RICHARDS — BOTANY

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HERBERT MAULE RICHARDS

PROFESSOR OF BOTANY
COLUMBIA UNIVERSITY

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WHAT is the content and scope of the science of botany? Popular opinion will answer somewhat easily: Botany consists in the gathering of plants, and the dismembering of them, in connection with the use of a complicated terminology. That is the beginning and end of botany as it is understood by the majority; there is nothing more to be said. In consequence, the employment of the botanist seems so trivial, so very remote from important human interests that no second thought is given to it. The conception formed in ignorance is continued in ignorance. Even the zoologist is at an advantage, for the public is finally forced to admit that it does not know what he is about, while it understands the botanist very well. He is quite hopeless, for, while flowers may be pretty things to pick, they should not be pulled to pieces, and if he does not happen to be interested in dissecting flowers he is not a botanist but simply a fraud.

Far from being remote, the study of plants comes very close to human interests. One has but to stop to think that plants are the great energy source for man himself and the animals upon which his well-being depends, to recognize that a careful study of their manner of life, the conditions which favor or hinder their growth is of the very first importance. Besides this, human curiosity demands that plants be investigated, if for no other reason than that they must be made to yield answers to the perpetual questions that man is asking regarding the world about him.

Under botany we have to consider all the questions as to the form, the functions, the classification and the distribution of those organisms that are called plants. Along what lines this study is prosecuted, how it is related to other fields of intellectual activity, and some specific instances of its problems and the manner in which they may be solved is what I shall attempt to tell you.

It would be out of place in a talk like this to devote too much time to a consideration of the historical side of the subject, and therefore only a few of the important movements can be pointed out. Any folk which had so far emerged from the stage of savagery as to stop to notice the world about it would perforce pay some attention to plants. A discrimination of the medicinal uses of plants is often noticeable even in primitive peoples, and with such observation goes also the discrimination of difference in form, the prototype of morphological research. I have seen a Malay coolie who could distinguish seven forms of tropical oaks where the botanist recognizes only four, an evidence that sharp observation is not confined to the highly developed races.

In our own civilization, we can trace back the history of botany to Aristotle, who affords us some record of the plant forms known at his time, though the influence which his philosophy wielded, even down to the middle of the last century, was of vastly greater importance than any contribution which he made to botany itself. Theophrastus gave a fuller account of plants, and later came the inquiring and ever curious Pliny. Dioscorides, however, in the first or second century of our era, was one of the first to investigate plants with any attempt at thoroughness even from the standpoint of the knowledge of the time. As is shown especially by Dioscorides' work, the study of plants was largely from their use as drugs, and they were described simply to facilitate their recognition. Any real knowledge of them was naturally meager, and false ideas that clung for a long time, some until comparatively recently, prevented any proper conception of form and function.

As would be expected the contributions become of less and less value as we approach the middle ages, the botanical writings of which time were full of the wildest fantasy and superstition. The efforts of this period need not arrest our attention.

In the sixteenth century in northern Europe, particularly Germany, there was a movement towards the real study of plants from the plants themselves as evidenced by the works of the herbalists, but no attempt at classification was made. Here there was an attempt at the enumeration and illustration of plants from living specimens, and confused and empirical as this work was, it was actuated by an honest endeavor to record, as accurately as possible, actual forms, and not fanciful abstractions which never did and never could have existed. All the descriptions were detached from one another and little or no attempt was made at classification, though by the repeated study of many similar forms the idea of natural relationship began to dawn in a vague way. The actual purpose of all this plant study was the recording of the officinal plants, for special knowledge of plants was still confined to their uses in medicine.

While this movement was advancing in northern Europe, a mainly artificial system of classification was developing in Italy and found its culmination in the work of Caesalpino, who strongly influenced the progress of botany, even after his own time and into the middle of the eighteenth century. Great as was the advance he made, it would have been far greater had it been given him to break away from the scholastic philosophy which hampered him. We find a curious mixture of a modern spirit of inductive natural science and Aristotelian methods of thought. The latter triumphed in the main, and the result was a formal classification built on idealistic abstractions that is wholly fallacious from our standpoint of to-day.

Emerging from such conditions we find Linnaeus—the bicentenary of whose birth was celebrated last year—and though he too was much influenced by the earlier writers, to him belongs the credit of the emphasis on the fact that some natural system of the classification of plants must exist even though he could not determine it. Linnaeus is popularly termed the father of botany and of zoology as well, and in many senses there is reason for it. He was a born classifier and brought considerable order out of immense chaos, but still his classification was artificial, and only to a very limited degree recognized the natural relationships of plant forms. Linnaeus, however, was wise enough to recognize its artificiality.

From Linnaeus the advance was more rapid, and, while most of the study in plants centered on the work of classification, there were unmistakable signs of other interests. The ideas of the classifier were still hampered by the dogma of the constancy of species, which continually clashed with the insistent and undeniable evidences of the genetic relationships of organic forms. Despite the movement in favor of the idea of the development of species from previously existing forms, despite the views advanced by Lamarck and others at about that time, despite, indeed, the more strictly botanical investigations in the morphological field which were brought forward during the first half of the nineteenth century: despite all these things, the botanist was unable to break away from the concept of groups of plants as abstract ideas. It was not until 1859 that the publication of Darwin's "Origin of Species" drove biologists to a different point of view. Then the rational idea of the evolution of organic forms explained in a similar rational fashion the observed genetic relationships of groups of plants. No longer did the classifier hesitatingly admit the possibility of the evolution of species and deny that of genera and higher groups, no longer did he maintain his artificial groups, which had no more relation to each other than successive throws of dice, but he admitted the whole great scheme implied by the evolution of organic forms from pre-existing types.

Naturally, it is difficult to point out at just what time the modern trend of botanical work found its origin, but one can say, in a general way, that it was about the middle of the nineteenth century, although of the two criteria of progress to which I shall refer, one dates about a decade before, the other about a decade after that time. The establishment by the botanist Schleiden in 1838, and by the zoologist Schwann in 1839, of the real nature of the cell, and the acceptance of what may be termed the cell doctrine, at once made possible the development of the study of form and structure, both as to adult and as to embryonic organs. With improved optical apparatus and with improved technical methods, many able students added a vast number of demonstrated facts to the general store of knowledge; in fact, for a time the additions to morphological information very much outran the development of the physiological side, though the latter had had a rational beginning at a prior date. The morphological development depended in the first instance upon the understanding that the cell with its living protoplast, and usually with a wall, constituted a not further divisible morphological unit of living organisms; that every cell must have arisen from a pre-existing one; and finally, that all but the lower organisms are composed of thousands of these cells differentiated into distinct tissues. One of the most important figures in this advance of botany from Schleiden's time was Naegeli, who brought to bear a powerful intellect on many of the fundamental concepts both of morphology and physiology. Of the many questions dealt with by him, that of the ultimate structure of organized substance was perhaps the most far-reaching; and today, despite its limitations, his Micellar Hypothesis, is the most stimulating of any of the theories which have been developed regarding this subject.

The other milestone of progress was Darwin's "Origin of Species" already referred to. Entirely aside from the particular question involved in that work, its importance lies in the fact that it fought the battle and won the victory for the inductive method of reasoning as applied to biological science. Previous to the awakening of botany, due to these and related causes, a botanist usually covered the whole field of his science and had the right to consider himself a specialist in all branches of botany. The rapid accumulation of facts soon demanded, however, a segregation of different lines of work. Thus arose the divisions of botanical activity, which, for our purposes, may be classed under three heads. First, the taxonomic, or as more commonly called the systematic side, which has to do with the classification, mainly as established by gross morphology. Second, the morphological field which concerns itself with the outward and inward form and structure and the development thereof, which may or may not have direct relation with taxonomic work. Third, there is the domain of physiology which treats of function. As Professor Wilson has pointed out, there are really but two divisions of biological work, the morphological and the physiological, so that the separation of taxonomy which really belongs in the first division is rather artificial. The separation however is necessary for many reasons, among which are the fact that the temper of mind and the methods of the workers in the two divisions are quite different.

It is perhaps the tendency of the time, at least in many quarters, to underestimate the value of taxonomic research and this is to be regretted since in classification we have the foundations of other branches of work. Entirely aside from the philosophical value of a well ordered classification, it is an absolute necessity for a starting point of morphology and physiology to have the different species of plants recorded in recognizable form, and, in consequence, to have a classification. It would undoubtedly be a great advantage could organisms be classified as are chemical compounds or could be located as the astronomers locate the stars and in the same definite and precise manner. Such is hardly possible when we reflect that the question of the identity of an organism must, even under favorable conditions, be somewhat a matter of opinion as well as of demonstrated fact. Despite such limitations of taxonomy, in most of the really important questions opinion is fairly universal, so that our classification is not developed simply at the whim of any one investigator. Taxonomy, however, as soon as it is considered an end in itself sinks at once to the level of mere cataloguing or, worse still, loses itself in the mazes of nomenclatorial controversy. It must be considered in its relation to the problems of plant distribution, of the evolution of new forms, of its philosophical intent, if it is to retain its vitality.

I have spoken of artificial classifications in connection with the work of earlier botanists. How then does the natural classification as understood today differ? Primarily, it differs in the admission of genetic relationship of forms, a thing not conceived of by older writers. A natural classification implies higher and lower forms, connected by intermediate ones in all stages of differentiation. However, it does not imply that all these forms exist today, nor does it imply that they developed in a single continuous series from the lowest to the highest. We have no particular right to suppose that all plants can be traced back to a single ancestor, indeed the evidence is against it. There is no reason why several phyla, or lines of ascent, may not have originated, perhaps simultaneously, from the most primitive form of living protoplasm. The story of the lower aquatic forms certainly indicates this possibility. Of these lower phyla some stopped short, some went on, which ones is a matter to be definitely settled. A good instance, though a somewhat special one, to illustrate the fallacy of the assumption of a single line of relationship, is found among the fungi, the chlorophylless lower forms. Many ingenious authors have attempted to unite them in a single continuous series, when every evidence we now have points to their having originated at several places from the green plants. Who, indeed, would care to deny that new phyla might be originating today? Any concept of evolution demands such a possibility; organisms are more plastic than the average person conceives, even in this age.

The object of a natural classification is to consider all the many plant forms, to determine by such marks of genetic relationship as we can discover their place in the series, where they have departed from the main stem and in how far they may have had a line of development of their own. Despite what I have said about the lower phyla, it is not improbable that the higher plants can be traced back to some single source, not that it is to be believed for a moment that this ancestor exists today. Living ferns or mosses are no more to be considered the direct ancestors of the flowering plants than are monkeys to be considered the direct ancestors of man.

The establishment of our classification today might be compared to the putting together of a puzzle map some parts of which are lost; we can determine how many of the parts fit together, and, by analogy, can tell something of the missing ones. The whole method depends on the admission of genetic relationship, a concept that is built up partly by the study of adult structure, partly by the story of the developmental stages, partly, though in botany less than in zoology, by the evidence of paleontology, but more vividly than in any other way by the actual behavior of certain plants in the matter of giving rise to new forms. This last consideration is of such great importance that we shall come back to it later.

One type of morphological investigation has to do with the study of life histories of plants,—the whole life story from egg to egg again—and here we find the morphologist in close relation with the systematist, for upon the results of such researches must largely depend the understanding of the relationships of the great groups. The morphologist who devotes his time to the study of life histories is engaged in the work of tracing the race history of plants from the comparison of the individual development of more or less nearly related forms. Thus the homologies which have been traced among the flowering plants and their nearest allies among the ferns and other forms indicate to us the probable race history of these groups. It is true that the beginning of this work dates back some decades, but it is still, to a large extent, an open field, and numerous investigators are actively prosecuting research along these lines. For example, the alternation of a sexual and nonsexual generation of plants which has long been known as characteristic of the life histories of higher forms has recently been established among the lower groups, and thus a much clearer view of the whole series of the plant kingdom is being obtained.

Somewhat separated, and to a large extent needlessly so, is the work of the plant anatomist and histologist.

Formerly pursued from the standpoint of the mere topographical relation of the parts, the conception of the plant as an organism with interrelated and interdependent tissues began to fall into abeyance, until a new point of view has within recent times revivified a somewhat barren field. This point of view is the physiological one, the correlation of structure and function. Here the student of gross morphology and the anatomist unite in a physiological interpretation of the form and structure of plant organs, from which has grown the study of experimental morphology. Advance in this direction has been considerable, and we have now a much clearer idea of the nature and development of plant organs; or at least, we have a much better attitude in the interpretation of the facts that have been established regarding these matters. The danger which lies in this attitude is the well known one of teleological reasoning, and consequently it behooves us to have some caution in accepting, without thorough evidence, the interpretations which may be made of the relation of form and function and of special adaptations for special purposes. As some one has written, "so many things may be true and so few things really are in the matter of use of special organs," that we must demand above all things experimental evidence before we can accept as conclusively proved any statement as to function. It is permissible to say without such proof that such and such an explanation is plausible, but beyond that is uncertain ground and mere assertion shows a temerity at once magnificent and pitiable. On the other hand, it is questionable if the extreme attitude of iconoclasm as to long established interpretations is necessarily a wholly reasonable one. Destructive criticism is not difficult, and unless some new and better interpretation is suggested the advance in a scientific sense is not considerable.

A further development from this physiological attitude is a branch of biological work known as ecology, a study of the relation and adaptation of single plants or whole communities of plants to their environment and to each other. It is the application in a broad and more philosophical way of the methods of the physiological anatomist coupled with those of the taxonomist; but, in addition, the work of the botanist touches the field of the physiographer and geologist. Ecology is the endeavor to uncover the plan of nature as it governs the relations of the different plant forms in a given area, to understand the why and the wherefore of the association of very different forms in one locality. The keynote of the philosophical development of this topic rests on the conception of the constant struggle of individuals or groups of individuals to maintain themselves against other forms, which leads to a balanced relation of the different species in a given flora. Understanding this, we can see why if this balance is disturbed the whole fabric of a plant community may be destroyed and a flora swept away. We are also able to understand how relatively slight climatic changes may alter completely the character of a vegetation in a given region, and thus to comprehend more readily the changes which must have taken place in past ages. It also shows us the effect of present changes, particularly in regard to the destruction by man of the essential elements of natural plant communities, notably one of the most important of these, the forests. Its use lies in these directions and the danger of its misuse lies in the direction of drawing too positive conclusions from data which are insufficient, and of accepting the results obtained as necessarily final, a common error it is true in any line of thought, but one to which the ecologist has especial temptation.

It is in the field of physiology more than anywhere else, perhaps, that the worker must humble himself before the immensity of the problems before him; that he must realize how fragmentary is the most advanced knowledge of this subject. The foundation stone of physiology is chemistry, and consequently its advance must go hand in hand with the advance of that science; but there is also, it must be admitted, the element of empiricism, which is an unfortunate necessity in any branch of learning where any considerable mass of facts are not yet correlated. The greatest advances are made in the direction of resolving this empirical information into more compact and definite form, a task only possible by the accumulation and correlation of great masses of data in connection with the more definite information afforded by chemistry or physics and more particularly modern physical chemistry. It is plain, then, that we can never go ahead of the data afforded by these sciences, but must always follow somewhat behind them. It must not be supposed, however, that physiology is in a nebulous condition, despite the fact that we are but on the margin of the unknown. Distinct and creditable advances have been made since the days when the knowledge of plant morphology and the chemistry of Lavoisier made possible any reasonably satisfactory explanation of the functions of plant organs. The establishment of a proper understanding of how the plant obtains its food has been a matter of the utmost importance, both from the development of theoretical physiology, and from the standpoint of practical use. We know not only the definite chemical elements which are essential for plant life, but we know also the quantity and form in which they are most favorable for plant growth. Having established this, it is possible to understand the rôle of plants in the general economy of the world, and how their manner of life, in a broad sense, supplements that of animals. There is also pretty definite information as to the physical phenomena connected with the absorption of the raw food materials which the plant afterwards elaborates, information which is largely due to the classic researches of Pfeffer, whose work, it may be remarked, also afforded Van t' Hoff valuable data for his contributions to the establishment of the modern physical chemistry. Application of the laws of diffusion and of osmosis, as shown by Pfeffer, enables us to understand why a plant may absorb more of one mineral salt than of another, though both be presented to it in solutions of equal concentration; why it cannot absorb some substances at all, while on the other hand it cannot avoid absorbing certain substances, even though they be violent poison and kill the protoplasm of the absorbing cell at once. We understand also a good deal of the mechanism of the production from simple inorganic substances of the first organic food by the green plant, the first organic food of the whole organic world. While, as will be shown later, the precise details of this process are not fully understood, the general facts are a matter of almost common information, so well known that I hesitate to speak of it here, though to sum up the matter in a few words it may be said that this process of photosynthetic activity of green plants is carried on by the living cells in the presence of sunlight, through the agency of the green coloring matter—chlorophyll—which is present in the leaves, and that the chemical reaction involved results in the union of the carbon dioxide absorbed from the air, with water absorbed from the soil, to form the first simple carbohydrate that is to be detected in easily recognizable form as starch. The fact that this process takes place does not interfere with the operation of another one, namely the absorption of oxygen with the giving forth of carbon dioxide, that is concerned in the mechanism of respiration. Respiration as a means of releasing the stored energy in available form for the constructive work of the organism is as necessary in plants as it is in animals. These four fundamental questions, namely, the inorganic substances required by plants, the manner of their absorption, the manufacture of the first organic food, and the nature of respiration are perhaps the most important physiological facts, in the field of nutrition at least, which have been definitely established, and from any point of view their importance is a far reaching one.

In the other great field of physiological research, the study of the mechanism of growth and change of form, much information, made possible by the proper understanding of the cellular character of all living organisms, has established many facts as to the relation of plants to the great physical forces which govern the conditions, the rate and the direction of their growth. This is the study of the dynamics of plants, of when and how the energy released by the nutritive functions is applied to the upbuilding of new tissue and the movement of plant organs. Besides the questions concerned in the influence of diffusely exerted external factors, there are also the effects produced by these same forces when the stimulus is unequal or one-sided. The latter conditions result in characteristic growth curvatures or tropisms, which continue until the plant organ by its own action is brought once more into a state of equilibrium with the external forces. In short, the various plant organs are attuned to the normal conditions of equilibrium under which they grow, and have the ability to perceive and, to a limited extent, to transmit the impulses resulting from a disturbance of that equilibrium. This brings us to the question of the sense perception of plants, manifested in a somewhat bizarre fashion in the sensitive plant, but we should go very slowly in the direction of interpreting this perception in the same terms that we do that of higher animals. It is not for an instant to be supposed that plants have any nervous system such as is characteristic of the higher animal forms. While plants can and do respond to differences in light intensity less than that which the human eye can perceive, it is gratuitous to suppose that there is anything analogous in the two processes. The possibility of any reasoning action or instinct on the part of plants is a question that the plant physiologist does not seriously entertain.

In selecting for discussion present day problems which may be considered fundamental, one is embarrassed by the wealth of material and therefore but one more or less connected series of topics which leads up to the modern mechanistic conception of life processes has been chosen. In doing so it has been necessary to ignore equally important questions which, though developed from no less a mechanistic standpoint, are more scattered.

In referring to the assimilation of carbon dioxide by green plants and the production of organic food thereby, it was necessary to admit that the details of the process are not satisfactorily known. It is evident, however, that the starch, which is the first substance that we readily recognize, is not the first substance which is formed. Modern research points more and more to the conclusion that it is the simplest of carbohydrates that is produced,—a substance known as formaldehyde. But what is especially interesting is that it seems not impossible that this primal reaction may not after all be a function of the living protoplasm, but a chemical reaction that can be carried on outside the cell through the agency of chlorophyll. It is in the further elaboration of this first substance formed that the living protoplasm is apparently necessary. At any rate we know that the energy demanded for the process must be afforded by the particular rays of sunlight which the chlorophyll absorbs.

In this photosynthetic activity of the green plant the carbohydrate supply of the world has been accounted for, but there is an equally important question not concerned in this process, namely the source for nitrogen. Nitrogen is of course an essential element for the construction of protoplasm. As is well known most plants can utilize it in simple combination with oxygen in the form of a nitrate, a sharp contrast, by the way, to the typical animal which requires it offered as an organic compound. It is also known that the same plants cannot assimilate the free nitrogen of the atmosphere, and further, in the processes of decay, free nitrogen is liberated by the breaking down of the nitrogen compounds in dead organic matter. The logical conclusion of these momentous facts is that soon all the world's supply of combined nitrogen would be exhausted,—neglecting the relatively small replenishment induced by cosmic forces—so that green plants and consequently animals, would not have the wherewithal to live, unless there were some organisms which could avail themselves directly of this inert gas. Now there are plant organisms which have the ability to assimilate the uncombined nitrogen of the air, certain bacterial forms, and it also appears some somewhat higher plants. But the operations that lead to this result are by no means satisfactorily explained, and the whole topic is one of live interest both from a theoretical as well as a practical standpoint. It should be added that from the latter point of view, a process by which a combination of nitrogen with other elements in a form that is acceptable to green plants has been devised, and bids fair to become of great importance, for combined nitrogen is the great need of the organic world.

The processes of nitrification naturally lead us to the question of the elaboration of nitrogen compounds within the cell, of the final construction of proteid material that is the actual food of the protoplasm; but here we are much in the dark, partly because we have so little real information as to the chemical structure of the more complicated nitrogenous substances. The explanations now given as to how this elaboration takes place are largely hypothetical and must be regarded as quite unsatisfactory.

A step further from the proteid food is the question of living protoplasm itself, and one of the most interesting problems connected with this is the nature and functions of the enzymes,—the ferments and digestive secretions of living cells. Many of the newer theories as to the nature of living protoplasm hark back to investigations regarding enzymes, indeed some extremists advance the opinion that the activities of the live protoplast are in themselves but the result of the interaction of substances enzymatic in their nature. There is no doubt of the power of the appropriate enzymes when present even in infinitesimal amount to cause enormous molecular changes in the substances on which they act, but it is necessary to exercise extreme caution before accepting generalizations along this line, no matter how brilliant. The amount of empirical information in this field is already becoming unwieldly, and nowhere else is the necessity of unifying principles so plainly shown. Here it is that more definite chemical knowledge may in one stroke clear up the whole situation.

If it is not possible to ascertain the chemical structure of a single enzyme, how much more difficult then must it be to determine that of the living protoplasm? It goes without saying, that if we try to analyze the living protoplasm, in the ordinary chemical sense, we kill it. This being the case, the student who is trying to penetrate these difficult problems must have recourse to other modes of attack. Therefore does he experiment with the effect of agents which do not kill but merely stimulate the organism or partially inhibit its functions and, by studying the nature and products of the reactions produced, obtain in an indirect manner clues to the real nature of life processes. The fascination of these plunges into the unknown is perhaps hardly comprehensible to those who are not engaged in the work, but all must admit the importance of the end they have in view, namely to penetrate a little further into the mystery of life. The advance in all these fields is of necessity along the line of the mechanistic conception of vital manifestations, that is, the reference of them to chemical and physical laws. To appeal to a "Vital Force," as my predecessors in these lectures have said, is to appeal to an empty name, a mere "question-begging epithet." It is obvious that if we are to make any progress at all, we must admit of the possibility of some solution that our senses can perceive, even though we are perfectly willing to admit that the final answer may never be reached. The reference of vital phenomena to a vague "Vital Force" would mean the extinction of inquiry by robbing the investigator of any sense of responsibility for adequate explanations of the results of his researches.

As you have heard in previous lectures, there is an increasing tendency on the part of biologists to segregate less sharply the physiological and morphological fields of work, to take a broader view of not only the content but also the methods of the two branches of biological investigation. It must not be supposed, however, that in this tendency towards co-operation there is a return to omniscience of the type of the old-time naturalist, who by reason of the lack of detail was able to consider himself proficient in many branches of science. The modern morphologist must still be a morphologist, and the physiologist a physiologist, only he has a broader point of view and does not hesitate to avail himself of the cognate branches of his science, or of any other science where he feels that he can further the aims of his researches; he is an eclectic and picks that which will serve to advance his work along the most fruitful lines.

Almost any investigation of wide scope is in these days an example of this improved attitude, but no other perhaps illustrates so conclusively what may be called the highest type of modern research as does the development of the Mutation Theory first propounded by De Vries. What De Vries has really done is to bring within the range of experimental proof certain questions which heretofore have been regarded as matters of observation and speculation alone. From this point, which might be said to have had its origin in the acuteness of observation of the taxonomist and morphologist, the physiological trend has ever increased until the last word in this discussion may perhaps be for the physiologist alone. The great question involved in the Mutation Theory is the old, old problem of the origin of species, a very considerable advance in which has been made by De Vries and those who were stimulated by his work. It is quite wrong to suppose that he has controverted the general results of Darwin's work; he has supplemented it, brought it within the range of more conclusive proof.

As the Linnaean or collective species may be regarded today they are usually separable into several more or less distinct strains which show no intergrading forms, and the diagnosis of any one species is, so to say, the average impression of them. To these distinct strains De Vries has given the name elementary species, and according to his interpretation they are the really discrete, finally segregable units, between which no intermediate types exist and concerning the origin of which we are really concerned. It matters not whether it was through ignorance or simply from convenience that the earlier taxonomists grouped many of these forms into a single species; we must conclude, that in general species, as recognized by the books, are quite artificial. It matters not, also, what we call these finally not further resolvable forms. Therefore let us accept De Vries' terminology and use the term elementary species; the real point of the inquiry is how did these forms arise. It is upon this that De Vries' work has thrown a great light. He has shown that they may arise suddenly and without previous preparation from preexisting forms, in which case the elementary species may be termed mutants, and the theory which has to do with the investigation of their origin the Mutation Theory.

The next task then is to examine more closely the methods which De Vries employed, the evidence which he has to support his views, both as to the observations on the origin of these mutants and their behavior after they have come into being, and further, what success subsequent investigators have had in supporting De Vries' evidence, and how far they have extended his conclusions. In the first place, it may be remarked that the conclusions as first published in 1901 and 1902 were not the outcome of any hasty experiments and ill digested data, but were the result of seventeen years of the most careful and painstaking work, and a fine example of the best kind of quiet, faithful research, removed from the rush of affairs and the demand for immediate results, the final conclusion of which fully warranted the time and labor expended.

As is well known, Professor de Vries found in Lamarck's evening primrose—*Oenothera Lamarckiana*—a plant most favorable for observation, though his conclusions are not based on that form alone. The most carefully guarded pedigree cultures were made from the true *Lamarckiana* type, and the astonishing result developed that among the offspring of these certain forms, to the number of about four per cent, showed new and striking differences. In all, more than a dozen new forms were obtained which, if they could be bred at all, bred true to their new characters and did not revert to the ancestral *Lamarckiana;* these were the mutants, the new elementary species, which had sprung suddenly in a saltatory fashion from the parent stock. The great importance lies in the fact that they were entirely constant to their new characters, and were thus not in the class of the merely unstable varieties. It must be remarked that time alone, many generations, of carefully guarded cultures in which accidental crossing was an impossibility, together with unimpeachable records, could adequately establish this momentous fact, that here was a new species, a new form, or whatever you may elect to call it, which had sprung all in one jump from its parental stock. De Vries, then, was the first man who ever saw a new type of organism come into the world and who recorded its advent.

You naturally ask how unlike were these new forms, a question which is difficult to answer without actual illustrations. However, it may be said that many of them were different enough from their parent stock to be admitted by taxonomists to come within the definition of new species, as species are regarded at the present time. The differences are not the question of mere stature, but of the whole habit of the plant and of the details of the form of both leaves and flowers. But to repeat, it really makes no odds whether the differences are of such quality that they must needs be recognized as specific by taxonomists; what is important is that they are differences which do not intergrade one with another and which are inheritable in the second, third and subsequent generations, and that no tendency to revert to the parent form is to be observed.

The results of De Vries have been verified by cultures in this country of his own and of other stock, so that there can be no question that this Lamarck's evening primrose behaves in its manner of mutation the same here as else-where. More than that, other mutating forms have been discovered, and by the application of biometric methods much that is important regarding the relative variability of mutants and their parent stock has been determined. Besides the actual experimental work, the history of Lamarck's evening primrose has been traced back for more than a century and a mass of inferential data is being accumulated which helps to support the main conclusions. Important as all these advances are, the most brilliant result is that obtained along the lines of the induction of mutations. By the injection into the developing ovary of a plant allied to Lamarck's evening primrose of reagents which might produce a chemical or osmotic effect upon the cell contents, MacDougal has actually succeeded in inducing mutations. The seed grown from the stimulated plant may produce forms quite distinct from the parent type and, what is essential, the mutations thus induced are constant to the second and third generations. That such a result can be obtained is simply astounding when one considers how firmly bound an organism is by its heredity. It would appear that a tremendous shock had been given the plant at a critical period in its life history which has enabled or forced it to break down some of the minor barriers imposed by its hereditary tendencies and to erect new ones, which circumscribe its offspring as the original ones did its parent. As to the precise nature of this shock we can at present only speculate, but it is permissible to suggest that it is perhaps of the nature of the rearrangement, in a chemical sense, of the protoplasm of the cells of the sexual generation. As to the natural production of mutants, given such a conception of the nature of the process involved, it is possible to suggest various ways in which it might have been brought about.

The line of departure of mutants from the parent type is not in any one direction, and the manner of variation appears to be wholly a matter of what we are pleased to call chance. As has been said, De Vries obtained more than a dozen different forms. Some of the mutants, we may say, are probably destined to failure, others perhaps are better placed, at least in new environment, than the parental type and might conceivably stamp it out in time. What the criteria of success or non-success may be is a matter upon which no one would care to give an opinion, but I have in mind the fact that one of the mutants of Lamarck's evening primrose has a tendency to germinate somewhat more quickly than the parent form, and the seedling grows a little more rapidly; it is conceivable that some slight advantage of this sort might be the crucial point. However that may be, it is here that we can apply the Darwinian concept of the struggle for existence, a struggle however not between single individuals, as the idea of continuous variation would imply, but the struggle between great numbers of individuals, whole groups of elementary species. The great contrast between Darwin and De Vries is the contrast between the slow and continuous accretion of variations implied by the former and the sudden jumping or saltatory variation insisted on by the latter. By such means as De Vries maintains the process of evolution might take place with far greater rapidity than by Darwin's method, for, generous as the geologists are in their allowance of time for the development of organic life on the world, it has always been difficult of conception how even the countless ages granted could compass the enormous development of the highest organic types from simple forms. To maintain that De Vries' theory is entirely complete, and must be the only means of the origin of new forms, is unnecessary. None but the extremist would go to such a length; it is not at all necessary to assume that the means to a similar end must necessarily be similar.

What may be maintained, and properly so, is that mutation constitutes one way, at least, by which new forms of organisms may arise on the world's surface. New forms, in the sense of the new combinations of old characters which come into being by reason of stable, non-reverting hybrids, are known to have originated, but such new forms imply of course the pre-existence of varied types, and do not have to do with the question of the origin of new characters.

It is not in the order of things that a new theory of such import as the Mutation Theory should not find opponents. These I think may, in the main, be grouped in three classes. First, the critics who doubt the evidence, who can be answered by referring them to the printed records, and recommending a repetition, as careful as the original work, of the experiments which have led to the new point of view. Second, those who quibble concerning terms, and this type I think constitutes the majority, who will likely suffer the fate that is usually meted out to quibblers, that of being ignored. Lastly, those opponents who, while they may not doubt the accuracy of the work, doubt the conclusions on philosophical grounds. These are the critics whom the advocate of the De Vries Theory must welcome and who will arrest his sober attention, for they will stimulate him to accumulate more and more evidence to support his position. Even were I able to analyze adequately the controversial side of the question for you, it is obvious that time scarcely allows, and I will, in consequence, state frankly that the account which I have presented is from the standpoint of an advocate of what the Mutation Theory teaches, and add that I am not aware that any experimental work has controverted it. Let me say, however, and here I wish to speak for myself alone, that I cannot see it makes great odds whether fifty years hence or five years hence we accept the Mutation Theory just as propounded by De Vries. The great point is that an advance has been made, the most important advance since the time of Darwin, by way of helping to elucidate one of the great questions in which man is interested. It is not to be supposed that we have as yet any final answer to this question, final answers are not indeed the goal of any one scientific research. It was Sir Isaac Newton, I think, who said that the seeker after ultimate causes did not show the true scientific spirit, and he was right. What we have is one of the proximate causes demonstrated to a degree which had not been previously attained. A scientific theory is like an organism, it grows and it may also propagate itself, and all the theories of evolution from Lamarck to De Vries, and those that will follow, will themselves be an example, as it were, of the principle that they teach. A theory starts life an intellectual pigmy, may develop, if it have the vitality, into a veritable intellectual colossus, and, after it has run its course, may leave behind its offspring. It is not a cause of reproach but rather of congratulation that the scientific theory of today may be discarded tomorrow, for no theory will be abandoned until a better one has been brought forward to take its place, one which can explain the facts in a way more satisfying to the human mind. Change in such a case is progress, and since science must of necessity be always progressing so also must it be always changing.

To those who are conversant with the problems connected with the origin of species it must be obvious that this consideration of the subject does not cover the whole ground; so obvious indeed that perhaps it is unnecessary for me to remark that it is not intended to. There are other theories to be considered and other equally important matters that are more or less interwoven with any one theory of the evolution of new forms. Thus no reference has been made to Mendel's researches on heredity, or the way in which they touch upon the De Vries Theory. This has been omitted purposely, for while the results of Mendel's original experiments in the breeding of peas might be cited at length, I doubt if an apter or more significant example could be found than the one which Professor Wilson used, and as Professor Wilson himself said, the explanation while not abstruse is one that requires considerable preparatory consideration. The Mutation Theory has been developed more in detail, as representing a type of research. Being one of the latest and most important contributions to biological science, and being also entirely germane to the subject in hand, it has seemed proper to devote some time to its consideration. At many points do the fields of modern botany and modern zoology touch, but perhaps it is nowhere so evident as in great problems like these. Here the two sciences work in generous rivalry, each eager to add its contribution to the store of general knowledge, to utilize such information as the sister science brings, to criticize it if need be, but always to accord it a respectful hearing

So much then for the purely theoretical side of botanical research of which I have presented a hasty glimpse. It is necessary before closing to make some reference to the utilitarian aspect; where and how botany directly serves the material needs of man. I hold it myself to be a matter of some pride that a science like botany with a side so purely theoretical and impractical can also lend itself to further, in such important ways as it does, the well-being of mankind, for in the direct application of botanical information to agricultural questions the ways and means of life may be ameliorated. Moreover, it is some of the most theoretical and recondite researches which have led to the most important practical results.

It is possible to consider only a few phases of the practical application of botany, and I will choose those which are not commonly recognized, and which require a high degree of special botanical training. The necessity of botanical knowledge in the use of plants and their products in the arts, or as drugs, is easily understood without further reference, and such uses do not necessarily involve any broad knowledge of plants as a whole.

It is quite different, however, in the matter of plant pathology, for here every channel of botanical information must be used to investigate plant ailments. Bacteria and parasitic fungi, which are themselves plants of a low order, are the cause of the bulk of plant diseases and for that reason the study of their life histories becomes a matter of no small importance. Then, too, the structure and habits of the host plants must be taken into consideration, for upon these may depend the means of prevention or of cure. The assembling of this information and its practical application to the question in hand devolve upon that type of botanist usually referred to as the mycologist, and despite many failures much that is of substantial practical use has been established. One of the earliest, if not the earliest, recorded instances of where a community has taken formal notice of the fungus pests of plants is found in the old Barberry Law passed by the province of Massachusetts before the Revolution. This called for the extirpation of the barberry which had been noticed by the colonists, without any knowledge on their part of the real cause, to be connected with the rust of their wheat fields. Today we may not pass laws for the destruction of diseased plants, realizing perhaps the hopelessness of enforcing them, but we combat plant disease by the establishment of experiment stations devoted to the investigation of such matters.

As a result, there is now at the disposal of the agriculturalist much definite information of ways and means of diminishing or preventing loss through the destruction of crops by disease, losses which statistics show may amount to tens of millions annually; and while the study of the action of bacteria and fungi in the disease of plants is by no means complete, no one can deny the practical results which have been attained. In the more indefinite functional diseases of plants not ascribable to definite parasites, there is room for much more information, which will be forthcoming when our knowledge of nutrition physiology is more full. Already, however, we have suggestions as to the cause of the functional diseases which often appear where the same crop has been raised for many years in succession in the same spot, which bid fair to explain some important plant ailments that are at present not understood.

A more popularly interesting line of activity that has a practical bearing is found in plant breeding, which has recently been attracting wide attention. Plants are now bred systematically for desired characters, not always simply for increased yield, but also for such qualities as resistance to extremes of temperature, to lack of moisture in dry or semi-arid regions, to resistance towards specific diseases, and even for the more esthetic qualities of flavor or color. The old hit or miss methods of the improvement of strains by empirical rules of selection is passing away, and more and more scientific methods, based on the latest results of investigations of heredity and variation, are being employed. Passing over the older methods I will take up two very different types of plant breeding, both modern: one the strictly scientific, the other the intuitive.

The first method we owe largely to Nilsson, who introduced it at an experiment station in Sweden in connection with the cultivation of various cereal crops. It may be said that previous to his advent the older methods had been tried and abandoned as a failure. With his knowledge of what had been published about heredity and variation, Nilsson, after some preliminary experiments, arrived at the conclusion that no new, pure or constant strains of wheat could be obtained unless the fruit of a single ear was bred separately, and thus he established what is known as the principle of breeding from the single ear and not from assorted lots of seed taken from many individuals. This breeding he continued, picking out any chance favorable ear which he could find, until he obtained many thousands of different forms owing to this multiplicity of strains mixed in the ordinary wheat. Of course some turned out to be mere bastard strains and only the ones which continued to breed true to character were kept. These constituted the new agricultural varieties,—in reality elementary species and mutants—which, after severe tests had proved them suitable, were raised in marketable quantity for seed. The amount of work involved was enormous, the mere bookkeeping of the accurate pedigree record with notes on the life history of each form and its progeny was in itself no small matter. Besides the principle of single-ear breeding, Nilsson also established the fact that but a single selection alone is necessary to fix a new strain, provided the progeny of the chosen ear are carefully guarded from admixture with other forms. All this seems absurdly simple, and it is simple, so much so that it is quite possible of application by a person of average intelligence who has had the proper instruction, but the important point is that it was discovered by the application of thoroughly scientific methods. Nilsson's principle is in very general application today and is being used to excellent effect in the improvement of Indian corn in the middle West.

Contrast with this the methods of Mr. Burbank, whose name is familiar to all. It is not that he should not be given the credit of having established new and useful strains of cultivated plants, or of having done some remarkable feats in the way of plant breeding; but it is that his methods are almost purely intuitive and would die with him, were his own records all that there was to be left behind, a striking difference from the mass of data accumulated by Nilsson. It is the rule of thumb method, picturesque but uncertain, as against the surer but less romantic practices of science.

The matter of general scientific agriculture opens an immense field in which I can call your attention to a few points only. The scientific care of our forests, for trees may be regarded as a crop and their culture agriculture, is a question to which we in this country are awakening none too soon. Forestry as practised in Europe, demanding as it does expert botanical knowledge, perhaps not by the foresters themselves but by those who direct their labors, has saved what were the fast diminishing wooded areas. There is need of haste with us for similar scientific treatment of the problem by men who are not simply woodsmen, but botanists as well.

The scientific rotation of crops, the use of fertilizers and the study of the physical and chemical condition of the soil in connection with the living plants, involve questions which may mean the success or failure of much of our farming. These questions can only be settled by careful investigations which take into consideration the nature of the plants themselves as well as the physical conditions of their environment. Some may say that knowledge along this line has been satisfactorily handed down from father to son, that the farmer knows his business better than does the scientist, but it is a patent fact that this is not so. For instance, many a farm which has been damaged for a long period of years by the over-liming of the soil might have been spared had the farmer of fifty years ago had the knowledge, which we now have, of the relation of lime to the other mineral substances needed by the plant, of when to apply it and when to withhold it. It is the difference between merely empirical knowledge and that which is based on scientific principles.

When the contest comes between virgin soil and long tilled land, the latter, no matter how rich it may once have been, must needs be cultivated more intensively if it is to hold its own. Intensive cultivation requires the aid of special information and it is here that scientific agriculture comes into play. Few people realize that, without artificial fertilizers, the direct outcome of highly theoretical work on the raw food stuffs of plants, much of the farming of today would be almost impossible. And the proper use of fertilizers is but one of many questions.

We are coming now in this country to a stage in its development when scientific agriculture must be seriously considered. Fortunately it is being so considered and the federal and state establishments devoted to the investigation of these agricultural questions may confidently be expected, I think, to help in the solving of the practical economic questions that must arise in the competition of our own agriculture with that of other lands. The way it must be done is by the introduction of improved methods based on carefully conducted scientific research, that often find their stimulus in the highly theoretical investigations of the pure scientist. Thus must the so-called impractical devotee of science come in contact with the practical man of affairs and furnish him knowledge that can be used for the benefit of all.

In this somewhat categorical fashion then, I have endeavored to present to you some of the content of the science of botany; that science which consists of the dismembering of flowers and the giving to them of long names. What its future will be is perhaps already indicated, but briefly you can see that it is the direction of physiological advance, away from pure taxonomy and formal morphological conceptions towards the realm of function; away, too, from any segregation of the science from kindred fields towards a better understanding of the place of plants in the whole cosmic scheme.

Man's attitude towards the unknown,—his philosophy in short—must influence his attitude towards botany as it will towards any science; and since philosophy, like other lines of intellectual activity, changes and progresses, man's attitude towards science is not a fixed or rigid one. But it is not likely that philosophy will ever tend to discourage investigation, and investigation is the keynote of scientific progress. Unquestionably, the world demands research, and any fact no matter how humble, if accurately established, helps on the cause. Perhaps the time will come when our knowledge of today will seem as crude as that of yesterday now seems to us. Let not that concern us, except to urge us to do what we may in hastening this time, knowing that that is where real progress lies, and knowing too that there is ample work that can and must be done.

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