FACTS AND HYPOTHESES IN THE PROBLEM OF EVOLUTION

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BY

E. BLATTER, S.J., Ph.D., F.L.S.

Botany is an empirical science, the plant its object. What we see and observe in the plant forms the contents of botany ; but we are not thinking of our observations as raw material, but as facts to be connected by the process of thinking and cleared up by scientific interpretation. This leads up from the raw material to the crystallization of a small number of well-founded sentences by which we give expression to facts or present the efficacy of the laws of nature. A heap of types is not a book ; in the same way, the mere accumulation of facts is not science. The economy of our thinking forces us to abstract general ideas from the variety of objects, and, again, to combine those ideas into general statements of narrower or wider significance, and into laws which may be specific or of a more general nature. This is the way leading to the knowledge of the plant.

How far this ideal is removed from reality is known to everybody who has worked seriously in any branch of botany. The difficulties which stand in the way of the realization of that ideal are many. Some come from the object itself, the plant. In many cases where we are not in a position to formulate laws, we have to be satisfied with mere rules which allow of a wider or more restricted application.

To a great extent the human weakness of the botanist has to be blamed who is ever ready, when experience fails, to fill in the gaps in his knowledge with speculation and who, in his desire for dogmatic finish, is only too often tempted to mix up mere problems with laws based on experimental facts. In order to justify such mistakes he applies the word theory to something that does not even deserve to be called a hypothesis.

A further aberration is caused by the fact that botanists are frequently guided in their judgment by tradition and school-opinion, whilst elimination of errors and search for truth should be the only guiding star. There is no dogma in science to which we should blindly submit.

But we have seen worse things in the botanical world, we have come across fashions. We admit, it is quite justified to strike out in a new direction of investigation after the prevailing interest in botanical research has followed a certain line for some time ; but we cannot see how it works towards progress if the disciples of the new or so-called 'modern' school look down with conceit on the representatives of the old school or, as it is called, the 'antiquated' school, and *vice versa.* There are, e.g., investigations in systematic botany which have been performed with the greatest possible mental acumen and, on the other hand, we meet physiological research whose intellectual value does not rise above a very moderate level. Both, however, help in the interpretation of nature.

But enough of these general considerations. I have put them before you in order that I may find it easier to make myself understood when I speak of the 'Facts and Hypotheses in the Problem of Evolution.'

We are botanists. We know the flora of the present day fairly well, we know its distribution, we know its aspects in various countries, we know a good deal about the migration of its members, we have classified the plants according to artificial systems and we have also tried to classify them according to natural systems. But we want to know more : we want to find out how the present-day vegetation came into existence, whether it is the product of evolution or not.

Whatever may be the answer to this last question, I am not going to discuss the origin of the first plant or plants on this globe. Observation and experimental science cannot give us a direct satisfactory solution of this question ; but biologists have established the axiom : '*Omnis cellula e cellula*,' and we are on firm ground when we conclude that the first cells cannot have evolved from matter, but that they must have been created. I take, therefore, the first plants as given, not determining whether there were few or many, and whether they belonged to one species or to many. And now I put the question again : Is the present-day flora a product of those first plants ?

You all will say : 'Yes,' and I say 'yes' with you. But now let us be absolutely honest, let us forget for a moment that those were great naturalists who put the idea of evolution into the world, let us forget that the whole scientific world *believes* in evolution, (I say on purpose 'believes'), let us forget that evolution has become so to say a universal law invading every domain of human knowledge, let us also forget that almost every fact in the organic world is being studied with a view to ascertain its significance in the great scheme of evolution : and now let us approach the problem without prejuidce, without inclination towards the opinion of this or that school of thought.

1. *Fact and Speculation.*—There is no branch of botany in which the differences are more prominent between experience and speculation, between fact and hypothesis, between knowledge and belief, between scientific and philosophical treatment of the problems, than they are in the field of the theory of evolution. At the same time there is no other field in which, through the mixing up of actual experience with philosophical speculation, there has arisen a greater pseudo-scientific confusion. You must not think that I want to condemn philsophical speculation ; it is justified side by side with experience ; but it becomes unscientific as soon as we

cease to know exactly where experience stops and where speculation begins, when we ignore how much of the theory of evolution is scientific fact and how much is mere philosophical conjecture. It is just here that we should carefully distinguish between the results of observation and the products of our imagination.

2. *Systematic and Historical Treatment.*—The chemist and physicist as well as the botanist are dealing with facts. In physics and chemistry facts that have been ascertained are classified and their mutual relations investigated; laws are recognized only in cases where they have been proved to be constant. In chemistry the elements are accepted as a given variety, so are the forms of energy in physics ; we do not inquire into their origin.

But in botany it is the historical consideration that is more prevalent. We consider the plant as the result of an historical process, and by this method botany resembles geology. We want to know, e.g., where the species come from, and we try to find an answer in the theory of evolution.

3. *Two Facts and One Hypothesis.*—The answer which may vary a good deal as regards detail is based on two facts to which evolutionists have added an hypothesis.

The first fact is the uninterrupted continuity of birth in any series of descendents. We can accept this fact without hesitation, as we start from the supposition that the laws of organic formation were the same in former periods as they are at present. If we deny this principle we must also deny to cosmogony, astronomy and geology the right of investigating into the past, because the laws of nature might have changed in course of time.—The second fact is the positive knowledge that in former geological periods our globe was covered with a vegetation which was different from what we see at present.

These are the two facts : continuity of birth and a different vegetation in former periods. Now I ask you, ladies and gentlemen, what can we conclude from these two facts ? Nothing at all. Are we justified in drawing the conclusion that our present species are not the product of evolution ? Certainly not. Can we conclude that evolution *has* taken place ? By no means. The mere fact that the records of palæobotany show us plants which do not exist in our days does not prove anything. That fact would prove evolution if we could show that those fossilized plants were the ancestors of our modern plants. But this is an almost impossible task. Anybody who has followed up the attempt of systematic botanists during the last 40 years will admit the enormous difficulties that stand in the way of working out a genealogical tree or trees. I think there was no botanist, after Bentham and Hooker, who had a better grasp of systematic principles and who tried harder to frame a natural system on the basis of evolution than Engler. And it was he who confessed in the last edition of his *Syllabus der Pflanzenfamilien* (1924): 'Though I expect results from phylogenetic methods in the study of single families, especially with the aid of plant geography, I cannot help being sceptical with regard to many attempts to derive families from one another, either from living or extinct ones.'

But I must not cite authorities. As I said before, our investigation is an independent one.

What then shall we do if the two facts mentioned above do not lead to any conclusion ? We could drop the question altogether ; but this is not satisfactory to the inquisitive mind of a scientist. So the question remains : Are the present-day plants a product of evolution ? As only those two facts are at our disposal and as they revealed themselves to be barren, there is only one solution possible, viz. to call to our aid a hypothesis. We may formulate it in this way : Let us suppose a phylogenetic development of the plants which, on the whole, progresses from single to more compound forms and which is analogous to ontogeny that begins with a fertilized cell and develops into a highly organized body.

This is the hypothesis which is the foundation of every investigation connected with evolution. And it is *only* a hypothesis and nothing more, and the fact that it is called theory of evolution does not change matters in the least. It is a hypothesis and will remain a hypothesis till phylogenetic evolution has been proved to be a fact.

Astronomy and biology have telescopes and microscopes to reveal a variety of things co-existing in space which to the unarmed eye remain hidden, but we do not know of any instrument or method by which we could penetrate or illumine the darkness of the past. We can, however, link up a modest number of facts by philosophical speculations and in this way the treatment of the problem develops into a discussion of possibilities, but never beyond, unless what our hypothesis contains is no more hypothetical but real. As long as we use our hypothesis as a heuristic working hypothesis and do not enunciate it as a scientific dogma, it may bear ample fruit in the tracing of connections between organisms. Just because it has done so up to now, it is of great value even to the purely empirical science.

But it will do harm to human knowledge as soon as we see in it more than a mere hypothesis. We have only to think of the fanciful and wild imagination betrayed by some fanatical defenders of the theory of evolution.

4. *Foundation of Speculation.*—The objective value of every speculation in natural philosophy grows with the number of clearly ascertained facts on which it is based. Considered from this point of view the theory of evolution is not well off.

We gather our facts and observations from two sources : from the fossilized plant world and from the changes which can be observed in the living vegetation. There is no other source, if we want facts!—All the rest is speculation, made up of conclusions from more or less important indications, of the discussion of possibilities and probabilities, in short, of hypotheses which can neither be proved nor refuted.

The amount of material accumulated from both fields (fossil and living vegetation) is great, and still the theory of evolution receives little light from it. The facts are rarely quite univocal, and it is for this reason that there are scarcely two botanists whose convictions as regards the theory of evolution are the same, whilst other speculative domains of natural science, e.g. stereochemistry enjoy the approval and consent of a large number of scientists. This also explains the strange phenomenon that, from time to time, a radical agnosticism in questions relating to evolution is gaining the upper hand. Agnosticism, of course, is the most convenient attitude towards such problems : it saves us the trouble of thinking.

5. *Palæobotany* : —*a. Cryptogams.*—Let us first consider the results of palæontology. The history of the past teaches us without doubt that in former geological periods the vegetation was different from ours. No plants have been preserved in the Pre-Cambrian and Cambrian rocks. The oldest plants, according to our present knowledge, have been found in the Silurian rocks—viz., ferns which resemble those of the present day. But I must not anticipate.

Of the Thallophytes (Algæ, Fungi and Lichens) only those species could be preserved which, by the deposition of calcium carbonate and silica in their cell-walls, formed a skeleton able to be fossilized, whilst the soft species soon became a victim to putrefaction. We find, therefore, amongst fossil Algæ only Diatoms and Chalk-Algæ. The Diatoms go down right to the carboniferous age and they are scarcely distinguishable from the living ones. Of the Chalk-Algæ we must separate two types : the simpler *Siphoneæ* and the more complicated *Corallinaceæ.* The *Siphoneæ* occur already in the Silurian age and amongst them we come across forms which resemble the modern types. Even a living genus (Bornetella) seems to occur in the Silurian rocks. *Corallinaceæ*, on the other hand, are known only from the Jurassic, Cretaceous and Tertiary deposits.

At the present day every organism which is not preserved by special circumstances, succumbs to putrefaction brought about by Bacteria. As we have good reason to assume that the soft parts of plants putrefied already in the oldest strata, we are allowed to draw the conclusion that Bacteria existed in those periods. But we have also direct indications of Bacteria having destroyed wood during the Carboniferous age. Well preserved fossils of Fungi have not been observed, but the Tertiary period has preserved a number of Lichens which agree with existing genera. As the bark of our trees is usually inhabited by Lichens, it is a striking fact that no Lichens have been discovered on the bark of Carboniferous strata. It is not unlikely that Lichens did not exist in that period.

Mosses in greater variety date back to the Tertiary period and they are mostly forms which resemble the Mosses of to-day. Some incomplete fragments, however, seem to have come down to us in the Cretaceous and Jurassic rocks.

Ferns have already been found in Silurian times, i.e., in the oldest formation which contains plant fossils at all. It is interesting to note that those ferns had reached the same degree of organization as ours. From the Silurian rocks upwards we meet ferns everywhere, but they reach the height of development in the Carboniferous period, and it is here where we come across types which are more perfect anatomically than the present ones.

The *Equisetaceæ* show the optimum of development in the

Carboniferous strata. Similarly, the Lycopdiales attained the maximum of form and organization during the same period. *Lepidodendraceæ* and *Sigillariaceæ* become extinct during the Permian and Triassic ages, which also saw the end of the tree-like Calamites.

If we compare all the data of palæontology regarding cryptogamic plants we have to admit that there are no forms which might be considered as connecting links between Algæ and Mosses, or between Mosses and Ferns. On the other hand we notice that all the classes of Pteridophytes reach their maximum development already during the Carboniferous period.

b. Phanerogams.—We come to the Gymnosperms with the families *Coniferæ*, *Cycadaceæ* and *Ginkgoaceæ*.

The oldest Gymnospermous types are the Cordates. Traces have been discovered in Devonian rocks ; after having attained their richest development during the Carboniferous age they are no more found in Permian strata. No other Gymnosperms can, with certainty, be traced in Carboniferous rocks. The first reliable fragments of *Cycadcaeæ* are Permian.

In the Triassic and Jurassic strata we find extinct genera of *Cycadaceæ*, *Ginkgoaceæ* and *Coniferæ*. The maximum of their development coincides with the Jurassic period, which has seen the first still existing Coniferous genus Araucaria. In the Cretaceous rocks numerous genera made their appearance which have continued up to the present day. In the Tertiary strata we find only genera which still exist, and in many cases even species.

No traces of Angiosperms have been discovered in the lower Cretaceous rocks. In the upper strata, however, we meet on a sudden numerous Monocotyledons as well as Dicotyledons which show considerable resemblance to their modern relations. The Tertiary period discloses representatives of still existing families, genera, and species.

What are the results of this short evidence of palæobotany ? No close relationship between the oldest Gymnosperms and Angiosperms can be established. Both phyla of phanerogams are as sharply separated in their fossil types as they are in the living ones. The Angiosperms are very young ; we know them only from Cretaceous rocks. The Gymnosperms may be as old as any plant-remains ; if we do not find them in the Silurian strata it may be explained by the fact that very few land-plants have come down to us from that formation. Later on the Gymnosperms as well as the Angiosperms approach the living types more and more, especially in the Tertiary period. In spite of this it is impossible to trace transition series between Tertiary and living species in a satisfactory way. Wherever such transitions have been constructed they are uncertain and allow of no univocal interpretation.

6. *Variation and Experimental Facts.*—We have dealt with the palæobotanical record. It remains to be seen what observation and experiment in the living plant can tell us regarding the theory of evolution.

When we speak of variation we generally mean three groups of phenomena: (*a*) Individual differences; (*b*) single variations;

(*c*) forms produced by crossing and Mendelian segregation. The question before us is this : What influence have these variations on the formation of species ?

7. *Individual Differences.*—We call individual differences all fluctuating inequaliites of an individual and of its organs—e. g. the hairiness of the leaves of a plant, the percentage of starch contained in a grain of wheat, and even more important features of a morphological and physiological nature. These differences, whether quantitative, meristic or individually quantitative, oscillate around a certain mean. We are told that useful individual differences can be increased indefinitely by selection and may finally become independent of selection.

But how do we know that everything that is ascribed to selection has come into existence *through* selection ? We know many races of cultivated plants, but do we know their origin ? Besides, many cultivated forms owe their origin not to the mere strengthening of individual characters, but to crossing and segregation of characters. If we consider only well-attested facts we must arrive at the conclusion that selection does not bring about anything *new* and that the maximum amount of quantitative modification is brought about in a few generations (mostly in three to five) and that only continued selection can maintain this amount. Stopping selection means inducing regression. New species, therefore, cannot arise through selection.

But does not environment influence plants and mould them in many ways ? Quite so, but experiments show that changes of characteristics and niceties of adaptation go to and fro without transgressing definite ranges of variation. And how are we going to explain the discontinuity of species in the presence of a continuous environment, whether it has acted directly in the Lamarckian sense, or as a selective agent as explained by Darwin ? We would have to call for accidental destruction and isolation of intermediate forms, in other words : a second hypothesis would have to give strength to the first.

8. *Single Variations.*—What is the significance of single variations for the theory of evolution ? When from among a large number of offspring some particular individual differs from the rest in one or more characteristics and transmits them to posterity, we speak of single variations and call the whole process mutation. If de Vries's new forms are really new ones, and if future experience shows that they do not owe their origin to some unexpected original cross, then, and then only can we say that single variations are of importance for the solution of the evolution problem, because they are discontinuous and constant and would, therefore, be capable of explaining the gaps between extinct and existing species. But till the possibility of an original cross is completely excluded, de Vries's theory can only be used as a hypothesis in the explanation of evolution. Even when the time comes, when no doubt attaches to de Vries's experiments, there still remains the remarkable fact that the fertility of mutants decreases considerably, and this fact becomes the more pronounced, the greater the deviation from the parent. In addition, the newly

produced mutants are comparatively weak. These two facts require careful consideration when we try to determine the value of single variations for the evolution of species. Finally we must not overlook the fact that those mutants do not exhibit any progressive development. The new forms have not shown the slightest progress in organization, not even indications of any kind of advancement in that direction.

9. *Crosses and Mendelian Segregation.*—We have now to pay a few moments' attention to crosses and Mendelian segregation. As regards cross-breeding in nature we can hardly consider it as a factor in the progressive evolution of species. We know by experience that forms of different degrees of organization do not cross, and even if they did, all deviations would soon be equalized according to the laws of chance and probability.

Apparently greater importance must be attached to the Mendelian segregations. You all know Mendel's rule. A simple analysis reveals three parts : (*a*) By fertilization the characters of the parents are united, but they do not lose their purity and independence ; (*b*) In the offspring the characters of both parents may again be separated from each other ; (*c*) The character of one of the parents may completely conceal that of the other. We know, however, from subsequent investigations that the latter part is not necessarily connected with the rest. I must add that Mendel's rule also holds good for the offspring of hybrids, in which several constant characters are combined. This is a splendid confirmation of the modern theory of the cell.

What is the bearing of Mendel's rule on the theory of evolution ? We cannot deny that it gives support to the idea that gaps in nature can originate through such segregation. But can the idea be applied to the formation of species ? We cannot answer this question at present. One thing, however, is certain, segregation does not bring about any progress in organization or any progressive specific development.

10. *What follows for the Theory of Evolution?*—Now that we have given a short survey of the facts of variation we naturally wish to draw conclusions. The central idea of modern evolution theories is progressive specific development. I appreciate the enormous amount of work that has been done in the way of elucidating the problems of variation, and we have to be grateful to the botanists and biologists who have put at our disposal an immense number of experimental facts, and we cannot help admiring the acumen and devotion that have been employed in the co-ordination of new observations and discoveries towards the construction and consolidation of the theory of evolution. At the same time I must confess that all the observations gathered from the world of organisms as it now exists does not give any confirmation to the theory which wants to explain the evolution of new species. What we have before us are hundreds of hypotheses ; a few are leading ones, some are subordinate, and others do not even deserve the name of hypothesis. I am not exaggerating when I say that in most of them the speculative element preponderates over facts, and it would not be difficult to show that many are

the product of mere imagination. If the fact-element were more prominent in all the treatises that have been written on evolution by defenders of evolution, botanical literature would not offer so many different views and opinions on the same subject, such a variety of contradictory statements, so many empty terms and meaningless phrases which can only have been coined in dream-land, but especially we would not come across so many personal attacks amongst colleagues which only betray the absence of facts.

11. *Conclusion.*—We have, therefore, not yet a satisfactory reply to the question: How did the present flora come into existence? The greatest difficulty is to explain the origin and constancy of new characters and the teleology of the process. The question as to the transmission of acquired characters is not by any means decided. The doctrine of propagation tells us that only such characters can be transmitted as are contained in the germ-cells or which have been either directly or indirectly transmitted to them. Hence it is clear that all peculiarities acquired by the cells of the body through the influence of environment, or by use or disuse, or any other agent, can only be inherited if they are handed over, so to say, to the germ-cells. But it is useless to discuss the question before we have sufficient experimental evidence that acquired characters are at all inherited.

Darwin's 'natural selection' is only a negative factor when we want to use it for the explanation of the origin of new characters. It is quite true that the plasticity of organisms has been proved by a number of experiments to be considerable. In a constant environment and by single variations changes may be effected which a systematist would classify as specific or even generic, if it were not clear from other sources that they are not such; but at present we are unable to ascertain how far that influence may extend. Lamarck's 'Inheritance of acquired characters' is not yet exactly proved, nor is it evident that really new forms can arise by mutation.

All this does not sound very encouraging. The theory of evolution is no more than a hypothesis, and it is highly unscientific to proclaim evolution as a well-established theory or as a fact, whether this be done in scientific treatises or in popular books. Science does not gain by exaggeration. It will make progress only by drawing legitimate conclusions from facts. We shall serve science much more efficiently by confessing ignorance where there is ignorance, than by constructing a system made up almost entirely by hypotheses, views, opinions, indications, probabilities, and possibilities, and only here and there supported by a meagre fact whose interpretation is only too often ambiguous.

Does this mean that we should give up the theory of evolution? Far from it! I suppose I am right in assuming that you *believe* in evolution! and so do I. I said on purpose 'you *believe* in evolution.' There cannot be a question of conviction for a scientist where not every link leading up to his theory is an established fact or a legitimate conclusion from facts. For us the mere idea of evolution has a peculiar charm. We are surrounded by a variety of organisms which are teaming with problems, whether we find them in geological strata or on the surface of our globe, or in the air, or in the water. We want to find an answer to all the questions which nature itself puts to us. There is especially one mystery the human mind wants to solve, viz., the origin of our species. It is a mystery of absorbing interest, whose solution will throw light into the remotest periods. We are still groping in the dark and sometimes it seems as if the sun would never rise on our mental horizon, as if the past would for ever remain a sealed book to the inquiries of our mind. At present we try to find the solution of that mystery in the theory of evolution. There are many facts and many indications that point in that direction ; we seem to feel that we are on the right path, though we are not as yet able to furnish convincing arguments to establish the truth of evolution. It is a gigantic problem and we may not see its solution.

It is over 100 years since Lamarck offered the world the first theory of evolution. His period ended with an almost complete victory for the theory of constancy (1830). Then came Darwin and gave us his 'Origin of Species' (1859). His theory entered into every department of the biological sciences and to a great extent transformed them. After Darwin followed a period of critical reaction and we belong to that period. We are not able to say what changes may befall the problem of evolution during the twentieth century. One new discovery may bring a solution we never dreamt of, or it may revolutionize our views and opinions, or it may even destroy our hopes and aspirations to see the theory of evolution confirmed and established. Whatever may happen and whatever the solution may be, we shall never regret having used the theory of evolution as a working hypothesis. It has opened out vast fields for investigation, it has called into life new branches of the biological science, it has given renewed interest to many departments of botany which were threatened to become dry archives of names and descriptions without an intellectual foundation, it has multiplied and perfected the methods of scientific investigation, and above all, it has given an importance even to the smallest detail of scientific knowledge, because there is nothing that has not been requisitioned to serve as a building stone in the construction of the theory of evolution.