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THE FAD AS A FACTOR IN BOTANICAL PUBLICATION[1]

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BUREAU OF PLANT INDUSTRY

IF, as Pope and many others have asserted, "the proper study of mankind is man," botanists may occasionally study botanists and no apology is needed for asking this botanical society to direct its attention to one of the manifestations of botanical psychology. It is obvious that all we know about plants comes to us through the medium of the botanical mind, and in studying the botanical publications of any period, it is important to know what botanists were thinking about at that time. For, much as we may dislike the idea, we must admit that the conclusions which investigators draw from their observations, perhaps even the observations themselves, or at least the kind of observations they are most likely to make and to publish, are influenced by what others are observing, publishing and talking about.

In the work of the Plant Disease Survey we deal constantly with observations made by others, and in an attempt to study the relative incidence of disease at different periods it becomes of first importance to discover what particular diseases were in fashion and thus most likely to be noticed at any given time. It was, then, this practical necessity which led me to spend a good deal of time during the past year in reviewing American botanical literature. Some of the incidental results of this study I wish to discuss tonight. To avoid wearying you beyond endurance I have confined the statistical portion of this paper to the last 50 years, 1881 to 1930, and to the following representative American publications: *Bulletin of the*

[1] Address of the retiring president of the Botanical Society of Washington, D. C.

Torrey Botanical Club, Botanical Gazette, Phytopathology, American Journal of Botany and the botanical material contained in the scientific publications of the U. S. Department of Agriculture, including the *Journal of Agricultural Research* and the *Contributions of the U. S. National Herbarium* in so far as these represent the work of the members of the Department of Agriculture.

I am aware of course of the immense volume of American botanical matter outside of this list and of the still greater volume outside of the United States, but the material chosen is merely illustrative, and I must bear in mind the necessity of having enough people left at the conclusion of this paper to elect officers.

The curve[2] which gives the total pages of botanical matter exclusive of reviews and abstracts in these publications indicates an increase so great as to eliminate any question of observational error. From a paltry 259 pages in 1881, we attained in 1929 an all-time American high of 5,284. To be sure, the botanical output, like the stock market, broke sharply in 1930, and the curve of increase seems to lose some of the steepness after 1918 or 1919. Nevertheless, this output, especially when viewed against a background of other special journals, experiment station publications and foreign literature in many languages, presents an aspect which is little less than appalling.

Of what does this mass of botanical print consist?

Its composition varies from year to year as a little study or even reflection will speedily reveal.

SPACE DEVOTED TO VARIOUS LINES OF BOTANY IN THE AMERICAN PUBLICATIONS LISTED IN THE TEXT

Year	Total pages	Approximate per cent. of total pages given to				
		Systematic botany	Morphology	Physiology	Ecology	Pathology
1881	259	85				
1890	640	61	5	2		24
1900	1,925	34	9	5	1	33
1910	3,342	33	8	6	6	11
1920	4,437	9	11	18	6	29
1930	3,841	10	13	26	1	41

Taking 1881 and the succeeding decimal years as examples we find that of the 259 pages published in 1881, 85 per cent. was systematic botany, including work on local floras. In 1890, of a total of 640 pages, systematic botany made up 61 per cent., pathology 24 per cent., all about diseases due to fungi, morphology 5 per cent. and physiology 2 per cent. In 1900 there was a total of 1,925 pages, of which systematic botany occupied only 34 per cent., morphology and physiology 9 and 5 per cent., respectively, while there was 1 per cent. of ecology and 33 per cent. pathology. The figure for ecology is unusually low and that of pathology unusually high, as 1900 seems to have been an exceedingly favorable year for pathology. Ten years later, out of a total of approximately 3,300 pages, 33 per cent. was systematic botany and 8 per cent. morphology, about 6 per cent. each physiology and ecology and 11 per cent. pathology. In 1920, of a grand total of 4,437 pages, systematic botany made up less than 10 per cent., morphology 11 per cent., physiology 18 per cent., ecology 6 per cent. and pathology 29 per cent., two thirds of which was about fungus diseases. Last year, out of a total of 3,841 pages, systematic botany had about 10 per cent., morphology 13 per cent., physiology 26 per cent., and pathology showed 41 per cent. The apparent slump in ecology is largely due, no doubt, to the establishment of an independent journal for papers in this branch of botany and zoology.

What causes these differences? Why, for example, do systematic papers make up only about 10 per cent. of the total botanical material which appears in these standard journals? I realize, of course, that many good systematic papers appear outside these series and that the decline is in part relative, but no one will contend that systematic botany occupies anything like the position it did fifty or even thirty years ago. No one will maintain either that the necessity for systematic work is past. Nor am I convinced that brains capable of serious taxonomic studies are no longer produced in America. I have heard various reasons advanced to account for this obvious decline, among others, that the systematists have made themselves ridiculous by describing numerous scarcely distinguishable "species" by endless changing of names and bickering about nomenclatorial rules. It has also been argued seriously by competent botanists that taxonomic work has been rendered more or less futile by the concept of evolution and that systematic botany depends for its existence on a belief in the fixity of species. However much weight these considerations may have, it seems to be the fact that taxonomy is out of fashion just as truly, if not quite so completely, as snuff.

The question just raised may fairly be reversed and injury made as to why morphology, physiology and pathology did not occupy larger places in Ameri-

[2] The paper was illustrated by a series of charts showing the volume of publication on various phases of botany during the period 1881 to 1930. The general trend of these curves is indicated in the text with sufficient detail to be easily followed by any one familiar with American botanical literature.

can botany in 1881. From a much longer list I will cite a number of discoveries or lines of work published between 1859 and 1869 well within the range of facilities then available in the United States and sufficiently striking to have deflected a substantial fraction of botanical interest but for the existence of strong inhibiting influences.

1859 The "Origin of Species," from which the study of structural adaptations, mechanisms of pollination, distribution of seeds, and related problems received a great impetus in Europe.

1861 Pasteur's discovery that yeast and several species of bacteria were able to live in the absence of oxygen.

1863 Max Schulze's demonstration of the identity of protoplasm of plants and the so-called "sarcode" of the animal physiologists.

1863 Work of Sanio on the process of secondary thickening of the axis of the Dicotyledons and Conifers.

1864 De Bary's demonstration of the heteroecism of the stem rust of wheat.

1860–5 Sachs' work on photosynthesis and especially the application of the iodine test for starch.

1865 Darwin's study of climbing plants.

1866 Sachs' great work, "Experimental-physiologie der Pflanzen," was published. Yet plant physiology did not get fairly under way in the United States until about 1890.

1867 Hildebrandt as a result of crossing yellow and a dark brown race of maize noticed xenia, although he did not call it by that name.

1868 The first edition of Sachs' "Lehrbuch."

1868 Williamson's first memoir on the coal measures.

1869 Darwin's work on heterostylism.

To the question as to why physiology was not more vigorously pursued in the United States fifty or sixty years ago, one is tempted to repeat that it was out of fashion at that time. Possibly the Victorian botanical mind recoiled instinctively from the study of the life processes and sexuality of plants. Speaking more seriously, the influences which kept American botany in the face of all this distraction almost exclusively on the single track of the taxonomy of flowering plants must have been profound. First of all, of course, was the exploration urge. We were then at the period when the finding of new things in the field took precedence over all other activities, but together with this, emphasizing it and, I believe, extending the period, was the influence of Asa Gray, who, for a period of over thirty years, dominated American botany more completely than any one botanist is likely to again. So complete was this domination that even the systematic study of the lower forms of plant life was almost crowded out of the picture.

The story of the change from this condition, which you may designate as the emancipation from the fetters of the herbarium or, if you prefer, the degeneration of American botanical science, is perhaps more easily traced in the graphs showing pages of publication. And this may be a good time to emphasize again that I am not discussing facts but conceptions, not value of publications but volume of publications.

Physiology, morphology and pathology are all represented, although, of course, very scantily in the first year included in this review. The first to expand markedly was pathology. The flood of papers on plant pathology, especially on diseases due to fungi, followed closely on the perfection by Koch of the plate method of isolating bacteria and fungi, and the discovery by Millardet in France, of the effectiveness of copper and lime as an agent in the control of downy mildew of the grape. Whether we have passed the crest of the curve of production of print on fungus diseases is, of course, problematical. But no doubt some of our colleagues would warmly welcome such a change and there are some indications of its approach. It may be a sign of the times that whereas we used some years ago to hear from mycologists much of the importance of mycology to phytopathology, the summer of 1930 found them insisting at Cambridge that mycology was entitled to consideration in its own right wholly independent of plant pathology.

The story of the rise of physiology, morphology and cytology in the United States is largely the story of the importation, belated importation, perhaps, and the development here of botanical conceptions and methods already under way in Europe. In 1888 or 1889, both physiology and morphology entered on a period of expansion which became more marked nine or ten years later. Ecology claimed a place in the American sun in the year 1899, three years after the publication of Warming's great work. The "Sem Bot" of the University of Nebraska was engaged in a study which would have led to ecology before 1892, but the cumbersome term they employed, "phytogeography," stood no chance against the shorter term, "ecology."

However, I am less concerned now with these large waves of interest than with the smaller wavelets which may fairly be designated as fads. I might point out, for example, that in the control of plant diseases we have passed through, during the last fifty years, a Bordeaux period, a lime sulfur period, a dusting period, and are now in an eradication and quarantine period. Of course Bordeaux and lime sulfur are still used, but they are no longer talked about. Our most recent pathological fad is obviously that on virus diseases, which was launched by the work of Allard, 1913 and 1914, and reached what may be a crest in 1926.

Nor are other branches of plant science free from the influence of fads. Among American morphological publications during the period under discussion, two conspicuous fads may be mentioned. The embryo sac fad which began about 1894 or 1895 and continued to show considerable activity through 1916, but is now apparently practically over, and the chromosome fad, which got fairly under way in 1897 and 1898, following Strasburger's great generalization regarding the different numbers of chromosomes in the two generations of a plant, slumped to almost nothing in 1916–17–18, and is now enjoying a second run of popularity due apparently to the discovery that there is a connection between the number of chromosomes and the possibility of producing fertile hybrids in certain genera.

In recent physiological papers the most noticeable fads, at least to the outside observer, are the study of hydrogen-ion concentration and the study of light relations. This last started by the publications of Garner and Allard on the relation between duration of daily illumination and reproduction of certain plants.

Are these Waves of Interest Fads?

Webster defines fad as a "hobby, whim, custom or amusement followed for a time with exaggerated zeal."

The International Dictionary elaborates this somewhat and defines a fad as "a trivial fancy adopted and pursued for a time with irrational zeal; a matter of no importance, or an important matter imperfectly understood, taken up and urged with more zeal than sense." I leave you to judge of the correctness of the term, but I can find no better English word.

What Starts a Fad?

In the first place it seems to me, that in order for a real fad to start the stage must be set. Apparently there is no great chance of a fad taking hold close on the heels of another one. It must wait until the collective botanic mind, or rather a portion of the botanic mind, has reached a condition approaching saturation. Note, for example, that the virus disease fad came to the relief of the phytopathologists, as fungus disease publication was approaching 900 pages a year and that ecology came into the United States when taxonomic work was getting close to 1,000 pages a year. Ecology seemed to offer a man with systematic instincts a chance to do a little systematic work without being too critical about the literature and synonomy, and to do some local flora work under a new and attractive name.

Given a favorable stage setting, the fad seems to be started by some discovery, paper or suggestion which is sufficiently different from the common run of good botanical matter to attract attention, but not sufficiently different to prevent its being readily understood. I may say, here, that I find the name of but one man connected with the inception of more than one fad. This is, of course, our fellow Washington botanist, H. A. Allard. I am credibly informed also that Allard is responsible for starting fads in the study of the synchronization of the stridulation of certain insects and the flashing of fireflies. Needless to say, I await with interest what this original mind will start next.

The Next Fad

I will not even venture to predict what the next fad will be. If I knew I should get out a paper on the subject immediately. A year ago I felt that we were well on our way to a fad for the study of peat bogs by the method of pollen analysis. But this, perhaps because it savors of fossil botany, seems to be thriving better in Europe than America.

Some Potential Fads that Failed to Materialize

It is, of course, perfectly possible for an outstanding, even a striking, achievement which attracts a good deal of attention to fail to produce a fad. I have never understood why Blakeslee's discovery of sexuality in the mucors did not start a fad. "Physiologically balanced solutions" and "antagonism," which were words to conjure with in 1906 and 1908, seem not to have caught the botanical imagination as did "length of day." "Carbo-hydrate-nitrogen-ratio" apparently started no such fad as did "hydrogen-ion concentration." One might reasonably have expected the outstanding success of Dr. Coville in developing the native blueberry and thus creating a new industry in the "barrens" of New Jersey to have aroused great interest in our uncultivated native fruits, but there seems to have been no great increase in such interest.

There are apparently certain lines of botanical investigation which have never taken very vigorous root in American soil. Notable among these is, of course, paleobotany, which, in spite of a few very distinguished workers and a wealth of available material, has never assumed a large place in American botanical work.

What Stops a Fad?

The real answer to the question of why fads stop is that they do not, they merely cease to be fads, that is, each of them leaves some more or less permanent imprint or, to change the figure, influences the course of botanical thought. On the other hand, we certainly do lose most of our interest in subjects before they become exhausted. One explanation which Professor S. W. Williston used to urge with some heat was that it was easier to pick up a new line than to

master the literature of an old one. Twenty years ago he predicted that ecology would become as unpopular as taxonomy as soon as the literature was sufficiently voluminous.

You have all heard the students of taxonomy, both animal as well as plant, express grief, even exasperation, that so little attention is paid to their work. I can see no help for this. Neither last summer could the owners of "Tom Thumb" golf courses find any way to bring back the patrons who thronged their courses in 1930. I would urge those who find themselves almost deserted in a no longer fashionable field of botanical effort to cease railing against fate, for, to quote Justice Holmes:

> The law of fashion is a law of life. The crest of the wave of human interest is always moving, and it is enough to know that the depth was greatest in respect of a certain feature or style in literature or music or painting a hundred years ago to be sure that at that point it no longer is so profound. I should draw the conclusion that artists and poets, instead of troubling themselves about the eternal, had better be satisfied if they can stir the feelings of a generation, but that is not my theme.

The Dangers of the Fad

The dangers of the fad are obvious. All investigators are possessed more or less with what some one has called "the devil of one idea." When a group is so possessed we get something very like mob psychology, which results in an inevitable bias in observation and publication. Illustrations of this will occur to each of you. Many of you will recall—at least from the reproductions in early editions of Wilson's book the "Cell"—that Guiginard described and illustrated centrosomes in the lily. It was soon apparent, or at least generally believed, that no such structures exist, but under the impulse of the fad for centrosomes they appeared real enough to this cytologist.

Some of you will recall or have noted in the literature that Dr. T. J. Burrill, honored wherever pathology is studied as the first to demonstrate the possibility of bacterial disease in plants, described,

> *Micrococcus toxicatus,* Burrill. Cells globular, single and in pairs, rarely in chains of several articles; .00002 in. in diameter; movement oscillatory only.

This organism he believed to cause the poisonous principle of species of Rhus and to be capable of penetrating the human skin and inducing the peculiar inflammation which takes place. So enthusiastic was Burrill over this imaginary discovery that he published it in three different places. Those who in 1922 saw the unrestrained enthusiasm of the recognized leaders of plant pathology over the preliminary and possibly mistaken announcement of certain organisms in the cells of plants affected with mosaic diseases were witnessing no new phenomenon. Similar enthusiasm, if I may judge from the literature, greeted the alleged bacteria in poison ivy forty years earlier. In one of the great speeches of all time, Paul, standing on Mars Hill, is said to have addressed an audience "who spent their time in nothing else but either to tell or to hear some new things." This is a condition not wholly foreign to other audiences in other times, even American botanical audiences.

The danger in this sort of publication is, however, more apparent than real. A wise and tolerant botanical public, realizing that these great investigators were acting under the impulse of "fad psychology," soon forgets these slips, and the careful reader of tomorrow, noticing in the pathological literature of 1915 to 1925 an exceedingly large number of reports of virus diseases, will recall that virus diseases were all the rage in those days and will take these reports with more than a grain of salt.

Just as a man in good general health who consulted a physician between 1910 and 1920 was predestined to be diagnosed as having appendicitis, so a plant which showed any unusual abnormality between 1915 and 1925 was sure to be under suspicion of having some mosaic disease. Five years ago we were busily studying two diseases of strawberries which we regarded as of virus origin. One has since turned out to be caused by nematodes and the other is apparently a genetic variation.

The Advantages of the Fad

A year ago I was convinced that fads, at least botanical fads, were an almost unmitigated nuisance. Somewhat more mature reflection, however, serves to convince me of the contrary. I now regard them with a toleration which approaches enthusiasm.

Something like a fad may be necessary to jar the human mind, even the botanical mind, from its old moorings. To cast out the devil of one idea from the botanical mind is often a decided advance. It may be that occasionally, when the devil of one idea is cast out he will return, and finding the botanical house swept and garnished, take seven other spirits worse than himself and enter in and dwell—which might appear worse. On the other hand, it may really be better, and at any rate they will not stay. New fads are often better than the old. I am glad, for example, that the fad for chewing tobacco has been replaced by the cigarette fad. I view with something very like dismay the possibilities if the ladies had all taken up tobacco during those earlier days. With almost equal dismay I view the possibilities if changes in American botanical interests had ceased at, for example, the embryo sac stage.

It may well be that the fad offers the only way to really introduce a new concept into the botanical world. By this I do not mean merely to get the idea into literature, but to get in into botanical thinking.

In his "Leaven of Science" Sir William Osler cites the following story, told by Sir Robert Christian, about Barclay, one of the leading anatomists of the early part of the nineteenth century. Barclay spoke to his class as follows:

Gentlemen, while carrying on your work in the dissecting room, beware of making anatomical discoveries; and above all beware of rushing with them into print. Our precursors have left us little to discover. You may, perhaps, fall in with a supernumerary muscle or tendon, a slight deviation or branchlet of an artery, or, perhaps a minute stray twig of a nerve—that will be all. But beware! Publish the fact, and ten chances to one you will have it shown that you have been forestalled long ago. Anatomy may be likened to a harvest field. First come the reapers, who, entering upon untrodden ground, cut down great stores of corn from all sides of them. These are the early anatomists of modern Europe, such as Vesalius, Fallopius, Malpighi and Harvey. Then come the gleaners, who gather up ears enough from the bare ridges to make a few loaves of bread. Such were the anatomists of last century—Valsalva, Cotunnius, Haller, Vicq d'Azyr, Camper, Hunter and the two Monroes. Last of all come the geese, who still contrive to pick up a few scattered grains here and there among the stubble, and waddle home in the evening, poor things, cackling with joy because of their success. Gentlemen, we are the geese.

Osler's comment on this story is:

Yes, geese they were, gleaning amid the stubble of a restricted field, when the broad acres of biology were open before them. Those were the days when anatomy meant a knowledge of the human frame alone; and yet the way had been opened to the larger view by the work of John Hunter, whose comprehensive mind grasped as proper subjects of study for the anatomist all the manifestations of life in order and disorder.

To Osler's comment I beg leave to add that probably only by strength of interest in various fads were the geese called away from their gleanings and but for the widening of interest induced by fads they and their successors might well have remained in the stubble.

OBITUARY

WICKLIFFE ROSE[1]

1862–1931

WICKLIFFE ROSE was educated in his native state of Tennessee and at the University of Chicago. The scope of his intellectual interests was manifested early and changed only in outward appearance as time and circumstances carried him into unfamiliar and unexpected fields. It is worth noting that his first teaching position was in history and mathematics—subjects which, while disparate in content, were yet both congenial to his mind. It was in philosophy, however, that he found his real vocation. He filled chairs of philosophy and the philosophy of education for more than ten years at Peabody College and the University of Nashville. Never afterwards did he lose the general and analytical point of view acquired in those formative years. In all his subsequent, varied activities he looked instinctively not only into, but around, his problems.

During this period his recognized talents, executive ability and devotion to education led to his selection as dean of Peabody College and the University of Nashville, as agent of the Peabody Fund and trustee of the John F. Slater Fund, both the latter appointments having to do with furtherance of education in the South.

It was doubtless while administering these funds that Dr. Wallace Buttrick, president of the General Education Board, came to know Dr. Rose and to appreciate his gifts. Thus it resulted that Dr. Rose was chosen in 1910 to be the director of the Rockefeller Sanitary Commission for the Eradication of Hookworm in the South, an undertaking which started him on the amazing career for the betterment of health and the upbuilding of science that was to assume world-wide dimensions.

It was not without trepidation that Dr. Rose entered upon the anti-hookworm campaign. The field seemed far removed from philosophy and education. He gave the opportunity minute thought and consideration, and it may well be believed that his natural humanitarian impulses and love of his country contributed to the affirmative decision. A wakeful night, it is said, brought conviction and yielded also a plan of operations. As many of us know the dangers and pitfalls of midnight vigils, it is proper to state that Dr. Rose's visions when tested proved to be realizable. It is an historical fact that the methods he put into practice at the outset later called for little modification even when applied on an international scale.

There was a critical moment in the year 1910 when Dr. Rose might have been lost to the great career

[1] Read at the meeting of the National Academy of Sciences, Washington, D. C., April 26, 1932, at the posthumous award on Dr. Wickliffe Rose of the Marcellus Hartley medal for eminence in the application of science to the public welfare.