IMAGE EVALUATION
TEST TARGET (MT-3)

CIHM/ICMH
Microfiche
Series.

CIHM/ICMH
Collection de
microfiches.

Canadian Institute for Historical Microreproductions

Institut canadien de microreproductions historiques

1980

Technical Notes / Notes techniques

The Institute has attempted to obtain the best original copy available for filming. Physical features of this copy which may alter any of the images in the reproduction are checked below.

L'Institut a microfilmé le meilleur exemplaire qu'il lui a été possible de se procurer. Certains défauts susceptibles de nuire à la qualité de la reproduction sont notés ci-dessous.

- ☐ Coloured covers/ Couvertures de couleur
- ☐ Coloured maps/ Cartes géographiques en couleur
- ☐ Pages discoloured, stained or foxed/ Pages décolorées, tachetées ou piquées
- ☐ Tight binding (may cause shadows or distortion along interior margin)/ Reliure serré (peut causer de l'ombre ou de la distortion le long de la marge intérieure)
- ☐ Additional comments/ Commentaires supplémentaires
- ☐ Coloured pages/ Pages de couleur
- ☐ Coloured plates/ Planches en couleur
- ☐ Show through/ Transparence
- ☐ Pages damaged/ Pages endommagées

Bibliographic Notes / Notes bibliographiques

- ☐ Only edition available/ Seule édition disponible
- ☐ Bound with other material/ Relié avec d'autres documents
- ☐ Cover title missing/ Le titre de couverture manque
- ☐ Plates missing/ Des planches manquent
- ☐ Additional comments/ Commentaires supplémentaires
- ☐ Pagination incorrect/ Erreurs de pagination
- ☐ Pages missing/ Des pages manquent
- ☐ Maps missing/ Des cartes géographiques manquent

The images appearing here are the best quality possible considering the condition and legibility of the original copy and in keeping with the filming contract specifications.

The last recorded frame on each microfiche shall contain the symbol ⟶ (meaning CONTINUED"), or the symbol ▽ (meaning "END"), whichever applies.

The original copy was borrowed from, and filmed with, the kind consent of the following institution:

Library of the Public
Archives of Canada

Maps or plates too large to be entirely included in one exposure are filmed beginning in the upper left hand corner, left to right and top to bottom, as many frames as required. The following diagrams illustrate the method:

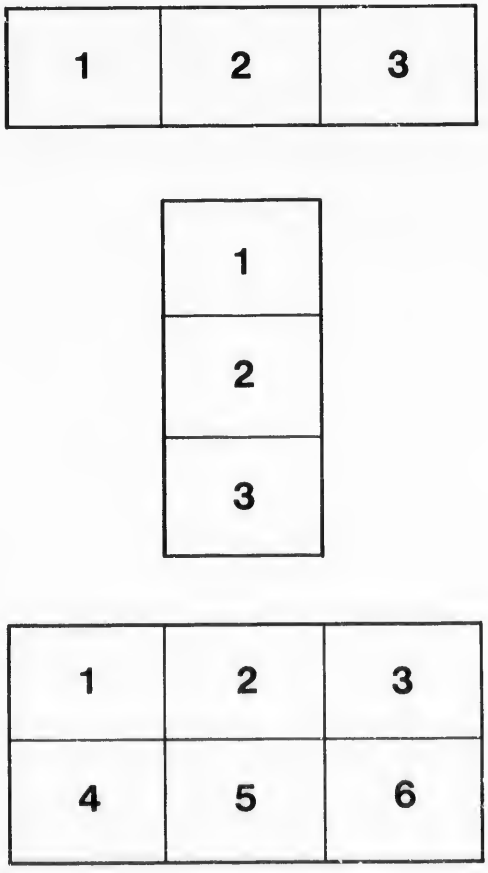
Les images suivantes ont été reproduites avec le plus grand soin, compte tenu de la condition et de la netteté de l'exemplaire filmé, et en conformité avec les conditions du contrat de filmage.

Un des symboles suivants apparaîtra sur la dernière image de chaque microfiche, selon le cas: le symbole ⟶ signifie "A SUIVRE", le symbole ▽ signifie "FIN".

L'exemplaire filmé fut reproduit grâce à la générosité de l'établissement prêteur suivant :

La bibliothèque des Archives
publiques du Canada

Les cartes ou les planches trop grandes pour être reproduites en un seul cliché sont filmées à partir de l'angle supérieure gauche, de gauche à droite et de haut en bas, en prenant le nombre d'images nécessaire. Le diagramme suivant illustre la méthode :



.24

MODERN IDEAS OF DERIVATION.

Address of Principal DAWSON, as President of the Natural History Society of Montreal. Read at the Annual Meeting, May 18th, 1869.

The sphere of this Society as a modest collector and preserver of local facts in Natural History, does not preclude its glancing at the more difficult and abstruse questions which agitate Naturalists elsewhere; and perhaps no place is more fitting for this than the annual address of the President. I propose, therefore, on the present occasion, to direct your attention to the present state of those exciting questions agitated in our day by Geologists, Zoologists and Botanists, as to the origin of Species and Genera, and the law of their creation.

Time was when Naturalists were content to take nature as they found it, without any over curious inquiries as to the origin of its several parts, or the changes of which they might be susceptible in time. Geology first removed this pleasant state of repose, by showing that all our present species had a beginning, and were preceded by others, and these again by others. Geologists were, however, too much occupied with the facts of their science to speculate on the ultimate causes of the appearance and disappearance of species, and it remained for Zoologists and Botanists, or as some prefer to call themselves, Biologists, to construct hypotheses or theories to account for the ascertained fact that successive dynasties of species have succeeded each

other in time. In our day, Darwin has given to such speculations a form and coherency which they did not before possess, by his doctrine of Natural Selection; and theories of derivation and transformation are perhaps more popular than at any previous time, and are impressing themselves legibly on the practical every day work of science. In these circumstances it becomes necessary to watch the phases of opinion on this subject, to examine the various doctrines propounded, and to ascertain what progress they are making, if any, toward the goal of truth.

A very important contribution to this work has recently been made by Professor Owen in the concluding chapter of his great book on Physiology, just completed; and I shall take this as the basis of some remarks on the present state of the question of derivation.

Prof. Owen, availing himself of the privileges of a father in Science, goes back to 1830 in reviewing the history of doctrines of derivation, and shows that in his student days the question of the origin of species was agitated by the great Cuvier and his contemporary, Geoffroy St. Hilaire, and that both of these great masters of Natural Science had doubts as to the permanency of species in geological time, though neither had before him enough of biological evidence to establish this as a fact, or to frame any certain theory as to the relation of modern to extinct species; and Cuvier, at least, saw evidence against derivation in the apparent want of connecting links between fossil and recent species. Owen endeavours to arrange the questions raised in 1830 under several heads, and to state each as then agitated, and to "post it up," so to speak, to the present period—his evident intention being to show that the views of Darwin and other recent advocates of theories of derivation are by no means so original as they are supposed to be.

The first great question agitated by the French naturalists forty years ago is that grand one — Is there unity of plan or final purpose in living creatures? Are the homologies or resemblances of structure in organized beings merely parts of the general plan, or do they point to genetic or other relations of derivation? Are the beautiful adaptations of organs to functions, and of organisms to places in nature, evidences of deliberate purpose working out its ends by means, or have the external necessities given form to the organs? On this question Cuvier, in his assertion of teleology, evidently took the broader and more

philosophical view, that which commends itself to the grander and higher style of mind; but neither he nor his opponent were in a position to see fully the bearings of the question. Owen himself, though largely in advance of most other writers of this time, is not free from misconceptions. He clearly sees, with all the more profound thinkers among naturalists, that whichever view we adopt, the problem can be solved only on the hypothesis of a "predetermining intelligent Will." Without this, nature is only a riddle without a solution—man himself a contradiction and impossibility. But, admitting this, are those resemblances which we call homologies, those adaptations which we call analogies, results of direct creative acts or of the operation of secondary causes? If the former, they are ultimate facts, referable directly to will; if the latter, we may study their more immediate causes, and the laws under which these operate. Cuvier and many of his most illustrious disciples have been content to adopt the former alternative. Owen declares that in this he has been led to differ from his great master. The reasons which he gives under this head are, it must be confessed, feeble. He found it necessary to assume an "archetype" or ideal type in explaining the vertebrate skeleton; but this would have been equally suitable under the hypothesis of direct creation or that of secondary causes. He saw in the recurrence of similar segments in a vertebral column and other cases of repetition of similar parts, something analogous to the repetition of similar crystals, as the result of "polarizing force in the growth of an inorganic body." But there is scarcely more philosophy in this than there is in the process by which a savage, ignorant of manufacturing processes, might explain, as the result of some unknown process of crystallization, the recurrence of forms in the pattern of a piece of calico or in the beads of a necklace. Still we are willing to allow due value even to the impressions made upon the minds of naturalists by such facts, and to go on to the next question of the series. Before doing so, however, we must take exception to one expression of the great English naturalist, which, in various forms, recurs in several places. He calls the theory of derivation a principle "more especially antagonistic to the theological idea" of creation. Now, if by the theological idea he means that promulgated in the first chapter of Genesis, he should explain wherein the antagonism consists. The object of the writer in Genesis is obviously to illustrate and enforce the existence and

attributes of the Supreme Intelligent Will as opposed to Polytheism, Pantheism and Atheism, and the fact of an orderly and serial origin of things. But if he says that animals were made "according to their kinds," has any modern naturalist a right to hold that the kinds or species of Genesis are equivalent to those of any school of zoologists in our day? Further, all who profess to be acquainted with this part of theology should know that the word "create" is applied in Genesis only to the first animals, and to man considered as an intelligent and moral agent. The other animals and all plants are said to have been "made," "formed," "brought forth," implying that the writer had before his mind the idea of a primary and secondary kind of origin of organized beings. I endeavoured many years ago, in a work well known to members of this Society, and published before Darwin's Origin of Species, to illustrate this old "theological idea." Since naturalists will bring up such subjects, I may be excused for reminding them that if they should come to believe, on zoological and geological grounds, that some of the entities which we call species have been produced by a method which may be properly termed creation, and others by secondary processes, they may possibly find themselves to be in perfect harmony with the oldest and most authoritative theologic ideas on the subject.

The second great question as to Derivation is that which relates to the succession of species in Geological time. Was this broken or uninterrupted? Did new species die out and were old ones created in their room, or were the new ones derived by some secondary process from those which preceded them? This question can only be finally settled by inductive investigation, and unfortunately our knowledge of extinct animals and plants is still too imperfect to give us the necessary accumulation of facts. We can only inquire as to a few cases a little better known to us than others. One curious feature of the inquiry is that it seems easier to show relationships between large groups of animals than between particular species. The reasons of this will appear farther on. Prof. Huxley, with his usual dexterity in presenting these problems to the popular comprehension, has recently taken advantage of this in tracing the links of connection between birds and reptiles.* By a series of cleverly arranged transitions, he has succeeded in constructing such a series as no doubt sufficed to

* Royal Institution Lecture on Animals intermediate between Birds and Reptiles.

convince many of his auditors that the gigantic and grotesque Iguanodons of the Mesozoic rocks might have been the progenitors, if not of wrens and titmice, at least of ostriches and cormorants. Yet he could not have placed together any two members of the supposed series without convincing any naturalist that an enormous gap had to be filled between them. Prof. Owen, writing to naturalists, does not attempt this sort of intellectual sleight of hand, but presents, as a case in point, the supposed progenitors of the horse. That useful quadruped was preceded in the tertiary period (Miocene and Pliocene) by a horse-like animal, the Hipparion, which, among other things, differed from its modern representative in having its splint bones represented by two side toes, a conformation supposed to adapt it to locomotion on soft and swampy ground. The Hipparion was preceded in the earlier tertiary (Eocene) by the Palæotherium, in which the side toes were still further developed so as to touch the ground, giving the foot a tridactyle character. These relations induce Owen to believe that these forms may be an actual genetic series, the species of Palæotherium passing through a succession of changes into the modern horse. Perhaps this case, as put by Prof. Owen, affords as fair an example as we can obtain of the bearing of a derivative hypothesis. The three genera in question are closely allied. They succeed each other regularly in Geological time. The horse shows in his splint bones rudiments of organs, which, serving little apparent purpose in him, were more fully developed and of manifest use in his predecessors. Modern horses have occasionally shown a tendency to develop the side toes, as if returning to the primitive type. Taking this as a fair example of derivation, and admitting, for the sake of argument, its probability, let us consider shortly some of the questions that may be raised with regard to it. These are principally two.

1. What limits, if any, must necessarily be set to such an hypothesis, and what relations does it bear to the origin of life at first and to the succession of animals in Geological time?

2. What causes may be supposed to have led to such derivation?

Under the first head, we have to enquire as to the limits set to derivation by the structure of organic beings themselves, and by the physical conditions and changes which may affect them. It will be convenient to consider these together.

Supposing that Palæotherium, Hipparion and Equus are links in a chain extending from the Eocene Tertiary to the present time, can we suppose that by tracing the same series further back it might include any Mammal. We must answer decidedly not, for if the whole time from the Eocene to the present has been required to produce the comparatively small change required from Palæotherium to horse, that in other cases would carry us back to the Mesozoic period, long before we have any evidence of the existence of "placental mammals." In other words, the Tertiary and Modern Periods will give us time enough only to effect changes of Mammals within the order Pachydermata, and perhaps in only one section of that order. The other orders must therefore constitute separate series, and these series must have been advancing abreast of each other. Had each series a separate origin, or is there any Mammalian stock in the Mesozoic from which, at the beginning of the Tertiary, these several lines of types may have diverged? Here our information fails. We know only a few small Marsupial Mammals in the Mesozoic. On our hypothesis it is possible that these may have been the progenitors of the more varied and advanced Marsupials of the Tertiary and Modern periods, but scarcely of the placental Mammals of the Eocene. There may have been placental Mammals, unknown to us, in the Mesozoic, which may constitute the required stock. The reptiles of the Mesozoic utterly fail to give us the necessary links. If they were changing into anything it was into birds, not into Mammals.

Again, the time in which the horse and its supposed progenitors have lived is one of continuous, unbroken succession of species. More especially in the later Tertiary there seems the best evidence of gradual extinction and introduction of species, without any very wide-spread and wholesale destruction, and this notwithstanding the intervention of that period of cold and of submergence of land in the Northern hemisphere, which has given rise to all the much-agitated glacial theories of our time. Can we affirm that such piecemeal work has continued throughout Geological time? At this point opens the battle between the Catastrophists and Uniformitarians in Geology, a battle which I am not about to fight over again here. I have elsewhere stated reasons for the belief that neither view can be maintained without the other, and that Geological time has consisted of

alternations of long periods of physical repose and slow subsidence in which our more important fossiliferous formations have been deposited, with others of physical disturbance and elevation, with extinction of species. Dana has well shown how completely this view is established by the series of Geological formations as seen on the broad area of the American continent. Now the question arises, how would the law of derivation operate in these two different states of our planet? Let us suppose a state of things in which far more forms were being destroyed than were reproduced—another in which introduction of species was more rapid than extinction. In the latter case we may suppose an exuberance of new species to have been produced. In the former there would be a great clearance of these, and perhaps only a few types left to begin new series. Do we now live in one of the periods of diminution or of increase? Perhaps in the former, since there seems to have been, in the case of the Mammalia of the Post-pliocene, an enormous amount of extinction of the grandest forms of life, apparently without their replacement by new forms. If so, how far can we judge from our own time of those which preceded it? They may have been far more fertile in new forms, or perhaps farther in excess in the work of extinction. The question is further complicated with that which asks if these differences arise from merely physical agencies acting on organic beings from without, or is there in the organic world itself some grand law of cyles independent of external influences? The answers to such questions are being slowly and laboriously worked out by Geologists and Naturalists, and all the more slowly that so many inevitable errors occur as to the specific or varietal value of fossils and the relative importance of Geological facts, while the great gaps in the monumental history are only little by little being filled up.

Nothing can more forcibly illustrate the amount of work remaining to be done toward the settlement of these questions than a glance at the elaborate and most valuable "Thesaurus Siluricus" of Dr. Bigsby, recording, as it does, nearly 9,000 species of animals already found in the Silurian rocks. The rapid increase in the number of known species shows that we know as yet but a fraction of this ancient fauna, while the facts relating to introduction, extinction, geographical distribution and distribution in time, show that we are still a very long way

from any definite conclusions as to the general law of succession and its relations to physical changes.

The application of these questions to the animals referred to by Owen, will serve farther to shew their significance as to limitations of derivation. Pictet catalogues eleven species of Eocene Palæotheria. Without inquiry as to the origin of these, let us confine ourselves to their progress. Under the hypothesis of derivation, each of these had capacities for improvement, probably all leading to that line of change ending in the production of the horse. If so, then each of our Palæotheria, passing through intermediate changes, may be the predecessor of some of the equine animals of the Post-pliocene and Modern periods. But if, as seems probable, the time intervening between the Eocene and the Modern was unfavourable to the multiplication of such species, then several may have perished utterly in the process, and all might have perished. Supposing, on the contrary, the time to have been favourable to the increase of such creatures, we might have had hundreds of species of equine animals instead of the small number extant at present. Again, what possibilities of change remain in the horse? Can he be supposed capable of going on still farther in the direction of his progress from Palæotherium, or has he attained a point at which further change is impossible? Will he then, in process of time, wheel round in his orbit and return to the point from which he set out? Or will he continue unchanged until he becomes extinct? Or can he at a certain point diverge into a new series of changes? We do not know any Palæotherium before the Eocene. Is it not possible that they may have originated in some way different from that slow change by which they are supposed to have been transmuted into horses, and that in their first origin they were more plastic than after many changes had happened to them? May it not be that the origin of forms or types is after all something different from derivative changes, and that new forms are at first plastic, afterwards comparatively fixed—at first fertile in derivative species, and afterward comparatively barren. Certainly, unless something of this kind is the case, we fail to find in the Modern world a sufficient number of representatives of the Palæotheria, Anoplotheria, Lophiodons, Elephants and Mastodons of the tertiary. On the other hand, it is scarcely possible to find a sufficient starting point in the

Eocene for the multitude of Ruminants and Carnivores and Quadrumana of the Modern time.

But it may be said, and truly, that these higher forms of life put the doctrine of derivation to the severest test. If we take marine invertebrata, we may trace analogues of these back into the earliest geological periods, and as the species are very numerous, and their structures more simple, it is easier to imagine a continuous derivation with respect to them. Still, even here such facts as the vast multiplication of species of Trilobites, Ammonites, Belemnites, and Ganoid Fishes, at different periods of Geological time, and their disappearance without modified successors, point to limitations of any law of derivation that may be suggested.

To sum up where all is so uncertain is not easy; but we may, I think, affirm that if existing animals are derivative as modified descendants of others—(1) They belong to a vast number of lines of modification which would require to be traced backward separately. (2) That many of these lines end abruptly in comparatively recent periods, perhaps in consequence of our defective information, perhaps because of some other law of creation. (3) That in some periods a series must suddenly bud forth into many ramifications, and in others contract to a few representations or be altogether dropped. (4) That the beginning of such series may take place in a different manner from derivation, and that the law of new series is probably different from that of those of longer derivation. (5) That it is absurd to suppose that any modern animal has originated from any now contemporary with it (e.g., man from the gorilla or bears from seals), since all these existing species must belong to series to be traced backward through species now extinct, and possibly unknown to us. (6) That while it is obvious that such derivation must be related to contemporary physical changes, our views of the nature of that relation must depend on those which we take of the causes of derivation itself.

Before proceeding farther we may remove another of the "theological" misconceptions under which Owen and some other writers on this subject seem to labour. They think that the "Biblical flood" interposes some difficulties in the way of their speculations as to the origin of species. They may readily be relieved from all embarrassment on this subject. The language of the Noachian record in Genesis implies that the Deluge was universal only in so far as man was concerned. The e[illegible] of

animals taken into the Ark, five times repeated, and that of animals destroyed, twice given, show that only a very limited number of species were in the Ark, and that of the rest some certainly survived—others may have perished. Farther, the catastrophe does not require us to suppose either that coral polypes and other marine animals were overwhelmed with fresh water or under an abyssal depth of ocean, for the submergence of the dry land, or of a portion of it, by the "breaking up of the fountains of the great deep," does not imply a deepening of the ocean, but possibly to some small extent a shallowing of it. If the Royal Institution, of London, which has recently done so much in its courses of lectures to ventilate new and sometimes questionable scientific hypotheses, would employ some one to give a few exegetical lectures on the earlier chapters of Genesis, without entering into any disputed questions of criticism, but merely explaining the literal meaning of the terms of the record, it would confer an inestimable benefit on those Naturalists who seem to have derived their notions of the Biblical Creation and Deluge from the picture books and toy Noah's Arks of their childhood, with the comments of their nursery-maids thereon.

It still remains to us to inquire whether the doctrine of derivation can throw any light on the origin of life at first. Nothing in the doctrine of derivation itself necessitates the belief that change has always been in the direction of improvement or of increased complexity; but the Geological history of the earth and the succession of fossils lead to the belief that the general tendency of creation has been from more generalized to more specialized forms, and from simpler to more complex organisms. Still, it is evident that this general doctrine of improvement is to be held with some limitations of detail. For example, the very lowest forms of life have continued down to the present, and some of them—for instance, the sponges and Foraminifera—have apparently attained to their greatest extension in number of species in comparatively late periods. Further, every new form when first introduced appears to be at its maximum in point of development; or, if not so, it rapidly attains to this, and again deteriorates when being supplanted by other and newer forms. Numerous examples of this will occur to every Geologist. Admitting, however, that development has in some cases been indefinitely postponed, and that in others it has advanced by successive waves, each retreating before the advance of the next,

still, we may hold that it would be fair to assume a gradual progress from lower to higher forms. Assuming this, and that the lower have preceded the higher, we may limit our inquiry as to the origin of life to the lowest forms, and ask what is involved in the question of their origin. Now, it is easy to affirm that the lowest animals and the lowest plants are but Protoplasm, which is only another name for one chemical compound Albumen, and that if we can conceive this to originate from the inorganic union of its elements, we shall have a low form of life from which we can deduce all the higher forms of vital action. In making such affirmation we must take for granted several things, none of which we can yet prove:—(1) That vital force is merely a modification of some of the forces acting in unorganized matter; (2) That such force can be spontaneously originated from other forces without the previous existence of organization; (3) That being originated, it has the power to form Albumen and other organic compounds. Or, if we prefer another alternative, we may take, instead of the last statement,:—(1) That Albuminous matter can be produced by the union of its chemical elements without life or organization; (2) That being so produced it can develop vital forces and organization, including such phenomena as sensation, volition, reproduction, &c. To believe either of these doctrines in the present state of science is simply an act of faith, not of that kind which is based on testimony or evidence, however slight, but of that unreasoning kind which we usually stigmatize as mere credulity and superstition. It will not help us here to say that vegetable and animal infusions, destitute of germs, will produce a "mucous layer" or "proligerous pellicle" from which organisms may arise, for in the first place such infusion itself contains organic matter, and, as Tyndall has lately shown incidentally in his experiments with the electric light, we have to operate with air and water and vessels, which it is wholly impossible by any chemical or mechanical process to free completely from the smaller kinds of germinal matter.

It is rather discouraging thus to find that, on the philosophy of derivation, as our faith advances the demands upon it increase, until, from belief in the derivation of Horses from Hipparia, we are finally obliged to believe that life with all that it involves is nothing but a peculiar manifestation of dead inorganic forces. In order that, if possible, we may relieve ourselves from this burden, let us now turn to our second inquiry, and consider the

causes which are alleged to produce the transmutation of species.

Leaving out of the account many fanciful and untenable hypotheses, both ancient and modern, we may notice :—(1) The Lamarckian theory of Appetency; (2) The Darwinian theory of Natural Selection; (3) The Owenian doctrine of "Innate tendency to deviate from parental type;" (4) The doctrine of arrested or advanced embryonic development;—with the view of ascertaining how far these several hypotheses may be employed to account for observed facts.

(1.) The Lamarckian theory is essentially that of effort in certain directions giving power in those directions, and consequently altering organs. That it has a real basis in nature no one can doubt who has observed the effect of use and effort in determining the development of organs. That it can produce only varietal forms and not species, and that it is practically very limited in its operation, are facts equally patent. It is a mistake, however, to suppose that Lamarck confined himself to the effect of will in producing change. He considered also the effect of external circumstances, and of habits induced by such circumstances, in which respect his theory differed less than is generally supposed from that of Mr. Darwin. The main difference is, that Lamarck supposed animals to be acted on by an attractive influence from before, Darwin by a propelling influence from behind. In this respect Lamarck's hypothesis is the more philosophical, when regarded as means of real progress; but it is less applicable to the lower animals and to plants than to animals of high grade.

(2.) The most popular theory of derivation in the present day is undoubtedly that of Darwin. This view is, essentially, that all organized beings are engaged in a struggle for existence; that in this struggle certain varieties arise, which, being more suited to the conditions, prosper and multiply more than others; that this amounts to a "Natural Selection" similar in kind to the artificial selection of breeders of stock; that members of the same species, isolated from each other and subjected to struggles of different kinds, will in process of time become specifically distinct. The difficulties of Darwinism are many. The following may be stated as fatal to it in its capacity of a sole mode of accounting for derivation : — (1) Conditions which involve a struggle for existence are found by experience to result in deterioration and final extinction rather than improvement, and are directly op-

posite to those employed by breeders for their purposes. (2) Even if we include, along with the struggle for existence, the action of all conditions, favourable and unfavourable, tending to change, we fail to find any evidence of this other than the formation of varieties and races. True species, no longer capable of interbreeding, have not been observed to be produced. (3) Though it is conceivable that species may have been produced during the lapse of time, yet even this is rendered improbable by the enormously long periods which Mr. Darwin himself admits to be necessary, and which seem to overgo the possibility of the existence of the creatures in question as far back in geological time as the theory demands.

(3.) Owen desires to substitute for the above views "an innate tendency to deviate from the parental type operating through periods of adequate duration." According to this hypothesis "a change takes place first in the structure of the animal, and this when sufficiently advanced may lead to modifications of habits." It is difficult to understand this as anything more than a mere statement of a belief in derivation as a fact. It seems to mean that species change because they tend to change. We may add to this if we please that they change independently of external circumstances, and by virtue of a creative plan embodied in them, or rather in the matter of which they are composed; for Prof. Owen appears to stretch his theory so far as to assert the formation of species spontaneously from inorganic matter, thus giving us the additional thesis that species tend to be before they actually exist. It is also to be observed that the tendency to change, though not caused by external circumstances must act in unison with physical changes, otherwise it would be worse than useless. Taking the case of the Hipparion and horse, Lamarck would inform us that the former endeavoured to accommodate itself to drier and harder ground, and thus changed the character of its feet. Darwin would say that as the ground became harder those individuals which had the most equine feet would succeed best in the struggle for existence. Owen very properly demurs to both views, holding that there were dry and wet places suitable for horses and Hipparia both in the Miocene and Modern periods, and that the increase of dry ground would merely limit the range of Hipparia and not produce horses; but he holds that the Hipparia changed into horses merely because they tended to do so, and that if the change suited the

conditions of the case, that was a correlation arising from the plans of the Creator, and with which their poor brains and greater or less safety and comfort had nothing to do. If we were disposed to accept this hypothesis of Owen, we should not in doing so arrive at any true cause, and we should at the same time find ourselves involved in the old difficulties. That a Hipparion should change into a horse it would be necessary that not only his feet but his teeth and other structures should change in harmony with each other. This must take place either at once or gradually. If at once, then a pair of horses must be born from Hipparia in one herd, and must be isolated from the rest so as to produce a herd of horses. This is hard to believe; and if we resort to gradual change, the required isolation of the breed will be still more difficult to secure. The demands upon our faith are obviously greater here than even in the hypothesis of Darwin,—that is if we can be induced to place any reliance on the argument of the latter as to struggle for existence.

(4.) The last of these hypotheses which I shall notice, and, in my view, the most promising of them all, is one which has recently been ably advocated by Mr. Edward D. Cope in a memoir on the "Origin of Genera," published in the Proceedings of the Academy of Natural Sciences,* and which is based on the well-known analogy between embryonic changes, rank in the Zoological scale and Geological succession. It may be illustrated by the remarkable and somewhat startling fact, that while no authenticated case exists of animals changing from one species to another, they are known to change from one genus or family to another, and this without losing their individuality. Prof. Dumeril, of Paris, and Prof. Marsh, of New Haven, have recently directed attention to the fact that species of *Siredon*, reptiles of the Lakes of the Rocky Mountains and of Mexico, and which, like our North American *Menobranchus*, retain their gills during life, when kept in captivity in a warmer temperature than that which is natural to them, lose their gills, and pass into a form hitherto regarded as of a different genus and family,—the genus *Amblysoma*. In this case we may either suppose that the Amblysoma, under unfavourable circumstances, has its maturity and reproduction prematurely induced before it has lost its

* Philadelphia, 1869.

gills, or that the Siredon has, under certain circumstances, the capacity to have its period of reproduction arrested until it has gone on a stage further in growth and has lost its gills. In any case the same species—nay, the same individual—is capable of existing in a state of maturity as a creature half fish and half reptile in regard to its circulation, or in a more perfect reptilian state in which it breathes solely by lungs. Further, we may suppose conditions of the earth's surface in which there would only be Siredons or only Amblysomas, and a change in these conditions inducing the opposite state. Here we have for the first time actual facts on which to base a theory of development. These facts point to the operation of two causes—first, the possible *Retardation* or *Acceleration* of development, and secondly, the action of outward circumstances on the organism capable of this retardation or acceleration. We here substitute for the tendency to vary of Owen's theory, the ascertained fact of reproductive retardation or acceleration, and for the struggle for existence, the action of changed physical conditions, and for the question as to the change of one species into another, the change of the same species from one genus into another. Further, instead of vague speculations as to possible changes of allied animals, we are led to careful consideration of the embryonic changes of the individual animal, and as to the differences that would obtain were its development accelerated or retarded. We can thus range animals in genetic series within which anatomical characters would show change to be possible. I cannot follow these series out into the elaborate lists tabulated by Mr. Cope, but may proceed to notice the limitations which his views put to the doctrine of derivation. It is obvious that, if this be the real nature of derivation as a possible hypothesis, then derivation must follow the same law with metamorphism and embryonic development. Those animals which undergo a metamorphosis must be those most liable to such changes; for example, a Batrachian would be more likely to be so than a true reptile,—consequently those lower forms of animals in which metamorphosis is most decided would be those in which derivation would be most active, and when they had attained to a condition in which metamorphosis is of less amount, the tendency to change would be diminished. When we compare this with the actual succession of animals in geological time, we can see, as many Palæontologists have remarked, that order of succession in time and order of

embryonic development correspond with one another to a remarkable degree. We see also, however, that in the higher animals changes of species have taken place more rapidly than in those of lower grade, though in the latter metamorphosis is usually more marked—a fact not apparently in accordance with our hypothesis.

According to this view, also, a species once created may have in itself a capacity for passing through several generic forms, constituting a cycle which ever tends to return into itself, or to advance and recede by steps more or less abrupt under the law of retardation and acceleration, combined with the influence of external circumstances. Yet the dimensions of the orbit of each species must be limited, its duration in time must also be limited, and its capacity to pass into a really new species must still be a point subject to doubt, but open to anatomical investigation and inference. As already hinted, it is a most important point of this theory, that when we have ascertained the series of embryonic changes of any animal, we have thereby ascertained its possibilities in regard to accelerated development. Its possibilities in regard to retarded development may be inferred by similar studies of animals higher in the scale. Now, if we knew the embryonic history of every animal recent and fossil, in its anatomical details, we should be able to construct out of this a table of possible affiliation of animals, and should be able to trace our existing species through the same genera, families, orders and classes in which they might have existed in geological time, and to predict what they might become in time still to come. This hypothetical scheme of creation would approach to the actual one in as far as we were able to correlate it with the physical changes which have occurred or will occur on our planet. Let us take as a crucial test the case of man himself. The actual anatomical and physiological differences which obtain between those races in which maturity is latest, and those in which it is earliest, and a comparison of these with embryonic characters, would give us the modern data. The comparison of these with the most ancient human remains might enable us to infer whether retardation or acceleration has been the tendency in historic or geological time. From this we might infer what might be the condition of man under a still more accelerated development than any now known, or under that antediluvian condition in which immaturity is said to have been

protracted over half a century, or that still future time predicted in Holy Writ when the days of a man shall be as those of a tree. Having worked out these problems, we would be in a position to inquire as to the possible transition of Homo from or towards any other generic form. I would by no means put forward this theory of embryonic development as including the whole law of introduction of species or genera* any more than the others reviewed, but I must say that to my mind it appears to hold forth the most promising line of investigation, with the hope of arriving ultimately at some true expression of the law of creation with reference to organized beings.

What that law will ultimately prove to be, and to what extent it may include processes of derivation, it is impossible now to say. At present we must recognize in the prevailing theories on the subject merely the natural tendency of the human mind to grasp the whole mass of the unknown under some grand general hypothesis, which, though perhaps little else than a figure of speech, satisfies for the moment. We are dealing with the origin of species precisely as the Alchemists did with Chemistry, and as the Diluvialists and Neptunists did with Geology; but the hypotheses of to-day may be the parents of investigations which will become real science to-morrow. In the meantime it is safe to affirm that whatever amount of truth there may be in the several hypotheses which have engaged our attention, there is a creative force above and beyond them, and to the threshold of which we shall inevitably be brought after all their capabilities have been exhausted by rigid investigation of facts. It is also consolatory to know that species, in so far as the Modern period, or any one past Geological period may be concerned, are so fixed that for all practical purposes they may be regarded as unchanging. They are to us what the planets in their orbits are to the Astronomer, and speculations as to origin of species are merely our nebular hypotheses as to the possible origin of worlds and systems.

One word in conclusion with reference to our own work here as a Society, and as individual collectors of facts. We may not be in a position to take any leading place in the agitation of the

---

* It is but fair to say that Mr. Cope himself admits the action of natural selection as one cause of change.

questions to which I have referred; but we are well situated for the useful task of accumulating the necessary data for their settlement. The broad area of the American continent, the wide space occupied by its geological formations, the completeness of the series of its palæozoic rocks, the unbroken connection of its post-pliocene and modern fauna and flora, the meeting on this continent within recent times of multitudes of indigenous and exotic species of plants and animals, the existence up to our own time of feral and aboriginal conditions which are pre-historic in the Eastern continent,—these are all points of vantage on which we can seize in dealing with these questions; and if we properly inform ourselves as to what is being done elsewhere, and diligently improve our own opportunities, I see nothing to prevent us from taking the lead of those who in the Old World are pursuing such inquiries in a comparatively narrow field, and under conditions in many respects less favourable. I must insist, however, that this is not to be done by vieing with them in crude speculations and hypotheses, or in building up specious fabrics of conjecture to dazzle the popular eye, but in patient, honest, and careful accumulation of facts.

We should also bear in mind that in the greater centres of literary and scientific life, there is a strong temptation, especially on the part of ambitious men who have their own fortunes to build up, to deal in that sensation science with which the popular literature of the day is deluged. In our own comparatively obscure field there is little inducement to this or opportunity for its display, and this is so far in favor of a healthy scientific tone, which we should endeavour to preserve and cultivate. Our danger arises from being too ready to follow the extreme views put forth elsewhere, and from impatience with the slow returns for honest and careful work.