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FORTY YEARS OF PLANT PHYSIOLOGY

SOME GENERAL IMPRESSIONS[1]

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FORTY years ago the work in plant physiology was changing from the old to the new. Those who were interested in the subject were concerned chiefly with the nature of the response of *Mimosa* or similar plants to stimuli of various sorts. In the main, they were not interested at all in any practical or applicable results that might accrue from their investigations. At about the time of my entrance into the field, the conflict between the purist and the practical man was at its height and was being waged bitterly. It is said that some purist when asked of what practical value his findings were in the field of science, replied, "None whatsoever and if I had thought before undertaking the work that they would be of any practical value, I would never have undertaken the investigation." Such a happening may be somewhat exaggerated, but it, nevertheless, illustrates the state of mind of some of the individuals who waged this bitter conflict. This condition illustrates the same spirit that was expressed by the so-called "malefactors of great wealth" who are reputed to have said that "the public could be damned" as far as they were concerned.

The public, rightly or wrongly, may eventually reach the stage where the workers not only in plant physiology, but also in most other lines of scientific work, must show that the results of their labors will contribute to the happiness or advancement of man-

[1] Address of the retiring president of the American Society of Plant Physiologists, December, 1942.

Contribution No. 443, Department of Botany, Kansas Agricultural Experiment Station.

kind before it will consent to grant the request of these workers for pecuniary aid in pursuing any investigation. This reaction of the public has an influence that is felt even in the privately endowed institutions. This attitude of the public may be wrong, but right or wrong, it exists and any one interested in research must reckon with it. The practical aspect now dominates investigational work in all regards and the so-called purist, especially in plant physiology, is now prominent only on account of his absence. Now we say that it was foolish to fuss over such a question for no one can tell when a scientific discovery even of the purist type may become of the utmost practical importance. Let us illustrate this fact from experience.

In 1924 I published a paper in the *Journal of Agricultural Research* entitled, "Daily Fluctuations of the Carbohydrates in the Leaves of Corn and Sorghums." No practical importance was attached to the results at that time and they were believed to have none. The facts were observed and it was considered worth while to give them to the world. Within five years after the appearance of this article, the Dairy Department at Kansas State College noted that silage varied markedly in its acidity. Those who were investigating this problem were worried greatly because they could find no cause for this difference in reaction. Some one in the Dairy Department, however, had read the paper just mentioned and wondered if the amount of carbohydrate in the sorghum plant at the time it was cut might not have some influence upon the acidity of silage. He called the attention of the investigators to the paper and to his surmise relative to the acidity. Further study proved that the time of day at which the plant was cut did have an influence upon the acidity of the silage. A purely scientific discovery thus became a practical one.

Forty years ago the work on the composition and action of enzymes dominated the field of plant physiology. We fully believed that the riddle of the universe would be solved when the nature of their composition and action was discovered. The biological workers have since found and proved many facts relative to the structure and mode of action of enzymes, but the ultimate cause of these problems has never been explained to the satisfaction of the student of plant physiology.

After the study of enzymes there followed that of the water requirement of plants, their point of permanent wilting, drought resistance, root systems, the effect of the H-ion concentration of their cells and of the medium in which they grow, the effect of ultraviolet light on their growth and development, photoperiodism, their resistance to heat and cold, their reaction to the carbohydrate/nitrogen ratio, growth substances, hormones, vitamins and numerous and varied other studies. The investigator may be prone to consider his particular problem one of the most fundamental of those confronting mankind to-day. The solution, however, of each of these problems is yet far distant. The present status of many of these problems can be stated by using the following illustration.

During the second year of our graduate work at Yale University, an extensive publication was discussed in weekly seminars for two months or more. It was a detailed review of the research work that had been done along various allied lines. The author would survey the research that had been completed in each phase, then close with a statement that read somewhat as follows, "Much work yet remains to be done on that subject." This statement was repeated so frequently that it became trite among the graduate students and their instructors.

From the viewpoint of many of us, scientific investigators have never completely solved any problem and it is doubtful if they ever will, even though they keep investigating a problem continuously. Many times, however, they leave the problem upon which they are working to enter what appears to them to be more remunerative fields.

Scientists behave and remind us very much of the actions of youngsters of our boyhood days. The Ohio Canal went within 500 yards of my country home and the boys and girls of that neighborhood fished along it. Suppose a dozen were fishing along its placid bank within a distance of a quarter of a mile. Suddenly one would land a fish larger than those generally caught. Immediately eight or nine of the boys and girls would run to that point to fish and cast their lines into the water. There were no more big fish in that particular spot than elsewhere, but the boys and girls evidently thought so.

Scientists behave relative to their scientific work much as did these boys and girls. Whenever a fellow research worker discovers an outstanding fact, literally hundreds of workers shift their investigations to that field and try to find some phase of it in which to work. We have long noticed the behavior just mentioned and wondered as to the reason why investigators behave as they do. We asked a colleague to explain such behavior. He too had noticed the same trends and had made up his mind on the subject. He replied almost instantly, "The vast majority of scientists do not think for themselves and the discovery of a new fact which promises to be of great importance stimulates them to greater activity. The only way they can show their ability is to follow in the footsteps of the fellow who has demonstrated the ability to think." We are inclined to believe that our colleague was right in his interpretation of the general behavior of scientists. We may be wrong in our reasoning on the cause of certain fluctuations in trends of research, but most of us will agree that investigational work goes in fads or cycles just as certain styles dominate the wearing apparel of both sexes of the human species. Likewise, we are all agreed that the progress of plant physiology, as in the advancement of all scientific endeavor, is due to the resultant of the efforts of thousands of ordinary investigators and that its progress is not due to the work of any individual or even a small number of workers.

We have also observed during the past forty years that a newly discovered fact seems quite simple in all regards. It is so apparent that we are amazed that it was not discovered long ago! However, as more and more is learned on the subject, it increases in complexity until the solution of the problem which at first appeared so simple is lost beyond recognition because of the complexity of the questions raised by it. Let us consider two cases which illustrate this point. First, let us consider the problem of growth substances. The first discoverer of their action considered that he was working with a single substance. All that needed to be said was that a certain reaction was caused by a *"growth substance."* Then after the problem was further investigated it was found that there were at least three different components of the growth substances, *viz.*, auxin *a*, auxin *b* and heterauxin or indole 3 acetic acid. Then investigators showed that there is a long and varied list of organic compounds that may produce the same effects as these growth substances. Whether the plant secretes all these substances is so far unknown. However that may be, it is certain that when we say "growth substance" at the present time, we must qualify and state the particular substance to which we refer. Thus we have an illustration of a subject that has become more and more complex as investigations have progressed.

The same situation could also be illustrated by considering the discovery of the carbohydrate/nitrogen ratio. This expression was considered a relatively simple one when it was first discussed, but as investigations proceeded, it became more and more complex. The question immediately arose as to the meaning of the expression. To some it meant the total carbohydrate/nitrogen ratio, to others the carbohydrate/insoluble nitrogen ratio, yet others believed that the starch/nitrogen ratio was the one to consider, while others felt that the total carbon/total nitrogen ratio was the important one. This disagreement of the exact ratio to be considered, coupled with the numerous ways by which it might be varied, together with the impossibility of attaining the desired ends because of conditions over which no one has any control, makes it one of the most complex problems for application.

The author has seen much water pass under the bridge in regard to the many problems left unsolved by those working on them. These problems have been deserted because the individual has become disgusted with his lack of progress or has entered new fields of research that seemed more promising. Alas, in most instances he soon finds that he has been chasing a will-o-the-wisp and his new love proves just as cold and unresponsive as the one he deserted. We thus refuse to be excited to any degree by a discovery in plant physiology, although the new discovery is blazoned to revolutionize the efforts of mankind. We are thus frequently the cause of considerable alarm to our young colleagues who have not been through the mill and are not veterans in the service.

We have been impressed with the fact that no problem has ever been totally solved. This point is illustrated by considering the history of the elements that are essential to plant life. The number of elements was formerly designated as ten. This fact was determined and considered settled over fifty years ago. It was so considered because it had been thoroughly proved by experimental methods. The problem, however, was not destined to remain in quiescence for all time. It was later discovered that the investigators had made numerous and varied mistakes in their procedure which induced serious errors in their results. A new series of investigations was then begun on a problem that had long been considered solved. As a result there has been more research work during the past ten years on nutrient solutions, primarily with the idea of determining the elements that are essential to plants, than in any other field of plant physiology save that of growth substances. The results have been most gratifying and, so far, the number of essential elements has been increased from 10 to 12. Several others are yet in dispute. These results should convince all scientific workers of the danger or folly of considering any problem fully solved.

We are convinced from our experience that to be a good investigator, the individual must be in that type of work because he is "born that way." By that we mean that he must be a person who, because of his temperament, likes the investigational type of work and is happy doing it. A good research worker, with but few exceptions, must be patient, a plodder and an individualist with a one-track mind. He must hew to the line and let the chips fall where they may. He must consider that the problem on which he is working, no matter how small it may seem to the average person, is the all-important one and that all others are more or less subsidiary to this one. Thus many scientific investigators could well be classed by their neighbors and compatriots as narrow folks who know little and care less about general affairs. That is perhaps the chief cause for their being irritable and prone to call a fellow worker names because he has obtained results that differ from theirs under what appear to be similar conditions. This may be illustrated by the well-known work of Eckerson in 1914 on the permeability of the protoplasmic membrane to solutes. She found that the permeability of the cells of roots depended on three main factors which may be listed as follows: (a) on the temperature range considered, (b) upon the type of plasmolyzing agent used, and (c) upon the type of plant.

Let us consider the case of the cells of the radish root, using a solution of potassium nitrate as the plasmolytic agent. She found that from a 10° to 14° C. range of temperature, the permeability of the protoplasm of these cells increased with a rise of temperature; that from 18° to 24° C. the protoplasm of these cells showed no change in its permeability with a fluctuation in temperature; from 24° to 40° C., the permeability of the protoplasm of the cells of this root increased with a rise in temperature; but from 40° to 50° C., the permeability of this protoplasm decreased.

Now suppose one investigator desired to study the influence of temperature on the changes in permeability of the protoplasmic membrane to solutes. Suppose he would choose the radish root and potassium nitrate as the plasmolytic agent and would choose as his range of temperature 18° to 24° C. He would find that the permeability of the protoplasmic membrane would not change over that bloc of temperatures with an increase or decrease in their value. Let us suppose that another investigator would choose the same plant, the same plasmolytic agent, but would choose for the range of temperature 40° to 50° C. He would find that an increase in temperature in that range would decrease the permeability of the protoplasmic membrane to solutes, while a decrease in temperature would increase the permeability. A third investigator might choose the same plant, the same plasmolytic agent, yet choose the range of temperature from 24° to 40° C. in which to work. Inside such a bloc of temperatures he would find that the permeability of the protoplasmic membrane to solutes would increase. Thus under identical conditions except temperature, each investigator would obtain different results. They would all be right. In times past each would have considered the other a fraud or a liar and would not have hesitated to say so publicly.

Privately, we can forgive a research man for speaking hastily because, as previously mentioned, his narrow attitude is one of the characteristics of a good investigator. The public, however, will not be so liberal as to pardon such behavior and will not tolerate such an attitude. We are becoming, whether we like it or not, more and more the servants of the public which is exercising increasing demands on all who in any way may be subject to its will. It looks to its servants for cooperation and leadership. The public in the future will not tolerate any antagonism between research workers. It will see to it that workers in research are congenial to all, including fellow workers. Whether this requirement is better than the old is a moot question in many regards, but right or wrong, the new method is the one that is in the saddle.

We often recall the experience of a former colleague who went to Europe many years ago for advanced study. He planned to "sit in on" the lectures of several noted men in agriculture because he knew professionally of the excellence of their work along special lines. When he arrived abroad he talked with these various professors relative to taking their lectures and all said they would be delighted to have him. Towards the end of his rounds in asking the various professors relative to sitting in on their lectures, he happened innocently and inadvertently to mention that he was also going to take the lectures of Professor X. Instantly the scientist to whom he was talking changed his cordial attitude and bluntly said, "If you listen to the lectures of that man, I will have nothing whatsoever to do with you." The American was nonplussed and had reasons to be so. Practically all American scientists would not hesitate to agree that such an attitude was most damnable. Yet in this country we have attained a similar attitude in many of our institutions of higher learning.

A graduate student who goes to some institution of higher learning soon belongs or is told bluntly that he belongs to such and such a clique. He soon learns that he can not even talk in the hallways to the leader of any other clique or even any of the followers of this man because if he does so, even in the most perfunctory manner, he is immediately marked by a member or the leader of an opposing clique as belonging to the group that opposes them. Such a condition is deplorable, yet it exists to-day in numerous of the large and leading educational institutions of our land. The average graduate student is greatly distressed and bewildered at the whole affair and rightly so. He has been taught that one of the main objectives of an education is to attain a more tolerant attitude towards all subjects and toward all people. He can not leave such ideals even though in scientific training we teach him to know more and more about less and less. To our way of thinking the great keystone in the advancement of science is tolerance to the views of others whether we agree with these views or not.

For almost thirty-five years the writer has been an instructor at Kansas State College and an investigator in the Kansas Agricultural Experiment Station.

That my colleagues and myself within that time have accomplished a few things relative to the teaching of plant physiology and along the line of physiological investigations, we believe no one will deny. We can say truthfully and in all sincerity that no rivalry whatsoever relative to the subject-matter to be investigated exists between the various departments of this institution. We talk without restraint to various members of other departments and ask their advice about work that we are doing. They in turn are free to consult us relative to matters in which they are engaged.

Recently one of our students who had finished his undergraduate work at this institution went to a neighboring university to see about taking graduate work leading to the doctorate. He found, to his consternation, that the botany and the chemistry departments of the institution were at swords' points because each felt that certain members of the other department were transgressing upon their sacred domain. This graduate student came back to our institution a sadder but wiser man. He had not realized until that time that such bitter rivalries exist within educational institutions.

There is unquestionably sufficient truth in any field to satisfy the most arduous workers of all parties. The liquor of our own vintage unquestionably is good but if we should mix the drinks from all sources, we would truly have the real nectar of the gods. We are not a prophet or the son of a prophet, but we predict that unless we quit fussing over tweedledum and tweedledee, the fate of our investigations is sealed, for the public that furnishes us the funds with which to conduct our research will withhold them from those who can not conduct investigations in a cooperative way.

Another fact that has been impressed upon me during forty years of experience is that one may know all about a certain subject and yet be a miserable failure as a teacher. He may be considered a "nut" and no more by his fellow men. We are convinced that to succeed as a teacher, not only in plant physiology, but in any field, two prerequisites are necessary. (a) He must know his subject and keep informed concerning its progress at all times, but (b) he should have common sense. We have frequently been taken to task because we have preached and advocated the last-named characteristics as a prerequisite of success. So far, we have been subjected to no arguments that have in any way whatsoever changed our idea relative to the matter. We would list under the term "common sense" three main factors that we believe make up the meaning of that word: A sense of humor, a knowledge of human nature and a trustfulness in humanity. If any individual possesses these three characteristics to a marked degree and if he, in addition, has a thorough knowledge of his subject, he will succeed as a teacher in his chosen field.

The observations and suggestions that have been listed in this paper are a few that have impressed us in our experience in the field of plant physiology during our forty years of experience in that realm. They are not new thoughts and they have been preached by many from the time that investigational work had its origin. We are convinced that the impressions gathered during our years of experience, if followed even to a limited extent, will benefit both the investigator and the field of plant physiology.

A NEW BLOOD-CLOTTING THEORY

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THE difficult subject of blood coagulation has been so beset by inadequacies of the numerous and often conflicting theories, in the light of experimental fact, that the author has labored for nearly a decade with an experimental approach and great hesitancy in propounding a comprehensive theory. Despite the complexities encountered and many "loose-ends" still to be brought into line, current interest in the preparation of plasma and hemostatics for war use makes the time ripe for presentation of a "working hypothesis" for the stimulation of continued research and, particularly, for the guidance of the many whose interest is at present confronted by the sad lack of agreement among so-called "experts" in the field. The experimental basis for the views here presented is to be found in the author's contributions and reviews, in such coagulation reviews as those in *Ergebnisse der Physiologie* (Morawitz, 1905; Wöhlisch, 1929; 1940), in the chapters on proteolytic enzymes and "thrombase" in Oppenheimer's "Die Fermente und ihre Wirkungen" and in Northrop's "Crystalline Enzymes."

It has long been agreed that the essential feature of blood coagulation is the (specific) conversion of a plasma protein fraction (fibrinogen) from the state of a colloidal hydrosol to that of the corresponding quasicrystalline (oriented micelle) gel (fibrin), and that this is the second of a two-phase process, the first being the elaboration of an essential agent (thrombin) from an inactive, probably protein, pre-