humor and keen characterization show themselves again and again. For example, speaking of the common *Leathesia*, he notes that it is "sometimes called potatoes by the unromantic dwellers on the shore," or again, in speaking of a nomen nudum, *Callithamnion Tocwottoniense* of Olney's list, which he says: "fortunately for printers and the throats of American algologists has never been described." It was one of Farlow's sincere desires that a new manual of New England algæ be prepared and issued, and the task fittingly devolved on Frank Shipley Collins, who had accomplished so much in that direction, but he, too, passed away without having completed the task.

Through his connection with the Bussey Institution and the turning of his attention from his favorites, the marine algæ, to what later came to be called plant pathology, or phytopathology, Farlow gave us the results of his work on certain species and groups of parasitic fungi. The potato rot and the grapevine mildew in particular led him to the Peronosporaceæ and their relatives, and his papers on these organisms were for long years authoritative. Onion smut, the black knot of cherry, and many miscellaneous plant diseases caused him to write other illuminating papers, but his chief attraction along these lines seemed to be the group of rusts, or Uredineæ, as they were called for so long a period. His pioneer paper on the Gymnosporangia led to a series of investigations, first, in the way of cultural studies by Thaxter and, later, by others, to determine their exact heteroecism. His critical notes on that troublesome question, synonymy, particularly vexatious in the group of the Uredineæ, and his notes on some species in the third and in the eleventh centuries of Ellis's North American Fungi (1883) are among his important contributions. He likewise elucidated and arranged the *Synchytrium* species of the United States. All these—*Synchytria*, Peronosporaceæ, Ustilagineæ, and Uredineæ—parasitic groups of fungi and of both biologic and economic interest, he touched but to adorn, and we feel bereft that out of his encyclopedic knowledge of these groups he did not find the opportunity to yield still more than he did in permanent form. We

feel that we might have expected, and with all propriety, one or even several monumental works such as the Bibliographical Index to North American Fungi, already alluded to, some monographs and revisions, but it was not to be. His index work, published and unpublished, numerous lists and occasional notes such as most of his later publications consisted of, are most grateful, but aggravating, as promise unfulfilled. His knowledge of the fleshy fungi was second only to that of his on the parasitic fungi, but we possess little of it. Even his collection of wonderful printed plates was not brought to publication. He is perhaps to be envied in that he leaves us in the position of Oliver Twist, asking hungrily for more with never an approach toward satiety.

Farlow's attitude toward general questions of a botanical or biological nature was largely expressed in conversation or in his public addresses, some of which have, fortunately for us, been printed. In conversation and formal address, he showed keenness of vision as well as great modesty, which he was inclined to cloak under pessimistic or sarcastic utterance. His classic statements, humorous or sarcastic, were generally the opening statements or used at times in the body of the address to suggest a "reductio ad absurdum." There has already been quoted in another account of Farlow an extract from his address before the American Association in 1905. In introducing his subject, which was entitled "The popular conception of the scientific man at the present day," Doctor Farlow says:

What is or is not progress, depends, of course, upon the point of view. Some are so far ahead of the majority that they cannot see how much progress is made by those behind them. Others are so far in the rear that they cannot distinguish what is going on ahead of them. We must also admit that there are different directions in which progress can be made. You have all seen the agile crab, and been surprised to find how rapidly he gets over the ground, although he never seems to go ahead, but to scramble off sidewise. The crab perhaps wonders why men are so stupid as to try to move straight forward. It is a popular belief, but, not being a zoölogist, I cannot vouch for its correctness, that the squid progresses backward, discharging a large amount of ink. One might perhaps ask: Is the progress of science sometimes like that of the crab, rapid, but not straight-forward; or, like the squid, may not the emission of a large amount of printer's ink really conceal a backward movement?

On another occasion, but at dinner and consequently informal, Farlow alluded to conventions and meetings and their purpose by relating the difficulties Mrs. Farlow experienced in obtaining eggs of the proper quality. On consulting with various dealers she was instructed and had trials of various grades, from "fresh," through "strictly fresh" to "newly laid" eggs. On inquiring as to how one could tell when eggs were newly laid she was informed, "by the cackle." Farlow then said, "How are we going to tell the newly laid discoveries at our meetings? The answer is, 'by the cackle.'" His attitude toward most of the newly announced discoveries was, as was natural to him in all things, skeptical. His address expresses this over and over again. On one occasion (Amer. Soc. Naturalists, 1886), he said:

Probably a good many of my hearers have heard the remark, "I suppose you must make considerable out of your scientific papers." Unfortunately, with the exception of text-books of a lower grade, one is only too glad not to be money out of pocket. I fear that you all can bear witness that, with rare exceptions, your published papers have never paid for themselves. It is only after the results of research have reached a homoeopathic dilution in some text-book or popular article that they begin to pay. Of such dilutions we already have an abundance, and the more important point is to get something new which will bear dilution. Unfortunately the public do not clearly see the difference between the original work and the dilution. The former does not pay, and needs encouragement; the latter is a commercial article having a recognized money value.

A characterization such as this is certainly definite and not by any means "out of order." It is matched by the graceful closing of the same address:

But you will probably think that this paper is not like a ball of twine, which, however much it may be twisted and snarled, really has an end. There is much more I should like to say on the subject; as it is, I have tried to avoid particular specifications as to subjects of research, which would be interesting only to botanists, but to state broadly some of the difficulties in the way of botanical research, and to indicate the path which promises to be most favorable in the future. If my life proves to be as long as your patience, there will be plenty of opportunities hereafter to consider some points which I have been unable to touch upon today.

Aside from his witticisms, as such, and often in connection with them, Farlow presented his general ideas in the same clean-cut and pointed fashion in which his detailed work was done.

His clear outline of "The task of American botanists" in 1886, and his analysis of "Biological teaching in colleges," in the same year, and his humorous but searching characterization of "The popular conception of the scientific man at the present day" convey no less direct and profitable food for thought than his masterly and detailed treatment of "The conception of species as affected by recent investigations on fungi." It is from these published addresses that one may obtain some vivid and truthful ideas concerning the nature and work of the man who wrote them. If one may add, as many still living are able to, impressions from personal contact, informal conversations and talks at small dinners or in company, one may discount certain impressions of cynicism, pessimism, and sarcasm, and realize the kindness yet clear vision of him, whom all those of us who did know him will love and revere.

As a conversationalist, Farlow was recognized as more than usually endowed with ready wit and repartee. The witticisms which characterize his public addresses were even more abundant and more pointed at times when the occasion called for them. To the bumptious or overgrateful person alike, his shafts struck directly and the conceited received short shrift at his hands. Yet he was ever gentle with the sensitive and, although really embarrassed, had extreme sympathy and desire to assist in the case of misfortune on the part of the truly deserving. He gave of his deep wells of information at times of friendly intercourse. Well do I remember being informally inducted into the history of the development of our knowledge of cryptogamic botany. This happened on the occasion of my more or less formal evening calls upon him in his rooms, then in Holyoke House. After a short call which I presumed would be agreeable to him and I rose to go, he would detain me, with my hand on the door knob, for an hour or more while he discoursed, almost in a monologue, on the personality, ancestry, botanical pedigree, and accomplishments of some distinguished botanist or botanists who had come up in our work. There was much of the unwritten history in these informal talks and food for thought as well as stimulus to further reading after I had finally been allowed to say my last adieu and depart, full of increased knowledge. At his dining club and elsewhere it was more or less a practice to bait Farlow, as it were, to bring out his ready and often biting repartee. It was a contest of some of the best wits of Harvard University, and Farlow is said usually to have borne away chief honors.

Farlow's letters were by no means the least of his influences exerted on behalf of what was best in cryptogamic work in the United States and even abroad. His correspondents seem to have been limited to those interested in any phase of cryptogamic botany. He was in constant interchange of views, literature, and specimens with practically all of the foreign cryptogamic botanists, while those at home had mostly been students with him or later in the cryptogamic laboratories at Harvard University. All difficulties, and particularly puzzles, were submitted to him, and while, at times, somewhat slow to answer, he generally replied briefly but to the point, giving much of his valuable time to this work, solely for the sake of assisting his friends or, possibly at times, to confound those of whose methods and work he could not approve. He must have written many thousand letters, with few exceptions in his own scrawly hand, and of which he, himself, was the severest critic. He did not accustom himself to a secretary or to a typewriter. In his experience were many extraordinary requests and he himself speaks feelingly (1887) of "the impecunious ignoramus who informs you that he is going to write a book, to include all the fungi of this continent, and coolly asks you to give or lend him all your books and specimens and tell him how to begin." While something definite is likely to have happened to this particular type of person, yet I have no doubt that if there were a grain of reasonableness to be discerned in such a character, Farlow would have recognized it and not have withheld such aid as he might be able to render. We have all fed upon the crumbs which dropped so plentifully from his well-filled larder and yet find ourselves unable to express our indebtedness and gratitude except in a few colorless words.

As a critic, Farlow was thorough and at times severe, but not intruding his criticism otherwise than called for by his duty to one of his students, nor unasked for. In his many reviews of particular papers or outlines of progress he was manifestly fair. He did not assume the rôle of mentor as Gray did occasionally in his later years. There is one review of Gray's, a rebuke of

some recent work in cryptogamic botany, in which the voice is the voice of Gray but the hand seems most likely to have been that of Farlow. He always warned us who were composing our youthful papers under his direction against too strong statements about any writer or his works. "Do not say," he often remarked, "that he is wrong or make use of any such direct expression, no matter what you think; simply quote him or his work with great respect and then show that he is thoroughly mistaken." This is a practice he always carried out in his own writings.

Farlow's influence as a builder up of unrivaled facilities for work in cryptogamic botany was supplemented by his influence on the teaching of this subject as well as on the teaching of botany in general. Few of us think of him as the founder of a pedagogical system and perhaps it is not possible to advance that claim; nevertheless, his methods and his viewpoints were so distinct, so analytic, and based so firmly on the psychologic aspects of both teacher and taught that he at least emphasized in botanical pedagogy a distinct and practically novel method. Farlow has given some of his ideas in his *Sketch of Cryptogamic Botany at Harvard University from 1874 to 1896*, and those who have access to a copy of this very interesting and instructive publication will do well to consider most carefully what he says. I suspect, however, that this privately printed document is not readily at hand to many, and I excuse myself for having quoted or abstracted many details from it. His other pedagogical disquisition, *Biological Teaching in Colleges* (1886), is readily accessible and much of his own attitude toward methodology is contained in it, with touches of his own personality which render it most illuminating.

It was my own good fortune to be associated with him as assistant (1888-1891) in the first part of what was called natural history 5 (botanical instruction being given the first half and zoological the second) and that, too, at a time when his ideas were fully developed. The first four plants used by him in this course were the distinctive features, since the training toward developing power in observing, recording, and inferring the structure and activity was the point laid stress upon. We began with a yeast cake, rock candy, and water. The rock candy was dissolved in water in a tall but slender glass cylinder and the yeast cake was then pulverized and added. The jar (or several of them) was placed in a warm place, usually on the window sill, where the student could look through it, and this was done several hours, or days even before it was to be used by the class, so that each cylinder might be evidencing proper activity. The details of preparation were announced to the class and they were asked to record in notes and drawings what they saw. The results, of course, are obvious, and the answers varied. The students were led by questions to distinguish their observations from their inferences. The plain facts of the rock candy being sugar and of the fermentation which most of them *saw* being an inference, as well as how they might, or might not, be able to demonstrate the truth of one or another inference, was brought out through questioning, objecting, and suggesting. Then the students were directed to make examinations with the compound microscope, using low power and then high powers, and to test with iodine and follow that with sulphuric acid of proper strength. Having listened to lectures on the cell and having heard that the "yeast plant" was concerned, all the students found cells, although usually their first finds were either air bubbles or starch grains. Many desired to know what they were to look for and seemed disappointed or even helpless when advised to determine, draw, and describe as many kinds of things as they might be able to distinguish in their preparations. After drawings were made, the students wanted names, but Farlow always suggested that they study each kind of object under each power of the microscope and under the influence of each reagent before coming to a conclusion. When the yeast cells were finally distinguished from the air bubbles, starch grains, and bacteria associated with them, they frequently proceeded to endow them with nuclei and even at times with chlorophyll. By the time the yeast exercise was completed most of the students had come to realize the manner of procedure and to distinguish "what they could see" from "what was purely a matter of inference."

After yeast came Spirogyra, the same care being exercised to emphasize method; and besides iodine and sulphuric acid, glycerin was applied to untreated filaments and also strong

alcohol. The students thus became acquainted with a vegetable cell, its wall, chromatophores, pyrenoids, and starch inclusions, the nucleus suspended in the center of the vacuole, and the primordial utricle, being induced to reason out each part and its structure by the "Yankee" method of answering one question by asking another. As a final test, each student was required to draw a diagram of a median longitudinal section of the Spirogyra cell. Thus the student was induced to infer the details of an object with three principal dimensions and portray it graphically. Spirogyra was followed by Nitella to show cyclosis, and a diagram of a median longitudinal section was also required to represent relation of layers from cell wall to center of a joint or tip cell. The final test of power to interpret solids came with the study of pine wood. First a transverse section was cut and mounted in balsam, so as to be properly cleared. This section was contrived so as to cover several annual rings. A careful drawing of this section was required, and the student was asked concerning his idea of the shape of the cells in pine wood, the answer usually being "square." He was also led to realize that there were several varieties of cells in the section and, by comparison with the microscopic view of the piece of wood whence the section had been cut, as to the direction of the center of the original tree, and consequently to distinguish spring wood, autumnal wood, and medullary rays. Most students were brought to the point of acknowledging that the only way to be certain about the shape of the cells would be to cut a longitudinal section. Over this would ensue a discussion as to what direction the longitudinal section must be cut, whether in any longitudinal direction relative to rings or rays or parallel to one or the other. The discussion in this connection, aided by suggestions as to consequences, led to the cutting of radial and tangential sections. About this time the student was frankly and thoroughly puzzled and at his wit's end as to how to match up three such different looking sections as those cut transversely, radially, and tangentially through coniferous wood. By directing attention through questions as to direction of center, occurrence, etc., the identification of the various kinds of cells and discoid markings was accomplished in all three sections. The final exercise, that of drawing in isometric projection the corner of a block of pine wood and matching the cell outlines, finally and emphatically completed the training in solid geometry and at least induced caution as to answering questions without careful consideration. After these several preliminary exercises the course proceeded to various selected plant types, from the simpler to the more complex, and the benefit of the preliminary training became apparent. The attack directed toward each problem was more straightforward, the reasoning more cautious and based on more actual observations, and the inferences drawn more logical.

Natural history 5, especially as to the first half, became nationally famous and one heard of it in various places and with differing comment. It was said that the instructor gave his students a razor, a microscope, and a broom handle and insisted upon a complete report. Many were the wild surmises and improbable hypotheses presented by the students, some received by Farlow with his inimitable chuckle, but all treated with respect and seriously argued. The instructor had need of ready wit and resource. The son of a distinguished member of Harvard University, after having ruined his best razor, told me in all solemnity that he considered that form of implement a very poor tool for cutting pine wood. The attitude of Farlow toward his students, especially beginners, but applying to all, was much more psychologically pedagogic than was usual in his time. It was something of the point of view of Louis Agassiz, but was more directive than his, so far as I may learn. He often said that if he were to live his life over again he would be a psychologist like "Willie" James because then he would not be compelled to bother to collect specimens everywhere and could dismiss them when through with studying them. His classification of students given in his naturalist address of 1886 is typical: Two classes, one of which was composed of individuals who wanted to be told what to see, and the other of those who knew so much (?) that they began to lecture on what they thought the specimen ought to show and who were led into extraordinary errors through their superficial training. The latter is the kind of student who, to use Farlow's own words, "called a hole in a cell wall a bioplast," and was highly pleased with his achievement until he was asked what a bioplast was. "The suggestion that a hole might without any great violence

to the English language be called a hole, was timely, if not pleasing." In quite another vein and yet to the same point, he said (The Task of American Botanists): "It is well to have our standard high but it should not be unattainable." "We may well set before our young men such models as De Bary, Sachs, Strasburger, and others; but it is just possible that a young man who is determined to be a De Bary, a Sachs, a Strasburger, or nothing, may have to adopt the latter alternative." "The trouble is, too many young men assume that the work they are destined to do is of the highest grade and they expect to be provided with all the refined apparatus and complete equipment which the leaders abroad possess." "They will not begin the simplest thing without an array of reagents which would be the envy of a good many chemists and the number of staining fluids which they must have around them would make the rainbow blush at its own poverty." "One young man thinks that he can not do any work because he has not a Jung microtome, another has been unable to do anything during a vacation at the seashore because he had no osmic acid. The botanist who declares that he can not do physiological work because he has not a large amount of apparatus would do well to recall the case of a Mr. Charles Darwin who published something on the power of movement in plants." His whole philosophy as to development of power rather than sponge capacity may be considered as being summed up in the sentence: "You can not make a boy a good mountain climber by carrying him up the Mount Washington Railway, no matter at how rapid a rate; and, in ordinary life, there are many mountains to be climbed, up which there is no railway."

As a lecturer, Farlow had a manner of his own. Incisive, yet coherent, with emphasis and yet not neglecting minor matters, glancing sideways to discover the effect being made, biting the ends of his mustache when he paused to allow the effect of a rhetorical question to sink in. He usually began: "The subject of my lecture to-day is—by the way, are there any questions about the last lecture"; and when there were none, continuing, "I am pleased to see that you understood it so well." He was accustomed to emphasize his points by touching the desk in front of him with the outstretched forefinger of his right hand. He was more than successful in extracting the meat from a topic and laying it plainly before his hearers. He had a horror of extraneous details, although he said they often help. His classical illustration was of ergot. "Ergot," he told his class, "is a very interesting fungus. By the way, it grows about here in the flowers of the wild rye on the banks of the Charles River," going on to describe its characters, etc. On examination, asking about ergot, he received the reply: "Ergot is a plant growing on the banks of Charles River."

With advanced students and those studying for higher degrees his methods were, of course, different, but he always used the question method, answer and rebuttal following. He could ask the most searching questions, taking the wind completely out of the sails of the overconfident and reducing superficial conclusions from a turgid condition to that of complete collapse. He never assumed an authoritative tone himself, but always expressed a conclusion tentatively and often interrogatively, unless it were negative, in which case he was often most decisive. I remember well his statement as to the claims of a botanist who had distributed a number of sterile specimens of a critical genus of the green algæ, claiming, when remonstrated with, the ability to determine such specimens, whether other botanists could or not. "One may not be able to say definitely whether such sterile specimens are undoubtedly of a certain species," said he, "but one can say what they are not, and the specimens distributed certainly do not belong to the species whose names are on these labels." In the first work of research I attempted with Farlow it was necessary to compare the structure of an alga (*Tuomeya*) with which I was at work with that of the type specimen. As Farlow possessed only a wee fragment of the type, I could take only one slice from it, and I was compelled to make a section of my material which corresponded exactly with that slice before he would allow satisfactory identity. I finally succeeded, but it cost me nearly a week's time to obtain that identical section. Farlow could find more flaws and raise more objections than any other instructor with whom I ever came into contact, but when he finally did approve there was the satisfaction that little further destructive criticism could be directed against it. On this account, the writ-

ing of a paper under Farlow's supervision was an experience long to be remembered, but also an experience worth while. Every sentence was discussed, both as to the truth of the statement and the way in which this truth might be conveyed.

Farlow's influence on the teaching and research of botany is by no means confined to the cryptogamic side, although most of his activity belongs there. His example, in its manifold excellence, penetrated to many fields not peculiarly his own. By the time of his death he had become the Nestor of American botanists, and his appearance at the annual meetings was always hoped for and thoroughly appreciated when he could attend. His words of wisdom, his witty remarks, his rare addresses, and his after-dinner speeches were events. In Cambridge he received and entertained visiting botanists so that his home became a veritable Mecca to those seeking counsel and consolation. He was welcomed into all American societies to which he was eligible. He was elected a member of the National Academy of Sciences in 1879. He was elected president of the American Association for the Advancement of Science and of the Botanical Society of America. He was a corresponding member of various societies and associations of England, France, Germany, and Italy; in fact the list of his honors in this direction is long and varied, even for a distinguished member of Harvard University.

Besides the degrees of B. A., M. A., and M. D., in course, Harvard University conferred the degree of LL. D. in 1896. The University of Glasgow in 1901 and that of Wisconsin in 1904 conferred upon Farlow the same degree and the University of Upsala that of Ph. D. in 1907, on the two hundredth anniversary of the birthday of Linnæus. Many species were named in his honor and at least two genera. He died full of honors, revered and respected by his colleagues and sincerely mourned by his former students and his friends. I may be allowed, in closing, to quote the final paragraph entered on the minutes of the faculty of arts and sciences of Harvard University, on December 2, 1919, as a fitting epitaph:

A pioneer, a cultivated and learned man of wide influence, a stimulating teacher and keen investigator, a loyal friend, Dr. Farlow was original, versatile, conscientious, modest, sympathetic, and generous; with him has passed from the Harvard group of scholars a unique personality.

I desire to make grateful acknowledgment to Mrs. William G. Farlow, Prof. Roland Thaxter, and Mr. A. P. D. Piquet for assistance and suggestion. I have obtained material and inspiration from the following biographical notices and resolutions:

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