SCIENCE

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THE PRESENT STATE OF THE PROBLEM OF EVOLUTION[1]

THE exchange of professors between the Sorbonne and Harvard University for the first time brings to Cambridge a professor of science. In a certain way I come in return for the visits which Professor M. Bôcher and Professor W. M. Davis have already made to the faculty of sciences at Paris. All my predecessors belonged to our faculty of letters. All have brought back a recollection of the hearty welcome which they received, and what they told me contributed largely in inducing me to accept the mission which was offered to me. I had the assurance of good-will and generous sympathy from my colleagues as well as from my pupils.

In the beginning I must excuse myself for not being able to express myself, at least for the present, in English. The most important point in teaching is clearness in expressing thoughts. By speaking to you in my own language I hope to succeed much better in a difficult subject and for that reason to obtain forgiveness for the effort which, to my great regret, I occasion you.

The purpose of the exchange between the two universities is to convey to the one the methods of teaching employed in the other. I have the honor to occupy at the University of Paris a chair of biology especially devoted to the study of the evolution of organic beings. It is then to the present state of this great problem that the lectures

[1] An introductory lecture in a course offered by M. M. Caullery as exchange professor at Harvard University, February 24, 1916. Translated from the French by Mrs. C. H. Grandgent.

which I am going to give will be dedicated. I do not enter upon this subject here without some apprehension. Certain of my predecessors, by the very nature of their subjects, were able to have, at least the illusion, that Europe is still the veritable center of learning. But I have not this advantage. The necessary conditions for the development of the sciences are now at least as well fulfilled, I will even say better fulfilled, in the United States than in Europe, and for many of the sciences, Europeans coming to this country have as much to learn as to teach. This seems to me particularly the case in biology and especially in the questions connected with the problem of evolution.

Besides, the advance of American science in these directions does not date from yesterday. In the study of paleontology, which has a large place in the questions with which we are to concern ourselves, your scholars have, for a long time, been working with activity and considerable success the marvellous layers of American deposits, and have drawn from them, to cite only one instance, magnificent collections of reptiles and mammals, which we come to admire in the museums on this side of the Atlantic. Here more than anywhere else have been enlarged the paths opened a century ago by George Cuvier. In zoology, properly speaking, the museum of comparative zoology, in which I have the honor to speak at this time, justly famous in Europe, bears witness to the importance and long standing of the results accomplished. Louis Agassiz, more than half a century ago, was one of the most eminent names of his generation. Later, when the investigation of the great depths of the ocean marked an important and consequent stage in the knowledge of earth and life, Alexander Agassiz, his son and illustrious successor, was one of the most eager and skillful workers. The expeditions of the *Blake* and of the *Albatross* are among those which have drawn from the deep the most important and most precious materials, and their results have been the most thoroughly studied. The personality of Alexander Agassiz, whom I had the honor of meeting in Paris about thirteen years ago, made upon me a striking impression. His real laboratory was the ocean, and he succeeded to the end of his life in maintaining an activity that corresponded to its amplitude. He was truly the naturalist of one of the great sides of nature. Around Louis and Alexander Agassiz, the museum and the laboratory of comparative zoology of Harvard College have been for a long time a center of studies of the first rank. In the domain of embryology Charles S. Minot also has carried on important work. But it is especially at the present moment that American biological science has made an amazing advance which expresses itself in the excellence of publications and in the results which they reveal by the number of collaborators, the activity of societies, the number of laboratories, and the abundance of material resources at their disposal. Here occurs a special factor, which has considerable importance, the enlightened and large generosity of numerous patrons. It is incontestable that men of talent find more easily in America than in Europe, and especially at the age of their full activity, the cooperation without which their greatest efforts are to a certain extent barren. Now, at the point to which we have arrived, the greater part of scientific problems demands the exercise of considerable pecuniary resources and of collaborators of various capabilities. This is particularly true of biology, where, moreover, many questions, notwithstanding their scientific importance, do not lead to practical application, at any rate immediately. We succeed too rarely in

Europe in combining these resources, above all in combining them rapidly enough. The European public does not sufficiently realize their necessity and interest. And the action of the state necessarily lacks the flexibility needful for rapid realization. Thus Pasteur was able to organize the institution which bears his name only at the end of his life, and at the inauguration he was heard to say mournfully, "I enter here defeated by Time." In America the power and the eagerness which private initiative gives provide for this need. Truly the greatest wonder is that this liberality is generally well conceived and well employed.

It is also true that the problems of the day in contemporaneous biology are nowhere else attacked at the present time with such activity, perseverance, and success as in the United States. As we look at different points on the biological horizon, we see the studies on the Mendelian theory of heredity in full development in numbers of laboratories. It will be enough for me to cite in this connection the names of Messrs. Castle and East in this very spot, and that of Mr. T. H. Morgan in New York. In the realm of the physiology and the structure of the cell and of the egg, the researches of E. B. Wilson, and of his pupils on the chromosomes, of J. Loeb on experimental parthenogenesis, of F. R. Lillie on the fertilization of the egg, of Calkins and recently of Woodruff on the senescence of the infusoria, suffice to show the share which this country has had in the advance of knowledge. And I ought also to mention numerous works on embryology and on the study of the filiation of the cells of the embryo (cell-lineage), on regeneration, on the behavior of the lower organisms, on geographic distribution and the variations of the species studied from the most diverse sides; all branches of biology are flourishing vigorously. In addition, the United States, more than any other country, has developed scientific institutions designed for the study of the application of biology to agriculture, to fisheries, etc.

In the face of this situation, I wish to make it clear at the outset that I have not the least expectation of bringing here a solution of the problem of evolution. I have too full a realization of the extent of the scientific movement aroused by this question in the United States and I hope to derive great benefit myself from my stay here, from the contact which is permitted me with my colleagues and with their laboratories. This latter advantage is not the least which arises from the exchange between the two universities. Nor have I the expectation of bringing to you a new solution of the problem, nor of examining it from a special and original point of view, such as might be the case in a single lecture or a small number of lectures.

I will adhere strictly to the point of view of the instructor, taking the question as a whole, expounding it in its older aspects as well as in its more recent ones. The interest in these lectures is above all, in my opinion, in the coordination of facts and in their critical examination. As this coordination is influenced in a large measure by the surrounding conditions, the view that a naturalist has of them in Paris ought to be interesting here. In questions as complicated and as undeveloped as these still are, where we have not reached a precise conclusion, the relations of facts can not be established in a harsh and unequivocal fashion. This is particularly true of the problem of evolution at the point we have reached. During the last few years very rapid and great progress has been made in our knowledge relative to certain kinds of data; notably heredity and variation. But they have not failed to shake markedly the notions which previously seemed to be at the very foundation of evolution. One of my compatriots, an ardent disciple of Lamarck, F. Le Dantec, wrote even as far back as eight years ago a book bearing the significant title "La Crise du Transformisme"[2] in which he brought out the contradictions in question, contradictions which, according to him, were to result in the ruin of the very idea of transformism. Since that time opposition has become even more marked and at the present day, either tacitly or explicitly, certain of the most authoritative men, by their works, have arrived very near to a conception which would be the negation of transformism rather than its affirmation.

The term "evolution," in French at least, has had historically two contrary meanings. In the eighteenth century, it was the expression of the theory of the preformation or "emboitement" of the germs, according to which the lot of every organism was determined from the beginning. The succession of generations was only the unfolding (*evolutio*) of parts that existed from the beginning. In the nineteenth century, and it is in this sense that it is always used now, it had an opposite sense; it is the synonym of transformism and it signifies the *successive* transformation of animal or vegetable organic types, not realized beforehand, in the course of the history of the earth, under the influence of external causes. Now, if one admits the general value of certain of the ideas recently expressed, evolution would be only the unfolding of a series of phases completely determined in the germs of primitive organisms. It is a reversion, under a modern form, to the idea which the word evolution represented in the eighteenth century. It is unnecessary to say that I use the word evolution in its nineteenth-century sense, which is synonymous with transformism. It is evident then that all is far from being clear in the present conception of transformism and that, in consequence, an exposition of its various aspects and an effort to coordinate them is not a useless thing in a course of lectures. Furthermore a comprehensive glance at the principal questions which we shall have to examine will make my meaning clear and will give me the chance to indicate the general plan of the course.

In spite of the contradictions to which I have just alluded, the reality of transformism as an accomplished fact is no longer seriously questioned. We can make the statement that, in the unanimous opinion of biologists, evolution, that is to say, the gradual differentiation of organisms from common ancestral forms, is the only rational and scientific explanation of the diversity of fossil and living beings. All the known facts come easily under this hypothesis. All morphology in its different aspects, comparative anatomy, embryology, paleontology, verifies it. By virtue of this same hypothesis, these different branches of morphology have made an enormous progress since Darwin's day. The significance of certain categories of facts, especially in the domain of embryology, may have been exaggerated. Scientific men have certainly overworked the idea that the development of the individual, or ontogeny, was an abridged repetition of phylogeny, that is to say, of the several states through which the species had passed, an idea which Haeckel raised to the fundamental law of biogenesis and which a whole generation of naturalists accepted almost as a dogma. Without doubt, ontogeny, in certain cases, shows incontestable traces of previous states, and for that reason embryology furnishes us with palpable proofs of evolution and with valuable information concerning the affinities of groups. But there can no

2 "Nouvelle collection scientifique," Paris, Alcan.

longer be any question of systematically regarding individual development as a repetition of the history of the stock. This conclusion results from the very progress made under the inspiration received from this imaginary law, the law of biogenesis.

The first part of the course will be devoted then to the consideration of the general data which morphology furnishes toward the support of the idea of evolution. Thus we shall see what conception comparative anatomy, embryology and paleontology affords us of the way in which evolution is brought about, and within what limits we may hope to reconstruct it. Evolution is essentially a process which belongs to the past and even to a past extraordinarily distant. It is a reasonable supposition that evolution is going on to-day, but let us remember that nothing authorizes us to believe that what we may observe in the present epoch about organisms will necessarily explain the succession of their former states. Evolution is an irreversible process and one which has not progressed at a uniform rate. We must not then expect to verify necessarily by the present organisms all the facts disclosed by morphology. It follows in my opinion that morphological data may force upon us indirectly certain conclusions even though we should have no experimental proof of them in contemporary nature.

Because of this very limitation which I have just pointed out, much of the difficulty of the study of the mechanism of evolution arises and to this may be attributed many of the profound differences among naturalists on the subject of evolutionary mechanism. The second part of the course will be devoted to the examination and the criticism of the solutions that have been proposed.

In a general way, the study of the mechanism of evolution is that of the reciprocal influence of agents external to the organisms, on the one hand, and of the living substance, properly speaking, on the other hand. There are then, if you wish, the external factors which together constitute the environment, and the internal factors which are the specific properties of the organism. These two elements are very unequally accessible to us. The environment is susceptible of being analyzed with precision, at least as far as the present is concerned, and we can surmise it with enough probability as to preceding periods. We know very much less about living matter, and especially about the way in which its properties may have varied in the course of time. Hence one meets with two tendencies which have been encountered ever since the evolutionary question arose and which are still very definite and very contradictory in their effects on the general theories of evolution. One of those attributes a large share to the external factors and attempts to explain facts by physico-chemical actions which are directly accessible. The other sees in internal factors, in the intrinsic properties of the organism itself, preponderant if not exclusive agents.

The first tendency attracts us more because it gives a larger share to analysis, that is to say to the truly scientific method. The second flatters our ignorance with fallacious verbal explanations. It is open to the objections brought against vitalist conceptions; and when, as is the case of certain old and new theories, we come to restrict the effective rôle to internal factors alone, we may ask ourselves whether there is a really essential difference between conceptions of this nature and creationist ideas; between declaring that species have been created successively and arbitrarily by an arbitrary sovereign will, without the external world having influenced their structure, or maintaining that organic forms succeed one another, derived to be sure one from another, but following a succession that is really determined in advance and independent of external contingencies. Between such views there is in reality no considerable difference. Such an idea substitutes for successive creations one initial creation with successive and continuing manifestations. The present crisis of transformism, as Le Dantec and others set it forth, is the conflict concerning the reciprocal value of external and internal factors in evolution.

The two principal and classic solutions proposed to explain evolution were based on the efficacy of external factors, both the theory advanced by Lamarck in 1809 in his "Philosophie Zoologique," as well as that of Darwin formulated in 1859 in "The Origin of Species." Lamarck starts in fact with the statement that the structure of organisms is in harmony with the conditions under which they live and that it is adapted to these conditions. This adaptation is, in his opinion, not an *a priori* fact but a result. The organism is shaped by the environment; usage develops the organs in the individual; without usage they become atrophied. The modifications thus acquired are transmitted to posterity. Adaptation of individuals, inheritance of acquired characteristics, these are the fundamental principles of Lamarckism. Except for its verification, it is the most complete scientific theory of transformism which has been formulated, because it looks to the very cause of the change of organisms by its method of explaining adaptation. Darwin adopted the idea of Lamarck and admitted theoretically adaptation and the inheritance of acquired characteristics, but he accorded to them only a secondary importance in the accomplishment of evolution. The basis for him is the variability of organisms, a general characteristic whose mechanism he did not try to determine and which he accepts as a fact. This being so, the essential factor of the gradual transformation of species is the struggle for life between the individuals within each species and between the different species. The individuals which present advantageous variations under the conditions in which they live have more chance to survive and to reproduce themselves; those which on the contrary offer disadvantageous variations run more chance of being suppressed without reproducing themselves. There is established then automatically a choice between individuals, or, according to the accepted terminology, a *natural selection,* a choice which perpetuates the advantageous variations and eliminates the others. And with this going on in each generation the type is transformed little by little. Natural selection accumulates the results of variation.

This is not the time to discuss Darwin's theory. I wish only to observe to-day that it is less complete than that of Lamarck in that it does not try to discover the cause of variations; also that, like that of Lamarck, it attributes a considerable participation to the conditions outside the organism, since it is these finally which decide the fate of the variations. And one of the forms in which the opposition to the transformist ideas, at the time of Darwin, manifested itself, was the very argument that if organisms had varied it was only because of an internal principle, as Kölliker and Nägeli have more particularly explained.

The biologists at the end of the nineteenth century were divided with regard to the mechanism of evolution, into two principal groups, following either Lamarck or Darwin. Among the neo-lamarckians some have accorded to natural selection the value of a secondary factor, holding that the primary factors are the direct modifying influences of the surroundings which according to them cause the variations. Selection came in only secondarily, by sorting out these variations and especially by eliminating some of them. Such was the particular doctrine developed by my master, A. Giard, at the Sorbonne. Others have more or less absolutely refused to grant any value to selection. Such was the case of the philosopher Herbert Spencer. We must also recognize that, since the time of Darwin, natural selection has remained a purely speculative idea and that no one has been able to show its efficacy in concrete indisputable examples.

The neo-darwinists, on their side, have, in a general way, gone further than Darwin because they see in selection the exclusive factor of evolution and deny all value to Lamarckian factors. This was the doctrine of Wallace, and has been especially that of Weismann. I will digress a moment to speak of the ideas of these last-mentioned authors, because of the influence which they have exerted and still exert, correctly in some respects, incorrectly in others, at least as I think.

Weismann attacked the doctrine of the inheritance of acquired characteristics and has incontestably shown the weakness of the facts which had been cited before his time in support of this kind of heredity. But he went too far when he tried to show the impossibility of this form of heredity. In so doing, he starts from a conception which meets with great favor; the radical distinction between the cells of the body proper, or *soma,* and of the reproductive elements or germ cells. He saw, in these two categories, distinct and independent entities, the one opposed to the other. *Soma* which constitutes the individual, properly speaking, is only the temporary and perishable envelope of the *germ* which is itself a cellular autonomous immortal line, which is continuous through successive generations, and forms the substratum of hereditary properties. The germ alone has some kind of absolute value. The *soma* is only an epiphenomenon, to use the language of philosophers. The *soma* is of course modified by external conditions, but for one to speak of the inheritance of acquired characteristics, the local modifications of the *soma* would have to be registered in the germ and reproduced in the same form in the *soma* of following generations, in the absence of the external cause which produced them in the first place. Now, says Weismann, the possibility of such an inscription, as it were, upon the germ of a modification undergone by the *soma* is not evident *a priori,* and when we go over the facts we find none supporting this conclusion. There are indeed modifications which appear in one generation and which are reproduced in the following generations; but Weismann goes on to attempt to prove that at their first appearance they were not the effect of external factors on the *soma,* but that they proceeded from the very constitution of the germ, that they were not really acquired and somatic, but were truly innate or germinal.

Such reduced to its essential points is the negative contention of the doctrine of Weismann. It rests upon the *absolute* and abstract distinction between the *soma* and the *germ.* In spite of the support which this conception has had and still has, I consider it, for my part, as unjustifiable in the degree of strictness which Weismann has attributed to it. It is true that the advance in embryology and cytology often allows us to identify the reproductive tissue and to follow it almost continuously through successive generations, but the conception of its autonomy is at least a physiological paradox. Though the continuity of the germ cells is sufficiently evident in many organisms, it is more than doubtful in others, particularly in all those which reproduce asexually, that is to say, many large groups of animals like the Cœlenterata, the Bryozoa, the Tunicata, and many plants. This has more than the force of an exception, it is a general principle of the life of species. One can not then say that the conception of Weismann carries full conviction. But this conception exercised a tyrannical influence upon the minds of contemporaneous biologists and it is exclusively through it that most of them look at the facts.

Weismann, besides, exercised a considerable influence by championing a theory of heredity based at the start on the preceding ideas. This theory, built with undoubted ingenuity, and adapted to the knowledge gained from the study of cell division, turns out on the other hand to agree with the recent works on heredity.

Lamarckism and Darwinism shared the support of biologists up to the end of the nineteenth century, discussion being in general restricted to speculation. The controversy begun in 1891 between Weismann and Spencer, who represented the two extremes, gives an idea of the extent to which one could go in this direction.

The last twenty years constitute indisputably a new period in the history of transformism where the field of discussion has been renewed and scientists have sought to give it a much more positive and experimental character. Two kinds of investigation have been developed in this direction: on one hand the methodical study of variations, and on the other that of heredity and especially of hybridization. These two categories overlap.

Note that this new point of view is not, properly speaking, a study of evolution. According to it, variation and heredity in themselves, under present conditions, are analyzed independently of all hypothetical previous states of the organism. Afterwards the results obtained with the Lamarckian, Darwinian and other succeeding theories will be confronted.

The sum of these researches, which are now in high favor, is a new and important branch of biology, which has received the name of *genetics*. It defines for us in particular the hitherto very vague notion of heredity and seems certain to lead us to an analysis of the properties of living substance somewhat comparable to that which the atomic theory has afforded concerning organic chemistry. We can not maintain too strongly its great importance. As far as the theory of evolution is concerned the results obtained up to this time have been rather disappointing. Taken together, the newly discovered facts have had a more or less destructive reverberation. In truth the results obtained do not agree with any of the general conceptions previously advanced and do not show us how evolution may have come about. They have a much greater tendency, if we look only to them, to suggest the idea of the absolute steadfastness of the species. We must evidently accept these facts such as they are. But what is their significance? On the one hand they are still limited, on the other hand as I have already indicated above, and as I shall try to show in the following lectures, the advances made by the study of heredity in organisms, at the present time and under the conditions in which we are placed, does not permit us to accept *ipso facto* the doctrine of heredity for all past time and under all circumstances.

To use a comparison which has only the force of a metaphor but which will make my thought clear, the biologist who studies heredity is very much like a mathematician who is studying a very complex function with the aid of partial differential equations and who tries to analyze the properties and the function about a point without being able as in the case of an elementary function to study it in itself, directly, in all its aspects. The properties ascertained about one point are not necessarily applicable to all space.

As far as the organisms are concerned, the conditions of their variability have not certainly been the same in all periods. The idea of a progressive diminution of their variability has been often expressed, notably by D. Rosa. Le Dantec, according to his favorite theoretical method in which he considers only the fundamental principles of the problem, has tried to reconcile these facts with the Lamarckian doctrine in his book on La Stabilité de la Vie.[3] In the transformation of organisms as well as in that of inert matter, he regards every change as the passage from a less stable to a more stable state. The many organisms, after having varied much and rapidly, might then, perhaps, be for the present in a state of very constant stability, at least the greater part of them. But for the time being, I must omit further consideration of this suggestion.

We shall have then in the third part of the course to examine, while bearing in mind the preceding opinions, the general results of recent researches in variation and heredity. I shall now sum up the principal lines of investigation preparatory to tracing the plan of these lectures.

The methodical study of variations in animals and in plants has led us to recognize that the greater part of these variations are not inherited. If we apply to them the methods of the Belgian statistician Quetelet, we shall perceive that for each property numerically stated the different individuals of a species range themselves according to the curve of the probability of error, the greatest number of individuals corresponding to a certain measure which represents what is called the mean. The term *fluctuation* is given to those variations that are on either side of the mean and the study of these fluctuations, begun in England by Galton, has been developed and systematized by H. de Vries and Johannsen.

In short, it is the whole of the curve of fluctuations which is characteristic of heredity in a given organism, and not such and such a particular measure corresponding to a point in the curve. In cross-bred organisms there is, in each generation, an intermixture of two very complex inheritances, since these organisms result from an infinite number of these intermixtures in former generations. On the contrary, the problem is very simplified, if one considers the organisms regularly reproducing themselves by self-fertilization as is the case in certain plants. Here there is no longer in each generation a combination of new lines, but a continuation of one and the same line. It is the same hereditary substance which perpetuates itself. The Danish physiologist and botanist Johannsen attacked, as you know, the problem in this way, by studying variation along a series of generations in lines of beans, and the conclusion of his researches, which have had in recent years a very great influence is *that each pure line gives a curve of special fluctuations under special conditions.* The variations that we observe in the action of external agents explain the different reactions of the hereditary substance to the conditions of the environment, but this substance itself remains unaltered. The consequence is that, in what since the time of Linné we have considered a species, and have admitted to be a more or less real entity, there is an infinity of lines, more or less different among themselves in their

[3] "Bibliothèque scientifique internationale," Paris, Alcan.

hereditary properties, which are fixed and independent of environment. This it is that Johannsen calls the *biotype,* or *genotype;* a species is nothing but the sum of an infinity of genotypes differing very little from one another. H. de Vries on his side reached analogous views which prove to harmonize with the results and ideas formulated some forty years ago by a French botanist, Jordan, an unyielding adversary of transformism. Jordan, too, by means of well-ordered cultures, had analyzed a species of crucifer (*Draba verna*) in two hundred elementary species independent of one another. He deserves to be considered in any case as the precursor of the ideas of which I have just given a synopsis.

It is not then in ordinary variability, as it was known up to this time, that one can, following the ideas of De Vries and Johannsen, hope to find the key to evolution, since variations can not be the starting point for permanent changes. Examining a plant (*Œnothera lamarckiana*), De Vries thought he had found this key in abrupt transformations succeeding one another in organisms, under conditions which he has not been able to determine and which remain mysterious. The abrupt and immediately hereditary variations he named *mutations* and set them in opposition to *fluctuations* (*i. e.,* common variations). According to him, evolution is not continuous but operates through mutations. The theory of mutations has been, since 1901, the occasion of an enormous number of experimental studies and of controversies, into which I shall not enter at this time, but I shall finally endeavor to extract the results won by this method of work. Let us note that, if De Vries and the mutationists do not formally deny the intervention of external factors in the production of mutations, the rôle of these factors is no longer very clearly or directly apparent, and some deny it more or less fully. In short, systematic study has led to an antithesis between *fluctuations* produced under the influence of the environment but not hereditary, and *mutations* not directly dependent upon the environment but upon heredity. We shall have to discuss the value of this distinction, the extent and the importance of mutations.

Another and very effective branch of research which has developed since 1900 and which dominates the study of biology just now, is the study of hybridization, which has led to the doctrine known as Mendelism. Sometimes the name *genetics* is specifically applied to it.

Toward 1860, the study of hybridization had led two botanists, the Austrian monk Gregor Mendel and the French botanist Naudin,[4] simultaneously but quite independently, to conceptions which did not particularly attract the attention of their contemporaries but which were brought to light again in 1900 and which then formed the starting point of very many and important investigations. The experimental study of Mendelian heredity has been carried on, especially here in Harvard, with great success by Mr. Castle on various mammals, and by Mr. East on plants. This topic therefore is familiar to the students of biology in this university. I shall speak of it for the present, only to state the general results. Let me recall to your minds as briefly as possible the essentials of Mendelism; according to this doctrine most of the properties which we can distinguish in organisms are transmitted from one generation to another as distinct units. We are led to believe that they exist autonomously in the sexual elements or *gametes,* and we can, therefore by proper crossing, group

[4] "Nouvelles Recherches sur l'Hybridité dans les Végétaux." *Nouvelles Arch. du Mus. Hist. Nat.,* Paris, Tome 1, 1865, *cf.* p. 156.

such and such properties in a single individual, or on the contrary we can separate them. The biologist deals with these unit characteristics as the chemist does with atoms, or with lateral chains, in a complex organic compound. The properties which we distinguish thus are nothing but the very indirect external expression of constituent characteristics of the fundamental living substance of the species. But we imagine, and it is in this that the enormous importance of Mendelism consists, that it has been the means of giving us a more precise idea than we have had heretofore of a substantial basis for heredity. In itself, Mendelism is only symbolism, like the atomic theory in chemistry, but the case of chemistry shows what can be drawn from a well conceived symbolism and the Mendelian symbolism becomes more perfect each day in its form, in its conception and in its application. The recent works of T. H. Morgan[5] are particularly interesting in this respect.

Further, the facts furnished by Mendelism agree well with those of cytology. The results are explained easily enough, if we accord to the chromatine in the nucleus and particularly to chromosomes, a special value in heredity. The agreement of cytology and of Mendelism is incontestably a very convincing fact and a guide in present research.

But if we return now to the study of evolution, the data of Mendelism embarrass us also very considerably. All that it shows us in fact is the conservation of existing properties. Many variations which might have seemed to be new properties are simply traced to previously unobserved combinations of factors already existing. This has indeed seriously impaired the mutation theory of De Vries, the fundamental example of the *Œnothera lamarckiana* seeming to be not a special type of variation, but an example of complex hybridization. The authors who have especially studied Mendelian heredity find themselves obliged to attribute all the observed facts to combinations of already existing factors, or to the loss of factors, a conception which seems to me a natural consequence of the symbolism adopted, but which hardly satisfies the intelligence. In any case, we do not see in the facts emerging from the study of Mendelism, how evolution, in the sense that morphology suggests, can have come about. And it comes to pass that some of the biologists of greatest authority in the study of Mendelian heredity are led, with regard to evolution, either to more or less complete agnosticism, or to the expression of ideas quite opposed to those of the preceding generation; ideas which would almost take us back to creationism.

Lamarckism and Darwinism are equally affected by these views. The inheritance of acquired characters is condemned and natural selection declared unable to produce a lasting and progressive change in organisms. The facts of adaptation are explained by a previous realization of structures which are found secondarily in harmony with varied surroundings. That is the idea which different biologists have reached and which M. Cuenot in particular has developed systematically.[6]

Two recent and particularly significant examples of these two tendencies are furnished us by W. Bateson and by J. P. Lotzy. In his "Problems of Genetics," Bateson declares that we must recognize our almost entire ignorance of the processes

[5] *Cf.* Morgan, Sturtevant, Muller and Bridges, "The Mechanism of Mendelian Heredity," New York, 1915.

[6] Cuenot, "La Genèse des espèces animales," Paris, Bibliothèque Scientifique Internationale (Alcan), 1911.—"Théorie de la préadaptation," *Scientia*, Tome 16, p. 60, 1914.

of evolution, and in his presidential address at the meeting of the British Association in Australia, in 1914, he goes so far as to express the idea that evolution might be considered as the progressive unrolling of an initial complexity, containing, from the first, within itself, all the scope, the diversity and all the differentiation now presented by living beings. As Mr. Castle cleverly expressed it, carrying the idea to its logical issue, man might be regarded as a simplified ameba, a conclusion which may well give us pause. Here we clearly recognize, on the other hand, modernized in form, but identical in principle, the conception of the "enboitement" of the germs, and of preformation, ideas to which, as I have reminded you, the eighteenth century applied the name evolution. It is a conception diametrically opposed to that of the transformism of the nineteenth century.

Mr. Lotzy, struck by the results of the crossing of distinct species of *Antirrhinum*, has reached in the last three years the conclusion that a species is fixed and that crossing is the only source of production of new forms. Hybridization among species, when it yields fertile offspring, may, according to him, give rise, all at once, to a whole series of new forms, whose mutual relations and differential characteristics correspond exactly to what the natural species show.

However subversive and delusive ideas of this kind, positive or negative, appear to generations saturated with Lamarckism and Darwinism, we must not lose sight of the fact that they were formulated by eminent biologists, and that they are the result of long and minute experimental researches and that many of the facts on which they rest may be considered as firmly established.

But without thinking of rebelling against the facts resulting from genetic studies, we may question, whether they have so general a significance. I have already more than once pointed out that the present aspect of organic heredity does not oblige us to conclude that it has always been the same. We may ask ourselves whether conditions, which have not yet been realized in experiment, do not either modify directly the germinal substance itself, or the correlation existing between the parts of the *soma*, and indirectly through them the germinal substance. The facts which the study of internal secretions are just beginning to reveal, perhaps indicate a possibility of this kind. Even if we admit that evolution proceeds only discontinuously by mutations, we still have to discover the mechanism of the production of these mutations. In short, we may believe that, with heredity and variations acting as recent researches have shown them to act, there are nevertheless conditions that are still unknown and that they have been realized for each series of organisms only at certain periods, as seems to be suggested by paleontology, and in which the constitution and properties of hereditary substances are changeable. Of course these are purely hypothetical conjectures, but such conjectures must be made if we wish to reconcile two categories of already acquired data which we are obliged to recognize as facts. On the one hand we have the results of modern genetics which of themselves lead to conceptions of fixity, and on the other hand, the mass of morphological data which, considered from a rational point of view, seem to me to possess the value of stubborn facts in support of the transformist conception. I will even go so far as to say in support of a transformism more or less Lamarckian.

It seemed to me necessary to devote the first meeting of the course to this general analysis of the conditions under which the problem of transformism now presents itself. I believe that this analysis is the justification of the course itself. It shows the advantage of confronting in a series of lectures the old classic data with the modern tendencies, all of which have to be brought into agreement. The crisis of transformism which Le Dantec announced some eight years ago is very much more acute and more in evidence now than it was then. In making this analysis, I have been able to furnish you in advance with an outline to the following lectures which together will form four successive parts; first, a rapid examination of data contributed to the support of the transformist conception by morphology in its different aspects (comparative anatomy, embryology, paleontology); second, the examination of the principal dynamic explanations of transformism, above all Darwinism and Lamarckism; third, a study of the main principles of genetics, and fourth, a few final lectures in which we shall review all the data.

A course on evolution might seem *a priori* a hypertrophy to a program of studies, and in fact it is nothing but an extremely restricted scheme for examining important questions and the many investigations which this line of study has brought forth. All I can do, then, is to confine myself to a general view of the question, limiting myself to facts and essential data.

M. CAULLERY

SIR CLEMENTS R. MARKHAM

SIR C. R. MARKHAM, the famous geographer and explorer, who died in his London home, January 30, from burns caused by the overthrow of a candle, was in many respects a very remarkable man and his services to his fellows deserve to be widely known. He thought so little of himself that he did not trouble even to have a correct notice in "Who's Who in Science," nor did he talk or write of his own doings, so that, having survived most of his contemporaries, few were aware how much the modern world is indebted to him. Due to his sagacity and enterprise was the introduction of the quinine-producing shrub in India and the East; through his energetic work for twenty-five years as secretary to the Royal Geographical Society, and later as president, there was a vast increase in geographical knowledge and scientific exploration, whilst his published books on many diverse subjects were almost all on original ground. They would form an excellent course of study for any young man desirous to train mind and judgment on a good foundation. Each is a mine of careful research and accurate information, with utmost simplicity in presentation. There is no writing for effect and no self-exploitation; the narrative flows along easily and the reader can enjoy it as evidently the writer did.

Born in 1830, the son of the Vicar of Stillingfleet, Yorkshire, he entered the navy in 1844 and began his adventures hunting Riff pirates in the Mediterranean. In 1850, when the expedition in search of Sir John Franklin's party was preparing, he applied to join, and being refused on account of his youth, it is said that he sat down on the steps of the Admiralty and declined to move until the decision was reconsidered. Leaving in May, 1850, they returned in the autumn of 1851, having explored 300 miles of coast to about meridian 115 degrees on Melville Island, and in "Franklin's Footsteps" (published 1853), young Markham gave a spirited account of all they had seen. After wintering on Griffith Island, parties were sent in different directions over the ice, dragging by hand sledges with their limited provisions; McClintock's party covered 770 miles in 81 days, going 300 miles in a direct line from their ship. Markham was with a small party who went 140 miles in 19 days with one sledge. No wonder he spoke with genial scorn at a recent British Association meeting, of the modern polar explorer with every contrivance for comfort.