be slight or extensive, fleeting or permanent. The extent of the paralysis at the beginning of the attack is no accurate measure of its endurance. Recovery from paralysis on a wide scale is not only possible but often takes place.

RECAPITULATION

The microbe of infantile paralysis is known to belong to the class of invisible, filter-passing micro-organisms to which the name of viruses is applied.

This virus has been found in the secretions of the nose and throat of persons ill of infantile paralysis and of well persons in intimate contact with the sick.

When the virus is applied to the nose and throat of monkeys it passes along the connecting nerve fibers to the brain and spinal cord and induces paralysis similar to that occurring in the human disease.

That communication of the disease from person to person is brought about by personal contact and the transfer of the secretions of the nose and throat of the sick to the well has been established by observation of human epidemics and by experiments on monkeys. Whether or not any other common manner of communication of the disease to man exists is not known. Present public health measures of control of infantile paralysis are based on this mode of personal infection.

An attack of infantile paralysis is protective for life, irrespective of the intensity of the attack.

Persons who have had infantile paralysis possess in their blood certain protective or healing substances which can be used effectively to treat persons sick of the disease, and perhaps to prevent the disease in other and exposed children. It is the fluid portion of the blood that is employed in this way under the name of convalescent serum.

Since many normal adults develop immunity to infantile paralysis as a result of exposure to the virus under circumstances not leading to obvious disease, their blood serum also carries, at times, the protective and healing substances. The serum of these adult persons, which is abundantly available, may sometimes be substituted for the serum of convalescents, which is necessarily limited in quantity.

There are strong reasons for believing that a gradual immunization of the population of the United States is taking place as a result of the epidemics of infantile paralysis which have prevailed in different parts of the country since the large Swedish-Norwegian outbreak of 1905.

The virus of infantile paralysis acts upon the nervous system and especially upon the nerve cells of the spinal cord which control muscular movements. The muscles themselves are not directly affected. Since the virus injures the nerve cells and adjacent tissues with varying degrees of intensity, the effects on the muscles range from very slight to severe paralysis. Even when the paralysis is severe, restoration of motion takes place in part or even wholly as the injurious consequences of the disease subside.

Although the name—infantile paralysis—carries the implication of actual loss of motion by muscles, yet many cases of the disease never show paralysis at all. Indeed, there are reasons for believing that the cases of the non-paralytic disease exceed greatly in number those in which actual paralysis occurs.

Infantile paralysis is mainly but not wholly a disease of childhood. Adults are affected but infrequently. Now that we have learned that young children have rarely and older children and adults have often become immunized through unperceived or subclinical effects of exposure, we can better understand the peculiarities of age and place susceptibilities.

GENETICS AND EVOLUTION[1]

By Professor MAURICE CAULLERY

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IT seems to me that one of the functions of a congress such as this one, where for the moment many zoologists of all nations and all interests are gathered, is to examine the situation presented by the great problems which change from time to time but which are never completely solved. Such a study is particularly useful in certain phases when our diverse doctrines tend to clash or are not altogether incompatible. Now this is actually the case at the present time with the problem of evolution. Since the peculiar character of my work as a teacher at the University of Paris, devoted as it is to the study of evolution, has led me to constant reflection on the subject, I thought you might permit me to submit some aspects of more timely problems to the consideration of the congress at this reunion of zoologists.

Indeed the nature of the researches most in favor at the present time compel us to face a situation that exists in other congresses (especially the Genetical) where the problem of evolution is brought up indirectly, and where the mass of zoological data is

[1] Address at the opening of the eleventh International Congress of Zoology, Padora, September 4, 1930. Translated from the author's manuscript by J. C. Greenway, Jr.

screened behind results of special methods, a procedure which often leads to paradoxical conclusions. Certain eminent geneticists are even beginning to think that the conclusions of their scientific studies, being based on precise and methodical experimentation, should force us to abandon the idea of evolution whenever their work contradicts it. Others, without going so far, conclude that evolution should only be accepted to the extent that it is in strict conformity with genetical laws, that all conclusions to which zoology and paleontology lead should be deliberately excluded. I do not flatter myself that I have any solution of the difficulty to offer, nor even a new point of view. I should like merely to sail close to the wind, as sailors say, and to confine myself to considerations of a general nature, and perhaps to provoke useful reflection and discussion. I have already had occasion to set forth my views and three years ago, I heard a botanist, Professor R. von Wettstein, express analogous views at the Berlin Genetical Congress. Even this year I noticed that some of these ideas had been presented at a meeting of the combined German Genetical and Paleontological Societies. There is, moreover, a general uneasiness in regard to these ideas, which, it seems to me, may well have an echo here.

The beliefs held when the generation to which I belong did its apprenticeship in science were very different from those of to-day. Then the problem of evolution seemed much nearer solution than it does now. The theory itself was no longer seriously contested, nor did we hesitate to reconstruct the phylogeny both of larger and smaller groups by means of genealogical trees and a general application of the fundamental biogenetic law. Haeckel was the prophet of those days which have now begun to be remote. To-day we are far more cautious in this matter.

In regard to the mechanism of evolution, too, we believed ourselves to be much nearer the truth. It seemed that we had only to choose between the theories of Lamarck and Darwin, or to complete the doctrine of one of these great forerunners with that of the other. Was not Darwin in his later years the first to recognize that he should have made greater concessions to Lamarck's ideas? Environment appeared without question to be the most powerful agent for the transformation of organisms and it moulded them adaptively to the environment under the control of the struggle for existence and natural selection. When one rereads the works of that time, written by the most orthodox of Darwinists (for example, the beautiful books of A. R. Wallace, inspired by his zoological explorations in the Malay Archipelago), one feels on almost every page that these views are in a sense automatic.

August Weismann initiated a change of capital importance about 1885 when he tore down the supposed proofs of the heredity of acquired characters, proofs that were scarcely thought necessary, so obvious did they seem. And we must surely recognize that, in spite of all efforts since that time, the proof of such heredity, even in limited cases, is still to be furnished. The opposition which Weismann remarked between soma and germ-plasm, and from which he deduced the theoretical impossibility of the inheritance of acquired characters, is undoubtedly too radical. And yet we can not deny that his contention has been at least one of the pivots of biological thought in the last decade, and that his theory of the germ-plasm was a remarkable anticipation of future modes of thought. It affected the ideas underlying cytological work and the most important experimentation that has been done since the beginning of the twentieth century under the combined influence of the concept of mutation, introduced by Hugo de Vries, and the laws of Mendel which had been rescued from oblivion at the same time. On this double foundation the science of genetics has reared itself, and you are well aware of the important place that it holds in contemporaneous biology.

Let us recognize without any mental reservation or restriction whatsoever the value of the results of genetical investigations and the considerable progress that they represent. Into an indefinitely complex field like that of heredity, where empiricism reigned, gratuitous statements were made and speculative views advanced, the science of genetics has introduced methodical observation and strict experimentation. The result has been that certain general and simple laws have been detected for phenomena of heredity which once seemed essentially arbitrary. Thanks to these laws, we are now enabled to prognosticate with certainty, and this is the most unquestionable criterion of true scientific knowledge. It therefore behooves us neither to cast doubt upon nor deprecate the value of the results of genetics but only to endeavor to determine the real scope of their application.

We may say that a first series of these results, which constitutes, as it were, a gateway to genetics proper, has its source in analytical experimentation on variations and furnishes us with a more precise notion of species. Nowadays we distinguish much more clearly two categories of variations; on the one hand those that are purely individual and not hereditary, *i.e.,* of the phenotypic order, as we say. We have ascertained that in general they are due to the action of environmental factors, that is, of factors external to the organism, and, viewed statistically, obey certain simple laws and are explainable by polygons of variation or Quetelet's curves. On the other hand, there are variations due to differences in hereditary constitution, or, as we say, genotypic. The species is not a genotypic unity but a collectivity of genotypic constitutions; the units differ from one another in details and remain distinct in cases where self-fertilization is the rule (these constitute the pure lines of Johannsen), or, in the most frequent cases where cross fertilization has taken place, they may be mixed and combine among themselves indefinitely. The Linnean species may be separated into secondary units, which are theoretically as numerous as the distinct genotypes, *i.e.*, to an indefinite number. Practically speaking, this amounts to recognizing the legitimacy of the conception of an elementary species, which had been so remarkably clarified in a series of plants by the botanist, Alexis Jordan. For this reason the name "Jordanon" has been suggested for the fundamental genotypic unity.

Obviously genotypic properties can best be studied in the pure lines. This problem in Johannsen's work has led the author to a double conclusion; first, that the genotype is altogether independent of outside influences, which merely produce the individual variations or phenotypes; and second, that selection has only the power to isolate, without changing, the genotypes which are pre-existing but confused in a population. In fact, these conclusions of Johannsen's are the result of observations on very limited material (the pedigreed culture of a limited number of strains of Princess beans during a few generations). It is certainly a very bold induction to extend—*mutatis mutandis*—to all animals and plants and even to cases of cross fertilization. Nevertheless, it may be admitted that it is likely enough that they have at least a certain general value in the present state of organisms. On the whole these conclusions amount to a deductive announcement of the fixity of the genotype; that is to say, he concludes that species are fixed in relation to their environment, and this is a downright repudiation of Lamarckism.

Genetics proper has been the experimental analysis of the genotype by the method of crosses. Its principal results are now classic. Henceforth we may consider as solidly established the fact that the constituent properties of the genotype depend primarily on the nucleus and more especially on the chromosomes. Every one knows how far Thomas Morgan and his collaborators have been able to push this analysis in *Drosophila melanogaster*. As a result of these magnificent researches, which look upon genes and their localization as tangible realities, a veritable genetical mentality has been created. These, however, have merely imaginary existence. Be this as it may of genes and their positions, the mass and precision of concordant and experimental results deduced from this conception indicate that there is at least some correlation between it and reality. Everything happens as if genes were exactly as the geneticists say they are. They permit of experimentation and prognostication, and this justifies their use, but it must not be forgotten that they are only symbols. They have permitted us to clarify the idea of mutation and its treatment. If we put aside the very special case of *Oenothera*, which has made its fortune, mutation constitutes a phenomenon, examples of which have been cited over a period of years. Darwin knew them under the name of "sport" and "single variations." We have been able to straighten out long series of facts from investigations of the nineteenth and even the eighteenth century, and some have been found of a much earlier date. A great number of our cultivated plants and races of domestic animals are mutations perpetuated from olden times by human beings. However, it is genetics alone that has yielded a good understanding of their properties, and by means of the Mendelian theory of crosses, has permitted us to tie up their breeding with the alteration of a gene or a limited but definite number of genes. They are, on the whole, genotypical variations.

The special value of mutations lies rather in their hereditary character than in their discontinuity. For if these mutations, which correspond to large variations attracted attention first, we now know that mutations can manifest themselves as small variations with the appearance of continuity. The method of crossing enables us to bring about their transmission and to group them *ad libitum* under the limitation of their correlations. You know how successfully this has been accomplished in *Drosophila*.

Knowing that mutations are hereditary variations, which represent distinct typical forms of a species, we are led naturally to look upon them as the processes by which new and lasting forms have come into being. Admitting, on the other hand, that variations due to the environment are individual and not transmissible, and that phenotypical modifications can not be repeated by the genotype, mutations appear to be the principal if not the only form of evolutional change. The Darwinian theory can be applied to them in the sense that in all produced mutations natural selection has eliminated the unfavorable to the advantage of the favorable.

Since, owing to the proof of the existence of mutations themselves and of their hereditary peculiarities, genetical laws are positive facts of experience, the doctrine of evolution appears to rest on a solid basis of experiment.

First of all, it appears from the various results of genetics that the plasticity which Lamarck attributed to species is real only from the individual and phenotypical point of view, but, when envisaged in a succession of generations, the species is on the contrary quite stable. It is characterized by a totality of genotypical and fixed attributes, and occurs in varied environments under more or less diverse phenotypical forms. The species is therefore a reality and not an individual, nor is it always susceptible to change in a continuous or arbitrary fashion under the influence of external factors which constitute the environment. The objections made by Lamarck's contemporaries to the identity of the hypogea of Egypt together with those of our time are of undoubted value. And we may even go further, since paleontology itself proves the stability of various forms during often extremely long geological periods, and sometimes even from the remotest ages. Thus there are certain ants from the amber deposits of the Baltic, that is to say from the Oligocene, which are scarcely different from the ants of the present, and therefore the typical ant was already completely and definitely developed at that time. In general, therefore, species are stable, at least in the present epoch.

Do mutations, as far as is known, actually exhibit a process leading to the formation of new species, and do they account for evolution? Such is the opinion of geneticists, and it has been developed in a masterly manner by Monsieur Emile Guyenot, professor at the University of Geneva, in a work which recommends itself by its lucidity of exposition, accuracy of documentation, and faithfully adheres to the actual facts.[2] However, I do not believe that mutations and genetical laws are sufficient to account for evolution, as it is accounted for by morphological and paleontological data.

First of all, as we know them, and as genetics has explained their growth, it is doubtful whether mutations really constitute new developments in the descent of a species. Moreover, they appear to represent rarely occurring combinations of normal elements of the genotype, that is to say, they are forms virtually contained and preexisting within the species considered as stable. Evolution, on the contrary, ought to be conceived as brought about by the production of new types which were not preceded by similar forms. The mammalian type is not really contained in the fish nor the reptilian type.

Now mutations are attributed to modifications of already existing genes and it is significant that we have almost always been forced to consider these modifications as degradations or losses. This follows from the fact that from the Mendelian point of view a large majority of known mutations behave like recessive forms. Dominant mutations are much rarer.

[2] E. Guyenot, "La Variation et l'Evolution," 2 vols., Paris, 1930 (Encyclopedia Scientifique, G. Doni, editeur).

Recessive forms are not apt to evolve since they usually will be transmitted in a heterozygote condition in which they are masked. Crosses intentionally combined by the experimenter are essential if they are to be perpetuated and revealed in the homozygote condition. Moreover, mutations which are continually occurring in a state of nature are hardly ever met with there, or fail to maintain themselves. Practically speaking, we do not find the very numerous mutations of *Drosophila* which Morgan has isolated and propagated in his cultures. Then, too, almost all the mutations obtained are malformed or weak individuals which would quickly be eliminated by selection in a state of nature. Furthermore, the better one knows the genetics of a species like *Drosophila* the more numerous appear the non-viable and lethal mutations. The number of those that are able to maintain their place in the sun without the aid of man is very small. Is there not a profound reason for this state of affairs? Is there a reason for the numerical predominance of recessive over dominant mutations and for the fact that the majority of them have defective constitutions? Is it necessary to see an essential and elementary process of evolution in a phenomenon which appears to our eyes, in the majority of cases at least, to be subpathological?

The following is an objection of another sort. Are mutations, as at present understood, really evolutionary processes, that is to say, are they capable of giving rise to distinct species and of eventually splitting up into diverse groups?

Without doubt all the mutations exhibited by a species correspond well enough morphologically to the diversity among natural species or even among allied species in the same genus. But if we examine them from the point of view of physiology we find, notwithstanding the often very considerable variation of form and structure that they assume, that they nevertheless remain strictly within the frame of the stock species. This is shown by the general and unlimited fecundity which they exhibit in crosses with the parent species. Many of Morgan's *Drosophila* mutations (like the one with vestigial wings, for example) differ in a much more striking manner from the normal form than do the various species of wild *Drosophila* (*D. virilis, D. confusa,* etc.). Nevertheless the close sexual attraction to the normal form holds good and fertility is wholly preserved, whereas, on the contrary, crossing does not occur or give results among wild species which are hardly distinguishable from one another. The criterion of fecundity, in spite of the numerous and important exceptions which might be cited to the contrary, is perhaps still the surest criterion of specific difference, and the sterility of the crosses or the absence of sexual affinity is the fact which permits natural species to exist side by side. Mutations do not, therefore, result in the formation of distinct physiological populations as would true species. Therefore, why look upon them as the essential processes by which evolution, that is, the production of independent species, has been brought about? Genetics, nevertheless, readily explains why this should be so. From the genotypical point of view a mutation differs from the normal stock only by a single or a very limited number of genes; all the others remain identical. On the contrary, two closely allied natural species differ by the ensemble of their genes, that is, by their whole genome, which almost always reveals itself materially by the number of the arrangement of the chromosomes. And this gives us sufficient insight into the infertility or the limited fertility of the cross. When, exceptionally, crossing is possible and normally fertile between distinct species, as Erw. Baur has observed among species of *Antirrhinum,* then the second generation, F_2, exhibits enormous polymorphism. From one single hybrid seed (F_1) the issue of one of these crosses, Baur was able to secure in the (F_2) generation more than 150 different types. Now this is interpreted by supposing that the two crossed species differed by a great many genes, and hence a considerable multiplicity of distinct combinations among the gametes. And certain geneticists, like Lotsy, starting from this conclusion, are seeking the source of the production of new types and of all evolution in hybridization.

However, be this as it may, mutations do not arise from a process corresponding to a fundamental genotypic change of an order of magnitude like that which separates one natural species from another. Are we justified then in seeking among the phenomena of mutations for the sole and fundamental laws of evolution? Or rather are not these laws of a much more complex character, the mutations entering into them as a partial element, but without thereby implying the exclusion which genetics is leading us to formulate?

It is significant that when we have started with mutations and genetics and tried to formulate an explanation of evolution, we have only arrived at disconcerting paradoxes. This was the case of the late lamented William Bateson, one of the most eminent geneticists, and who is still considered to be one of the masters of contemporary biology. Almost all the mutations known to him being recessive, he explained them by the loss of a gene, on the genetical theory which he called "presence-absence," and which to-day I admit has been abandoned. We should have to admit, therefore, that new forms resulted from successive losses in their genotypes, that is to say, that evolutionally superior forms of the animal and vegetable kingdom are due to a progressive simplification of the initial complexity of the most primitive and inferior forms. Thus man would seem to be a simplified *ameba.* No doubt there is a touch of humor in the idea, although it revealed an embarrassment in constructing a theory of evolution on the basis of mutations.

One other grave difficulty with the conception that evolution is fundamentally the result of mutations, to the exclusion of all Lamarckian mechanism, is the explanation of adaptation, that is, the frequently astonishing structural conformity between organisms and their conditions of existence.

Adaptation, which is one of the most difficult questions in biology, has been much discussed of course in the past and even recently. Formerly naturalists saw it everywhere and marvelled at the wisdom of Providence which had endowed every living being with all the organs best suited to its rôle in nature. Such was even Cuvier's point of view. Lamarck, on the contrary, announced that adaptations were *a posteriori* results, an effect on the organisms of environmental conditions which modeled them. To-day, it is well established that adaptation is not a universal characteristic of organisms. There are many and important details in every organism that have no adaptive significance whatsoever, but which even seem to be maladapted. Obviously the organism gets along as well as it can with the organs which it possesses and its manner of living is regulated by its organs. Certain biologists are even beginning radically to reject this morphological order and to consider all evidence brought to bear in support of it as pure illusion due to coincidence. I am unable to subscribe to this point of view. An organism such as a cetacean or a bird is manifestly the result of transformations of an original type, which are strictly correlated with an aquatic or an aerial mode of life. The reality of adaptation being admitted, its realization is as difficult as it is important. The Lamarckian solution pure and simple, according to which the need suffices to create the organ, can not be adopted. Furthermore, the most clearly adaptive characteristics of the individual are apparent in ontogeny before any functioning of the organs. The mutation theory permits us to regard adaptation only as the fortuitous result of variations non-adaptive in themselves but which are preserved by selection in the event that they are found to be favorable. This was Darwin's concept also. But can we admit that a series of chances has developed such highly coordinated wholes as the body of a cetacean or that of a bird, or can mere chance have set up certain organs which are veritable machines, or tools such as the human intelligence constructs. To explain these facts it seems to me that it is impossible to reject the direct action of the environment on organisms or the influence of the phenotype on the genotype, that is to say, a mechanism of the kind suggested by Lamarck. Every theory of evolution should be able to explain even circumscribed adaptation, but I doubt whether it can be done by genetics.

There are then undeniable difficulties in explaining evolution in general by means of genetical data and the phenomenon of mutation. Therefore, what attitude shall we adopt?

There are geneticists who remain strictly in the territory of the established facts and experimental results and who deliberately sacrifice everything that stands in their way. If the genetical mutationist can give no account of evolution, and evolution is a mere hypothesis, then there is no alternative but to sacrifice this hypothesis. Such is the conclusion at which certain eminent geneticists such as Mr. Heribert Nilsson are arriving.

Evidently there would be no disposition to accept such conclusions on the part of the majority of zoologists and botanists. For them evolution, as a fact, obviously results from all data of comparative anatomy, embryology and paleontology. Naturally I shall not enumerate the arguments, which are classic, that support this statement. Nevertheless, we are clearly unable to avoid taking account of the positive results of experimentation. But what seems to me to be of vital importance is their true significance.

However extensive experiments of this sort may be—the most ample are Morgan's on *Drosophila*—they are always extremely limited in comparison with the totality of natural facts and we are always able to ask ourselves whether the best conceived experimentation really embraces all the natural conditions, not only of actual present conditions but also those of the past. Alfred Giard, one of the biologists of the end of the last century who had the widest knowledge of zoology and botany and the most penetrating insight into evolution, was in the habit of poking fun (and with good reason) at certain botanists, who, he said, could not recognize the crucial value of an experiment unless they had repeated it in a flower pot. Nature, he added, shows us irrefutable and clearly significant experiments which she has made and which we can never dream of reproducing, but which we can not fail to interpret. Thus, by parasitism, she has converted a cirriped into a rhizocephalan, such as *sacculina*. Now all the essential phenomena of evolution are of this kind; they are experiments which require such a long time for their performance as to be impossible for us. But even if we reject this subterfuge, nature herself probably no longer performs such experiments at the present time and has realized them only at certain epochs without our being able to discover the reason. She does not keep repeating them continually. The period during which terrestrial mammals related to the carnivores passed over to a marine life and became pinnipedia and cetaceans is confined to a limited portion of the earth's history. We may make every effort to breed dogs or even such purely aquatic animals as otters during many successive generations without being able to produce seals, porpoises or anything approaching them. Moreover, it is not in a continuous or repeated manner that the species of crustaceans, cirripeds, copepods or isopods adopted a parasitic way of life and underwent the transformations which produced the rhizocephalia, the various parasitic copepoda and the epicaridae. Paleontology is a witness to the fact that all these evolutional changes are brought about in each group only during a limited period, and, in most cases, an extremely remote one. It is even conceivable that they have been evolved with relative rapidity, after which everything indicates that they had attained a perfectly stable equilibrium, as genetics has clearly demonstrated. What conditions have influenced these transformations, the reality of which is evident, we do not know. We know only that evolutionary changes are not brought about in all groups at the same time. The reptile type, which had become so highly diversified at the beginning of the Mesozoic period, remained perfectly fixed and stable at the beginning of the tertiary, in the many forms which are still surviving to-day, at the same time that the diversification and evolution of mammals was going on in large and rapid strides. Without ignoring the fact that to establish an evolution the past must be considered and the present must always, *a priori,* give an illusion of stability, we may seriously ask ourselves whether evolution has not been completely accomplished and whether the organisms whose stability and fixity are established by genetics, have not lost the mysterious ability of transformation, at least to a considerable extent, in the present stage of the earth's history. As the paleontological past teaches, the evolution of groups is limited, as Dollo has so ably contended. Evolution is not a process that is carried on indefinitely or continuously or at a uniform speed. Every group has had its stage of differentiation, after which it is congealed, as it were, into already acquired forms. We ought to ask ourselves if the characteristics we recognize in existing organisms are identical with those of the geological epochs of which paleontology gives us such an adequate picture. It is possible that at that time the phenotype had an effect on the genotype and that, in accordance with the general sense of Lamarck's theory, if not in accordance with its formulation, an environment contributed to the modification of organisms which transformed themselves, moreover, to a great extent independently of the environment, in conformity with the correlations resulting from their intimate structure. It seems that at the present time we do not know whether stabilized nature and genetics will inform us of the modalities of this stability. Are these conclusions of genetics valid for the periods and the conditions during which each group became diversified? Or rather, as seems more probable to me, do the evolutional transformations depend on some other causes which still elude us? I am not concealing from myself the fact that it is very improper to imagine that the causes known at present are insufficient to explain the past and I ask pardon. But I still prefer to adopt such a supposition rather than to deny evolution or to confine myself to a statement of the contradictions between the results of our inadequate experimentation and the facts attested to by the past.

I ask your indulgence for having preempted your attention so long, only to end with such doubtful conclusions; but, as I said in the beginning, my intention was above all to emphasize the difficulties of the problem and to provoke reflections, suggestions and even contradictions among the experts present.

SCIENTIFIC EVENTS

THE INTERNATIONAL CONFERENCE ON BITUMINOUS COAL

THE Third International Conference on Bituminous Coal will be held at the Carnegie Institute of Technology from November 16 to 21, it is learned from Dr. Thomas S. Baker, organizer of the meeting and president of the Carnegie Institute of Technology.

Prospects for the third congress are excellent. "Because of the deep concern that is felt all over the world as a result of the great depression in the coal industry, it is felt that it is a particularly appropriate time to hold our conference," Dr. Baker said. "There has been some pressure to have the meeting postponed for another year. These suggestions have come principally from some of the European scientists, but it is thought that because of the condition of the industry it is very important that we go on with our plans."

One of the objections of the foreign scientists to coming to this country this year, apart from the difficulty of securing necessary funds, is the fact that so many industrial plants are shut down that they will be unable to study American methods of business. In spite of these conditions, there will be a larger number of European delegates than were present at the previous conferences.

"The conferences in the past have been devoted to the scientific aspects of coal utilization," Dr. Baker continued. "As this meeting is sponsored by a technological institution, the emphasis has been placed on new methods of utilizing and treating coal which are continually being developed. When the first meeting was organized in 1926, it was undertaken with the hope and expectation that it would be of service to the coal industry and the subsequent meetings have been planned with this in mind.

"Although in comparison with the previous meetings, the scientific program next November will be of equal, perhaps greater, importance, it is impossible to discuss coal at the present time without reference to the economic aspects of the industry. Therefore the various processes that will be presented will deal very definitely with economics and less with theoretical questions. There will be a certain number of papers that will be solely economic in character."

The congress will unite scientific men from all over the world, who will bring to Pittsburgh the latest developments in soft coal utilization. Some of the foreign delegates will speak also on the coal industry as a business in their respective countries, and it is felt that suggestions will be made by them that will be helpful to American coal men.

The conference will be attended by representatives from Austria, Belgium, Canada, Czechoslovakia, England, France, Germany, Italy, The Netherlands, Poland, Roumania, Spain, Sweden, Switzerland, South Africa and U. S. S. R.

PAINTINGS OF PREHISTORIC LIFE AT THE FIELD MUSEUM OF NATURAL HISTORY

THE series of twenty-eight large mural paintings depicting life on the earth in successive prehistoric ages from about one and one half billion years ago down to the beginning of the modern era, which has been in the course of preparation for the Field Museum of Natural History during the past several years, has just been completed with the installation of the last three paintings, it has been recently announced by the director of the museum.

The paintings are a gift to the museum from Ernest R. Graham, an architect of Chicago, who provided a fund of $125,000 for them and certain other material illustrating historical geology. The hall in which they are exhibited has been named in Mr. Graham's honor by the museum's board of trustees.

Charles R. Knight, of New York, known as a fore-