

FERNALD AS A REVISER OF GRAY'S MANUAL

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BEFORE entering college I wrote to Professor Fernald from Indianapolis asking if a Freshman who was already keenly interested in botany would be allowed to take his course "Botany 7: The Flora of New England and the Maritime Provinces of Canada." The answer was "Yes," for those were the good old days of President Eliot and free election, when any student could enroll in any course if he could persuade the professor to admit him. There never was an educational policy better adapted to professors and students who liked to do as they pleased! After poring over all the college catalogues available, I had decided that nowhere else was there a course like Harvard's "Botany 7." How amazingly true this proved to be I soon discovered, for Fernald was as extreme an individualist in teaching as he was in his scientific work. He worked in such flares of enthusiasm that whatever engaged him at a particular moment was for the time being the most important thing in the world to him, and so it had to be to his students. There were only two of us that year (1904-1905) and we were guinea pigs on whom his ideas were tested out. As for background, he seemed to assume that if we didn't already know the minute distinctions among all the species he talked about with such glowing enthusiasm we soon would, and since he referred to *Carex* so frequently, his monograph "The Carices of the Section Hyparrhenae" was soon fixed upon as an exemplar for method and systematic concepts. These particular sedges, it would seem, must illustrate all truly important botanical phenomena and types of geographical distribution in North Eastern America, and it would be a long time before any other region need concern us! Fernald took me to an old wooden case where the reserve numbers of the "Contributions from the Gray Herbarium" were kept, got out a copy of *the most important one*, that on the *Hyparrhenae*, inscribed it to me, told me that he had also written a big one on the genus *Salvia* in Mexico, but that it was really far inferior, because he had never seen the plants in the field, and, anyway I shouldn't be concerned with Mexico. A firm teacher-student friendship was at once established between Fernald and myself, for he became

and remained my favorite professor. He was inclined to think well of people from Maine, other things being equal, and since both his mother and my father had been born in Bethel, Maine, I was only a generation removed, which made a certain bond that nobody but a down-East Yankee might recognize!

Many who did not know Fernald well too hastily concluded that his predominant traits were vanity and acrimoniousness. This was very far from the truth. He was easily moved to intemperate expression of emotions which others with more control might conceal, but he was essentially friendly and helpful. His vast excitement over what sometimes seemed of small significance was what kept him so amazingly active and productive. In the days when I knew him best he was never assailed by doubts about the value of what he was then doing, but he was very critical of those who were doing something that he would not spend his own time doing. I remember that one time he remarked on what a disappointment Thiselton-Dyer's career had been, as Director of Kew. He said that Thiselton-Dyer had had as great a chance for a productive career as Joseph Dalton Hooker, but had frittered away his time, although his obvious duty to botany was not to waste a moment of an opportunity denied to most botanists, when he was the one, chosen from among hundreds, who had a chance to do great things. At this outburst Dr. Robinson, who was the essence of kindness and moderation, and seldom allowed himself to pass a snap judgement, was genuinely shocked, and made what was, for him, a vigorous rebuttal. The clash of personality between the two men was so great that they never seemed sufficiently compatible to work together harmoniously, and actually their co-operation in the revision of Gray's Manual for the seventh edition was negligible, consisting merely in each doing part of the work. There was no community of concepts.

Their social life was utterly different. Robinson would typically invite his friends to meet some distinguished musician at his home and meticulously observed all the social amenities. Fernald would propose an all-day Sunday tramp in midwinter, starting from some point reached by rail. Then, as he said, the swamps and bogs were all frozen over and you could see just what they were like. After tramping all day in the cold with

nothing to eat except maybe frozen cranberries from a bog, boiled in melted snow in an old tomato can salvaged from a roadside dump, he would take his guest home to a midnight repast of lamb chops only, broiled on forks over the coals in the furnace down cellar, and eaten out of hand, squatting on the floor in front of the open furnace door. On such an occasion Fernald was at his best, jolly, full of zest and good-fellowship, and infectiously enthusiastic about life in general and the New England flora in particular.

After I got acquainted at the Herbarium, it was not long before great piles of pasted-up manuscript made their appearance for the forthcoming (7th) edition of Gray's Manual, which was not actually published until 1908 but had already been long in preparation. The basic copy had been prepared by pasting clippings from the older edition, family by family, onto sheets and arranging them in the Engler and Prantl sequence. Changes had been made in almost every line at various times, so that the revised copy resembled especially foul corrected proof. Some parts were Robinson's especial responsibility and were mostly revised by him, and others were Fernald's, but either of them fixed whatever errors or omissions came to his attention.

Robinson was the more systematic worker, for he was inclined to work straight along, in the quiet of the old Gray study, dealing with the pages as they came. Not so, Fernald. He would get started on some particular species or group by finding something of interest in the course of current routine determinative work. It would lead him into a hectic investigation that sometimes fell flat but generally resulted in a big or little article for RHODORA, and the random articles provided the basis for revision of the Manual copy. So practically everything he did after about 1901 was directly contributory to the Manual, but some groups received minimal attention. He needed the stimulus of some discovery to set him off. It did not have to be a large one. Sometimes, in fact, the supposed discovery petered out, but it would have resulted in some critical determinations that helped the good cause along. So there was continual progress with the Manual but it never seemed to get done.

Even "Botany 7" had to do some small part. Our laboratory work consisted largely in trying to prepare tentative keys to

genera that had been skipped, or in testing out revised keys with current herbarium accessions, or in testing the applicability of work published after the "Manual" copy had been prepared, which might necessitate still further changes. To what extent in later years "Botany 7" continued to be a device for preliminary testing of the "Manual" revision I do not know, but Fernald was not one who readily changed his ways and it is to be presumed that the preparation of the eighth edition followed much the same course as that of the seventh.

In Fernald's early years as a staff member there were four chief continuing institutional projects at the Gray Herbarium. In addition to (1) the revision of Gray's Manual, these were (2) the continuation of the Synoptical Flora of North America, (3) the study, in accordance with an agreement of co-operation with the National Herbarium, of the numerous new collections that came to hand yearly from Mexico and Central America, and (4) the indexing of newly described systematic entities of the Western Hemisphere.

As already indicated the first of these was originally shared by Robinson and Fernald, but fell eventually to the latter; the second was Dr. Robinson's; the third was divided among Robinson, Greenman and Fernald; and the fourth had come to be exclusively Miss Day's.

Fernald's inheritance of the Manual revision came about gradually. Participation in the study of the Mexican and other tropical collections became increasingly distasteful to him, for he could not keep up an interest in floras unless he personally knew many of the species in the field, when he had not actually worked in the regions. Any species that grew in northeastern America interested him wherever it occurred, or was supposed to occur, and he would therefore spend much time studying Scandinavian specimens and publications. A region whose flora was largely dissimilar to that of the Northeast had no attraction for him.

To accord with his interest in the plants of eastern Canada the limits of the Manual region were extended northward, and for a good many seasons he worked with a succession of botanical comrades in Quebec and Newfoundland. At length, having stimulated the interest of Canadians in taking over the study of the northern border, and wishing to do his own field work where

there were the best chances for significant discoveries, he turned to the coastal plain of Virginia, where he made a multitude of interesting additions to the flora of the "Manual region." At the close of each field season he returned to the Herbarium with keenly whetted enthusiasm for studying the new collections, and the idea of doing anything unrelated to that seemed almost intolerable to him.

It was part of the routine of the Herbarium to identify the tropical collections as such. Then, at length, after isolated species had been described in a genus, and sufficient material seemed to have been accumulated, an effort would be made by a staff member or student to prepare a comprehensive revision. The annual collections of Pringle and Palmer were the chief dependence for progress in the somewhat vaguely defined tropical American project, but there came to be more and more field workers, such as Millspaugh and Gaumer, in Yucatan, C. C. Deam, in Mexico and Guatemala, Peck in British Honduras, John Donnell-Smith and his associates in Central America, Rose and associates, mostly in Mexico, Lumholtz in Mexico, and not a few others. The effort to make some current systematic disposal of all this material often required that species be described not by systematic revision of a mass of material but from single specimens, the distinctions of which might or might not hold up in the light of subsequent collecting.

This work on miscellaneous tropical plants engaged much of Fernald's attention until about 1901 when he practically declared his independence of it. This restriction and unification of his interests were clearly in the best interests of the Gray Herbarium. The New England Botanical Club contained many of the best friends of the Herbarium, who did whatever they could, financially and otherwise, to support it. Financial support was never sufficient and although the Gray Herbarium was one of the most eminent and deserving of Harvard departments, it led a somewhat hand-to-mouth existence and had to beg of its friends in order to carry on any kind of a worthy program. So it was essential not to fail to serve the local constituency of those who were primarily concerned with the local flora. In botany Harvard's policy was then, as it still remains, to belittle its own best achievements and to disregard its most valuable resources

and traditions in building for a glorious new future. So the Herbarium, then and subsequently, had the problem of finding in large part its own sustenance, with little aid or encouragement from the top echelon.

In providing for successive editions of Gray's Manual, Harvard, however grudgingly, has performed an important national educational and scientific service. Fernald was an inspiring leader of local flora investigators and during four decades botanists looked forward to the appearance of the new "Gray" as an event of genuine importance, as it was. One of Fernald's botanical colleagues of many years standing, Professor Bradley Moore Davis, has well expressed what many of us feel about Fernald's constant devotion for over forty-five years to revision of the Manual. He wrote in a recent letter: "Fernald's death brought to close a well ordered life that followed a consistent pattern, to the end that he accomplished much."

In its early years the New England Botanical Club was an enthusiastic organization, largely of amateur systematists and of professional botanists whose interest was not chiefly systematic, but whose attention to various local floras or incidental collecting turned up many problems that could best be referred to the staff of the Herbarium.

Fernald was ready and willing to act as a central consultant for this large group of botanists, which soon extended far beyond the membership of the club. By the time the eighth edition of the Manual was completed, he had had cooperation to some degree from about 400 collaborators, whose problems and questions all had to be reasonably answered by his investigations. Probably no botanical systematist had ever before gone so far in satisfying so many active finders of deficiencies and faults in a standard flora!

Fernald's concentration on plants of the "Manual" region began with his early work on the local flora about Orono, Maine. His first botanical correspondents, John Parlin and Kate Furbish, established the type of relationship that later extended to correspondents far and wide. His enthusiasm for regionally restricted floristic study was so boundless that it sometimes impressed others as ludicrous or boring. Published expression of it probably reached its height in an advertisement which he wrote for

the Bangor and Aroostook Railroad, published (anonymously!) in RHODORA for April 1903. Never before or since has there been such an advertisement, which should by all means be included in a bibliography of Fernald's writings. There have been many railroad and steamship blurbs directing the attention of tourists to such natural wonders as the big trees of California or the hot springs of New Zealand, but surely even the botanical traveller had never before been invited by a railroad in an advertisement of four pages of fine print to patronize its facilities in order to see such astounding treasures of Maine as the "rare Carices, *C. tenuiflora*, *gynocrates*, and *vaginata*, or the little sundew *Drosera linearis*." On the upper Mattawamkeag the botanist was promised no less than "the white foam-like masses of the spicy Labrador Tea, *Ledum groenlandicum*, rich rosy banks of the Pale Laurel, *Kalmia glauca*, indefinite white waves of the Alpine Cotton-grass, *Eriophorum alpinum*, brightened here and there with the deep yellows of *Cypripediums*." Nor must he fail to look below the surface, for, in the Piscataquis and the Mattawamkeag, would be found *Myriophyllum Farwellii* and *Potamogeton obtusifolius*! Elsewhere the visitor would thrill at the sight of "the largest of the Rattlesnake Plantains, *Goodyera Menziesii*, the rare Arctic Fleabane, *Erigeron acris*, the remarkable local Wood Betony, *Pedicularis Furbishiae*, unknown outside the St. John Valley." Finally, if these and other delights should pall, the prospect of even greater adventure was held out, for "the botanist whose good fortune takes him to the upper St. Frances may watch with hope for *Pleurogyne carinthiaca*, *Eriophorum russeolum*, *Astragalus elegans*, *Parnassia palustris*, *Saxifraga caespitosa*, *Anemone parviflora*, *Cornus suecica*, *Pedicularis palustris*, and many other arctic plants known closely to approach northern Maine"! I know nothing in botanical literature with quite the flavor of this advertisement except Bartram's "Travels." It would warm the cockles of any botanical heart.

Fernald's enthusiasm was literally unbounded when he had made or thought he had made some discovery, whether in the field, or among his own collections, or those of his correspondents. The moment anything came to light that seemed to require the segregation of a new species or a revision of the accepted delimitation of some group, he would immediately start sorting the

material of the Gray Herbarium into piles. In his earlier days, at least, he seemed to have a sublime trust that all essential material would either be ready at hand in the Gray Herbarium or that a problem insoluble with its then available resources could well wait until he had personally seen to the collection of new specimens. He rarely borrowed from other herbaria except



FERNALD AT WORK IN THE GRAY HERBARIUM.

for the utilization of what might be at hand in the collections of the New England Botanical Club. As for the Herbarium of the Arnold Arboretum, it might as well have been in Timbuctoo as in Jamaica Plain, so far as any utilization of it during my five years in Cambridge was concerned. During that period, I believe I am correct in stating that Fernald did not once visit the Arnold Arboretum, nor did his colleague Robinson more than once, and then not with a botanical objective.

Like many older herbaria, the Gray Herbarium in the pre-Fernaldian era had been developed to conserve space and costly paper by gluing specimens supposedly of the same species to the same sheet, quite regardless of geographic origin. This exasperated Fernald beyond measure, for it made it impossible to sort specimens into piles, first by one characteristic and then by another, or geographically, with the ceaseless industry that was Fernald's when he was intent upon a problem. So many a time

he started work on a genus by snipping all the mixed sheets to pieces, which he strewed in apparent unconcern all over the big central table in the main herbarium room, where he always worked. Dr. Robinson would come in, look with consternation and anguish at the wreckage, allow himself to remark gently that he wondered "how anyone could ever again interpret the concepts of the Synoptical Flora," and retreat from the painful sight into the old Gray study. (Among the Harvard botanists of the time Robinson was the chief exponent of the soft answer that turneth away wrath.) Then Miss Anderson would gather up and patiently glue all the severed fragments onto new whole sheets, but with each specimen by itself.

Fernald's technique, in those days, at least, was not to make notes and study those until he arrived at a classification of specimens that satisfied him. There was no tabulating of data. Nothing would do but interminable rearrangements of the specimens themselves. Once when Dr. Robinson mildly suggested that much deterioration could be prevented by sorting notes instead of specimens he retorted indignantly that one who had a *feeling* for the importance of *habit* would never be satisfied to handle notes instead of specimens. "Could you," he said, "see something you had quite overlooked before if you were just sorting cards?"—and the argument was unassailable. Even when sorting specimens by measurements of some organ he seldom, if ever, recorded each measurement and subjected the data to even the most rudimentary statistical analysis. Rather, he decided upon some measurement that might set the best limit between two groups, and sorted his material as "greater" or "less" than that. If the separation by a single critical measurement of a mass of material into two piles failed to correlate with other criteria of distinction he simply tried again with a new measurement.

Even though Fernald was prone to be satisfied to come to conclusions by examination of only the material that was at hand, he was indefatigable in making some disposition of every specimen that he had, down to the poorest. He was not one to pick out a single apparently distinctive specimen and describe it as a type, hoping that time would confirm his judgement. He was unsparing in caustic criticism of persons who would propose a

new species, or even a variety, without making a decision about the identity of every other related specimen at hand. Whatever may be thought of Fernald's segregations, they were proposed after making a conscientious identification of every related specimen that he had, according to his own criteria. He was especially suspicious of proposed systematic entities that did not seem to have a consistent or logical geographic distribution, and having to interpret or recognize one of Greene's numerous species, based upon a single specimen, caused him extreme indignation.

He was therefore sometimes vigorous in his denunciation of his own earlier work on the Mexican species of *Salvia*. He never referred disparagingly to "species" of *Salvia* however, except when hearers had a shrewd suspicion that they would get his meaning more correctly if they mentally substituted "Eupatorium" or "Crataegus" for "Salvia."

In his talks to students Fernald was often quite intemperate in his criticism of other botanists. On one occasion when he saw written down in our notes some of the disparaging remarks he had made about botanical colleagues at various institutions he was deeply chagrined and apologized for having gone so far. As he warmed to his subject, however, he was soon very nearly back to the point of departure. Still, he had a very generous appreciation of many other botanists. He generally referred to Bicknell's work on *Sisyrinchium*, for instance, with commendation, but had a very low opinion of Burgess's work on *Aster*. It caused him a pang when he had to admit the validity of certain of Green's propositions in *Antennaria*, or Nash's and Ashe's in *Panicum*. Fernald's denunciations of other botanists and their work were not often intended to hurt, however, and after he himself had forgotten making them, were quite as likely as not to be followed by friendly and appreciative expressions.

A good example was given by his reaction to Kükenthal's work on *Carex*. At first sight he thought it magnificent. On second sight he found it full of exasperating errors, which he condemned roundly. Then he thought that what the worthy pastor needed was to spend a year in Cambridge as his guest, learning American geography and examining plenty of American material. But Kükenthal couldn't come. "Then he isn't much of a botanist

anyway, if he doesn't accept a chance to correct his errors, and would rather just let them go," raged Fernald, and condemned the whole breed of Germans in general for gross carelessness, and Kükenthal in particular. Then finally he arrived at the conclusion that Kükenthal had done a fine job after all but had made sundry little errors, that he, Fernald, might quite calmly rectify himself! One German of whom he always (that is, *nearly* always!) approved was Buchenau.

Nearly every investigation, large or small, that Fernald undertook in the interval between the two last editions of the "Manual" may be considered a preliminary study for the eighth edition of that work, but was promptly published in RHODORA. This journal afforded him an outlet for one or more articles, critical reviews and notes each month, from the very beginning, the whole representing a prodigious amount of writing. As time went on, this journal became more and more an expression of his personality and views. Sponsored by the New England Botanical Club, in which Fernald's strong personality was dominant, it early came to be one of the most highly personalized of scientific journals, in an era in which most editors have deemed it scandalous to reveal any personality at all.

In the early days of RHODORA there was an annual meeting of the Club at which the editorial board of an imaginary "Rhodorella" made a report that tingled with satire and fun. It was much in accord with Fernald's impulses to express himself with an informality and freedom that seldom appear in these stodgy and formal days. So the reviews in RHODORA were sometimes almost as spicy as they might have been in Rhodorella!

Since Fernald's prejudices were so strong and so unsuccessfully inhibited it is quite understandable that he did not keep every trace of his personality out of the "Manual." For example, he had no use for spurious common names, made by translating scientific names into English, and had the courage of his convictions in refusing to adopt them in the Manual. The sheer pedantry of "standardized common names" got no encouragement from him. He required that common names, to be admitted, must belong to common language, not merely to an artificial jargon. It is greatly to his credit that he was content to be considered reactionary in this respect. He was likewise

reactionary to a certain extent in disregarding some of the technical jargon that distinguishes newer developments in systematics. It is just as well. There is much pomposity and verbosity in science nowadays that has little if any utility. Fernald worked from the end of the period in which systematics dominated botany, through a period in which systematics seemed old-fashioned and on the wane, and into a new period in which systematics is being revitalized by the experimental investigations of geneticists, cytologists and biochemists. Methods of investigating relationships are becoming more and more critical and time-consuming. The days of dependence in distinguishing species upon intuitive perception of the integrity of an assemblage of characteristics may be thought to have passed. If, however, we consider the need for comprehensive works on large floras, the small number of botanists to do the world's vast botanical work, and the inadaptability of many plants to experimental investigation, then Fernald's life work in the honorable "Old Systematics" will be seen to have a value that the passage of time will not soon efface. In view of the application of experimental methods in an increasing number of groups, it becomes more and more apparent that the time during which one man can come to have a critical knowledge of most of the flora of a region as large as that of Gray's Manual has passed. So his book will be a lasting landmark in the botanical history of our region. It is a source of deep satisfaction to his devoted botanical following that he lived to see it in print.

As I have said elsewhere, "the new Manual is a highly satisfactory and noble achievement, the culmination of a lifetime devoted to the reinterpretation of our flora, as largely on the basis of zealous personal field work as half a century would permit."

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