SCIENCE

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THE CHANGE FROM THE OLD TO THE NEW BOTANY IN THE UNITED STATES[1]

IT is generally known that in the seventies there was a sudden development of the study of botany in this country. Just how and why this sudden development took place at that particular date is, I suspect, not clearly recognized, at least by our younger men. From histories and reports of progress they can learn the main facts, but those who, as students or instructors, have lived through the transitional period when the old botany was changed into the new are in a better position to appreciate the underlying causes. There are, however, few such persons still living and the small number is not wholly due to the normal death rate. The relative number of botanists was smaller then than now and it will not do to assume that this was owing solely to the lack of attractions in the botany of the day. The main reason was that one could hardly expect to earn a living as a botanist. When I graduated from college in 1866 and wished to become a botanist, Professor Gray told me that I ought to study medicine first because the possibility of gaining a living by botany was so small that one should always have a regular profession to fall back upon. In fact, at that time medicine was practically the gate through which it was necessary to pass in order to enter the field of botany. Some years later De Bary told me that, when he was a young man, there was a similar state of things in Germany and, although desiring to devote himself to bot-

[1] Address of retiring president of the Botanical Society of America, given at the Botanists' Dinner, Cleveland, January 1, 1913.

any, he had to study medicine, taking his degree in 1853. In 1872, however, things had changed in Europe and when I went to Strassburg to study I was the only student in De Bary's laboratory who had studied medicine. The others had begun the special study of botany on entering the university and were, although no older than I was, much better trained in botany.

In 1866, there were very few botanical professorships in this country, the salaries were very small and the equipment very shabby. Gray was professor at Harvard, D. C. Eaton at Yale and Porter at Lafayette. Torrey, in spite of his distinction as a botanist, really depended on his position as a chemist for his living. The comparatively few positions in government and state stations offered few attractions and changes were frequent. To a young man the prospect was not assuring.

If we look further and ask what was the attitude of the public towards natural science, we find a state of things very difficult to appreciate at the present time. This can be illustrated by my own experience as a school boy. When I was in the high school one of the books we had to study in the upper classes was Paley's "Natural Theology." You may perhaps infer from this that the object was to give us religious instruction. Not at all. The real object was to smuggle a little human anatomy into the schools. This was the way it was done. Very few of you probably ever heard of Paley's "Natural Theology," in its way a remarkable book. In the opening chapter Paley supposes that a man walking in the fields finds a watch on the ground. He sees the complicated machinery adapted to a definite purpose and therefore, according to Paley, at once infers that it must have had an intelligent creator. How much more strongly, therefore, should a contemplation of the organs of the human body, well adapted to perform special functions, lead us to believe in the existence of an intelligent creator. Paley then proceeds to give a rather mild account of human anatomy illustrated by plates intended to impress the readers; a ghastly head with the cheek dissected to show the parotid gland; an abdomen with the lid removed to show the bonbons inside, the stomach and spleen ingeniously arranged so as to show also the deeper lying organs, etc. Paley's reasoning does not now seem altogether convincing. If you or I had found the watch, we should have seen that it was complicated and we should have known that its purpose was to show the time of day. We should have known also that it had been made by a watchmaker. If, however, a savage who had never seen or heard of a watch had found one in the field, he would have been mystified by the mechanism and would not have had the least idea what its purpose was. Instead of recognizing an intelligent creator he would have regarded the watch itself as a god.

Now, at the time of which I am speaking, it would not have been proper to teach anatomy *as such* in the schools, but anatomy, so far as it served to show the goodness and intelligence of the creator, was quite legitimate. In other words in studying natural history one must never forget that God had made man to be the center of the universe and all other things had been arranged for the benefit of man, and, when facts to the contrary appeared, they must be properly interpreted or denied. Since an omniscient and omnipotent being can not make a mistake, all the species of plants created in the beginning must forever remain as they were created. With this simple theory of living things people were perfectly contented until in 1859 the "Origin of Species" fell like a bomb in

the camp and shattered time-worn theories. That the variations and adaptations of plants and animals were not for the benefit of man, but for the benefit of the plants and animals themselves, was a dreadful heresy. The violence of the controversy caused by Darwin's great work was something of which the present generation can have no conception. It was at its height when I was a college student. Young men were generally inclined to accept Darwin's views, and in our college natural history society most of the meetings were spent in discussing evolution. Some of us had really read the "Origin of Species," but all were ready to talk about it. The older men, even the naturalists by profession, were much more conservative. A few adventurous spirits were more Darwinian than Darwin himself, but college professors had to be careful in what they said, for practically the whole religious world and the greater part of college graduates were not ready then to accept evolution. The bitter feeling of the antidarwinians continued for a considerable number of years, as is shown by the following instance. A little more than twelve years after the appearance of the "Origin of Species" one of our leading universities wished to appoint a professor of zoology. The place was offered to a friend of mine with the stipulation that he should never, directly or indirectly, refer to evolution in his lectures. As my friend was one of the most rabid evolutionists in America, the conditional offer seemed amusing. He, of course, declined and the place was then offered to one hardly less radical in his views, and was again declined. It was rumored that the place was offered to a third person and again declined, but I have no direct knowledge that this was the case. The present incumbent, I presume, believes in evolution, but probably no one has ever taken the trouble to ask him whether he does or not for, at the present day we should no more think of asking a professor of zoology whether he believes in evolution than whether he is the fortunate owner of a tooth-brush.

At a time when many of the leading zoologists, including Louis Agassiz, were strongly opposed to Darwin's views, the botanist, Asa Gray, exerted a powerful influence in converting the public to the doctrine of evolution. His simple and attractive style enabled him to reach an audience which would have been repelled by the dryness generally supposed to be characteristic of scientific writings. He was also known to be a member of the orthodox church and the good religious people of the country said: if the orthodox Gray sees in evolution nothing inconsistent with revelation, why may we not also accept it? Furthermore, Gray did not go too far in his views, whereas some of the evolutionists started off on a wild sea of speculation whither the public would not be expected to follow.

Having tried as far as the limited time allows to give you an idea of the attitude of the public towards natural science, at the time when I began the study of botany, a word may be said about the botanical instruction in colleges. At Harvard botany was a required study for the whole class during half of the sophomore year. The text-book was Gray's "Structural Botany." Gray had no assistant. To require botany of a whole college class—I am not speaking of agricultural schools—is enough to condemn it to neglect and abuse. This, however, can be said of college students. If their instructors do not interest them they are always able to amuse themselves. In the corner of our lecture room was the trunk of a palmetto which had been used to grace the funeral procession of Calhoun

and afterwards given by Professor Gibbs to Gray as of historical as well as botanical interest. It was the duty of the athletes while the attention of the instructor was diverted to seize the trunk and carry it to the entry and later on to start it rolling down the very winding staircase. This method of studying botany I discovered later was not confined to Harvard. Once while visiting a western university I noticed, to my surprise, a cannon ball back of a door. I asked why it was there and was told, not by a student, but by the instructor himself, that during the lectures the students rolled it along to the head of the staircase when gravity was left to do its perfect work. Afterwards some attention was paid to the lecturer, and how much was learned on any one day depended on how early in the hour the cannon ball was started on its way. Compulsory botany was not a success. In my junior year eight or ten students who really wished to study botany asked Gray to give them some instruction in systematic botany during the season when fresh material could be obtained. The work on our part was entirely voluntary and in addition to our regular work. It was not recognized by the college and we received no credit for it in the rank list. The number of voluntary workers was reduced to two in my senior year, when we had so much regular work as to leave almost no spare time. I have noticed in recent years a growing disposition to demand some reward in the shape of a degree or a certificate of some kind for any work done outside the regular curriculum. To do work for the pleasure of adding to one's knowledge is, I regret to say, getting to be a sign that one is not up to date.

On graduating I followed Gray's advice and entered the medical school, hoping sooner or later to be able to return to botany. The opportunity came in 1870 when Gray returned from Europe. During his absence Horace Mann, Jr., who had been taking his place, died and I was then appointed assistant. I was always interested in cryptogams and, had it been possible for me to do as I pleased, I should never had studied anything but marine algæ during the rest of my life. It became my duty to arrange the thallophytes of the Gray Herbarium and the work I did was radical, I assure you. Not knowing that Littleton Island was near the North Pole, but supposing it to be somewhere in Long Island, I arranged into the waste-paper basket a number of rather shabby-looking algæ which I afterwards discovered to my mortification were very rare. It did not take long for me to find out that, whatever professors of pedagogy may say, one can not teach a subject without knowing something about it. But where was I to go to study cryptogams? It was proposed that I should study fungi with M. A. Curtis, but he died in 1872. For marine algæ I had to depend on Harvey's "Nereis" and J. G. Agardh's "Species," works which were not easily followed by a beginner, with occasional reference to the by no means exhilarating "Micrographic Dictionary."

Evidently, I must go to Europe, and Germany was the country whose universities offered the greatest facilities for my purpose. The most promising were those of Strassburg, where De Bary was professor, and Wuerzburg, where was Sachs. I chose the former rather at a venture. The other botanists there were Solms and Fr. Schmitz, then a very young man whose work had been in histology. The venerable W. P. Schimper, the bryologist and paleontologist, whose valuable herbarium had been given to the university before the Franco-German war, remained in charge of it and gave a course of lectures. My fellow students were Stahl, Rostafinski,

Gilkinet, Suppanetz, an Austrian, Kamienski, who recently died at Odessa, Karl Lindstedt and Doelbruck, who died young. I learned that I was not the first American who had studied with De Bary. A short time before, while he was professor at Halle, an American, T. D. Biscoe, had taken a course in botany, although not studying botany as a specialty. The only information I have in regard to Mr. Biscoe is that he published a paper on the winter state of our duckweeds in the *American Naturalist* of 1873. There was only one other American, a law student, at Strassburg when I arrived there, for, to the surprise of my fellow-botanists I was not willing to acknowledge as a fellow-countryman a Chilian, whose principal occupation seemed to be duelling and whose English vocabulary was limited to the two words, "damn Yankee."

The general arrangements at Strassburg were the same then as those of other German universities at the present time, but the method of working in the laboratory was very different. I was given a *Chara* to study and in a couple of hours reported that I had studied it. I was told that I had not even begun. Studying, it seems, meant that I must make sections through the scheitel and trace the successive cell-formations. But how was I to make a section and what was a scheitel? The microtome and modern methods of imbedding were then unknown to botanists and all sections had to be made by hand. The nearest approach to imbedding was in sectioning small objects like pollen grains; a few drops of mucilage were placed on a cork, the pollen mixed with it and the whole allowed to harden. Then by holding the cork in one hand one could make sections of the pollen if one were lucky. The student of the present day, when hand-sectioning seems almost a lost art, does not realize what skill in sectioning could be acquired by practise, but, like playing on a musical instrument, constant practise was needed to keep one's hand in. Modern technique, which was borrowed by botanists from the zoologists, has of course many advantages, especially in cytological work, but, for certain work, hand-sectioning has its advantages, as, for instance, the rapidity with which sections can be made.

If I was fortunate in my fellow students at Strassburg, in one respect I was less fortunate. At the time De Bary himself was at work on his "Vergleichende Anatomie," which was published in 1877. Anatomical studies were not his strong point, but, in an unguarded moment, he had promised Hofmeister that he would write the volume for his series and he felt in duty bound to keep his promise. We should have preferred to have had him working on the mycological subjects in which he excelled, but the management of cell cultures and the technique required in such investigations were taught to his pupils. Rostafinski took his doctor's degree while I was in Strassburg, with the thesis, "Versuch eines Systems der Mycetozoen." The monograph of that group did not appear until 1875. I happened to hear De Bary and Schimper talking about Rostafinski's thesis, which they thought was a good work, although they regretted that he had made so many genera. What would they say were they now living, when it almost seems as if we were trying to create a new genus for every species?

In the laboratory I noticed that the students seemed to refer frequently to a book of which I had never seen a copy or even heard. The book was Sachs's "Lehrbuch," second edition, 1870. I bought the book and was perfectly amazed. I had never dreamed that botany covered so large a field. The "Lehrbuch" was an ad-

mirable summary of what was known of all departments of botany up to that date, well written and excellently illustrated. The fourth edition, which appeared while I was in Strassburg, was still better. On looking at the second edition a number of years later, I noticed what seemed to be a curious omission. No mention whatever was made of bacteria. In the fourth edition they are mentioned under *Schizomycetes*. The absence of reference to bacteria in the earlier edition, however, was not an omission. There were no bacteria at that date. There were no bacteria until Cohn published his "Untersuchungen über Bacterien" in 1872. The fact that forty years ago Sachs had never heard of bacteria, while to-day life has almost become a burden, one hears so much about them, is a striking instance of the rapidity of development of a subject having a practical as well as a theoretical value. I know no single book which has had so great an influence in shaping the course of modern botany as Sachs's "Lehrbuch." It may be that the facts there given were generally known in Germany, but they were not known in other countries. On returning home by way of England in 1874, I showed my copy of Sachs to several English botanists and it was evident that it was quite new to them. It was certainly unknown in America. If imitation is the sincerest flattery, the value of Sachs's "Lehrbuch" was quickly recognized, for, using it as a model or basis, there soon appeared a large number of really excellent text-books in various languages in which one recognized Sachs translated, Sachs condensed, Sachs diluted, Sachs trimmed to suit local demands. Publishers, were they capable of gratitude, would have erected a monument to Sachs's memory long ago. Draughtsmen, on the other hand, had little reason to bless his memory. Even now we can hardly open a new text-book without seeing the inevitable "after Sachs."

One evening I was present at a dinner given by De Bary. On that gay and festive occasion I heard more gossip about botanists than one hears even at a meeting of the Botanical Society of America. My neighbors kept saying: "der schmutzige Kerl." On asking who the dirty fellow was, they said Naegeli. In my innocence I inquired what Naegeli they meant. They answered "*Der* Naegeli." Even starch could not save his reputation, and they proceeded to tell not one but many tales which I know you are dying to hear but which I am not going to tell you. What I wish to say is this: At the same dinner some one, possibly Rostafinski, spoke of a certain Strasburger, a botanist. I understood him to refer to some botanist living in Strassburg and asked his name. I was told that he was a Pole named Strasburger who lived not in Strassburg but in Jena and had written a work which showed him to be a promising young man. That was the first time that I had heard of Strasburger, who had not then begun his work in cytology. The promise was fulfilled and the young man of 1873 became one of the bright lights of the botanical world. At the close of his long but too brief career he left a brilliant school in a department of botany which he had created and of which he remained until his death the leading spirit. Fortunately we have with us a younger generation admirably qualified to continue the work which he began.

For the last twenty years most young American botanists have thought it necessary to study in Germany to complete their education, but, when I returned in 1874, I was looked upon very much as one would be who had returned from a journey in Thibet or Central Africa. Things had

changed. The country had recovered from the effects of the civil war, money was more abundant and more could be spent on science. New professors were appointed in the colleges and courses for the instruction of school teachers in botany and zoology were provided by private individuals. I have time only to refer to one curious episode in the development of botany in America. I refer to what may be called the biological epidemic which broke out soon after I returned to America and threatened for a time to drive botany from the field. If at some future time some one ventures to write a book on the abuse of the "ologies" the chapter on biology will be the most interesting. As far as I can make out, as originally used, biology did not differ much from physiology. The laboratory manual of Huxley and Martin was planned to correct the common idea that botany and zoology consisted in the description of different species of plants and animals, whereas in reality they are the study of plants and animals in all their relations to one another and to their surroundings. Huxley and Martin's book was extensively used in this country and was in many ways excellent. The criticism might be made that it was not well proportioned. Without saying that it was all lobster, there was so much lobster and so little of plants that there was not enough to make a good lobster salad. Soon it became the habit of young persons who knew precious little about either plants or animals to call themselves biologists, disdaining to be called botanists or zoologists. It does not follow, however, that because one is neither a botanist nor a zoologist one is to be considered a biologist. Trustees of colleges and similar institutions were given to understand that a superior race of beings had arisen, the biologists, and that botanists and zoologists had had their day. Colleges being always impecunious, this information was gladly received by their governing boards. By calling their zoologists biologists they could escape appointing professors of botany. This clever device for saving a salary worked very well for a few years, but at last it became evident that the teaching by a zoologist with the aid of a text-book, how to distinguish a yeast cell from a fern prothallus and a fern prothallus from a germinating bean, was not all that was wanted in our colleges, although it might have been sufficient in a kindergarten. The epidemic of biology, although it hindered for a time the development of botany in England and America, fortunately never spread to other countries.

Although garrulity is the privilege of old age, I feel that I am still too young to take up more of your time this evening. This occasion, in which the body as well as the soul naturally participates, seemed to me to call not so much for a formal historical account of botany in my day as for a series of personal reminiscences, more or less anecdotical in form, which would throw a little light gained from the experience of one who, although he has lived long, hopes that he has not outlived sympathy with the present, on some of the steps by which our present advanced position among the botanists of the world has been reached. It has been my fortune to see the old order of things overturned by the appearance of the "Origin of Species" which, by freeing science from the fetters of a semitheological bias, opened the way to a free scientific study of the distribution of plants and animals and the great questions of heredity and evolution. To most of you this great change is only a historical fact. To me it is a living memory. I, who was almost the first American student to seek the benefit of botanical instruction abroad,

have lived to see the time when a very large number of our botanists have brought back to America the best that Europe had to offer. There was a time when our botany might have been said to bear the mark "made in England." In more recent years it may be said to have been "made in Germany." There are some patriotic souls who hope that the time will come, if it has not already come, when we may say "made in America." I do not share their feeling. To me it seems that botany is destined to become more and more widely diffused until it becomes world-wide and it will be enough if we contribute our proper share to the general stock. I have lived to see the growth of several branches of botany which practically were not studied at all when I was young. Bacteriology and cytology are of recent origin. Plant physiology has been with us a child of slow growth, but it frequently has been the case that the strongest men have been slow in their development. Plant pathology from a crude and semi-popular beginning has become an exact science in whose study and practical application we have already surpassed other nations. When this society meets forty years hence, I shall not be present. Few of you will be present. But whatever of progress the speaker on that occasion may be able to report will be the result of a gradual development. It can hardly be expected that he will have to record any such radical and complete transformation as it has been my privilege to present to you this evening.

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THE SIMULIUM-PELLAGRA PROBLEM IN ILLINOIS, U. S. A.[1]

The advancement of entomology owes much, of recent years, to the stimulus supplied by the discoveries made by medical men with respect to the agency of insects in the transmission of contagious diseases; and just now our knowledge of the species, distribution, habits, life histories and ecology of *Simulium* is progressing by leaps and bounds in consequence of the well-known *Simulium* theory of the transmission of pellagra, announced by Dr. Louis W. Sambon in 1905, and fully elaborated by him in the *Journal of Tropical Medicine and Hygiene* in 1910.

This stimulus to a study of these insects reached me, in one of the interior states of North America, in August, 1910, when, in consequence of the appointment by the governor of Illinois of a state commission for the investigation of pellagra as occurring in the insane asylums and other institutions of that state, I was requested, as the official entomologist of Illinois, to contribute to their report an account of the distribution of *Simulium*, especially in the neighborhood of state institutions in which cases of pellagra were occurring. As an investigation of all insects injurious or dangerous to the public health in Illinois is one of the prescribed duties of my office, I was bound to avail myself, to the best of my ability, of this opportune call. This I did by detailing an assistant, Mr. C. A. Hart, August 8, 1910, to commence observations and collections along the central part of the course of the Illinois River, and especially to make a careful survey of the vicinity of the general Hospital for the Insane, built upon a bluffy bank of that stream near the city of Peoria. My reason for giving particular attention to this asylum was the fact that it had been the principal seat of pellagra in Illinois, containing in 1909 eighty per cent. of the cases of this disease—that is, one hundred and twenty-seven out of two hundred and twenty—recognized that year in the whole state. This bad preeminence has, in fact, been since maintained, this asylum containing sixty-three per cent. of the four hundred and eight cases known to occur in Illinois during the twenty-six months preceding the first of September, 1911.

[1] Read at the Second International Congress of Entomologists, Oxford, England, August 8, 1912.