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1932

## 755TH ORDINARY GENERAL MEETING,

HELD IN COMMITTEE ROOM B, THE CENTRAL HALL, WESTMINSTER, S.W.1, ON MONDAY, MARCH 21ST, 1932

AT 4.30 P.M.

G. A. LEVETT-YEATS, ESQ., C.I.E., I.S.O., F.Z.S., IN THE CHAIR.

The Minutes of the previous Meeting were read, confirmed and signed, and the HON. SECRETARY announced the election of the following as Associates :—Miss E. M. Herriott and Mr. Robert J. Cobb.

The CHAIRMAN then called on Mr. Douglas Dewar, F.Z.S., to read his paper on "The Limitations of Organic Evolution" which had been chosen as the Dr. A. T. Schofield Memorial paper, 1932.

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# *THE LIMITATIONS OF ORGANIC EVOLUTION.*

By DOUGLAS DEWAR, ESQ., F.Z.S., Barrister-at-Law.

(*Being the Dr. Alfred T. Schofield Memorial Paper.*)

Sir Michael Foster began his course of lectures on Physiology at Cambridge for the session 1892–93 with the following words : "I find every year that I have to cease repeating statements which I made in my previous courses of lectures, because new discoveries have shown these statements to be incorrect." Such words would be a fitting prelude to every course of lectures on Natural Science, especially on Biology and Geology, because in these theory has outrun fact, owing to the modern tendency to depart from Baconian principles and to indulge in speculation.

The history of natural science is a history of discarded hypotheses. Almost every hypothesis hitherto put forward has been either abandoned or greatly modified. The theory of evolution, as generally held to-day is very different from the hypothesis enunciated by Darwin. It seems to me that ere long it will have to be still more drastically modified.

AS the result of forty years' study I have come to the conclusion that the amount of transformation in the organic world that has been effected by the process of evolution is limited. As the term " evolution " is very elastic, let me here say that I employ it in what I believe is the most generally accepted sense, to denote the gradual, as opposed to the sudden, origin of new types of organisms ; thus the statement that the whales have evolved from a terrestrial ancestor means that the supposed ancestor, in the course of successive generations, gradually lost its terrestrial form and acquired an aquatic form. The proposition which I submit for your consideration is the changes that have been effected gradually in animals are strictly limited, and do not transgress the limits of the natural family. Go back as many generations as you will, you never see evolution taking an animal from one family into another. Several living biologists have openly enunciated this proposition. E. G. Dehaut, who has made a special study of the living and extinct fauna of the islands in the Mediterranean, would perhaps place an even greater restriction on evolution, for he writes (*Contribution à l'Etude de la Vie Vertébrée dans la Region Mediterranienne Occidentale*, p. 19): " The species appears to me to be *par excellence* the unit of the organic world ; from this I conclude that its production indicates a particular intervention of the Creative Power. This is why I do not consider it right to describe as distinct species animal forms that pass from one to the other by insensible shades, because the action of mere secondary causes seems sufficient to account for their differentiation."

From this it is apparent that Dehaut puts a wide and vague interpretation on the term " species," inasmuch as he would describe as a species a group of animals, no matter how large it be, of which the members are separated from one another by insensible gradations. Indeed there may not be much difference between Dehaut's view and that which I am advocating. So far as I am aware, the German Palæontologist, E. Dacqué, was the first definitely to assert that evolutionary changes in animals are confined to the ambit of the natural family. As long ago as 1911 Dacqué asserted (*Palæontologie, Systematik- und Descendenzlehre*, p. 179): " New types, because always specialized, must have originated suddenly by leaps, as the result of an important transformation in embryonic life, which is certainly no more astonishing than the metamorphosis of an insect."

A few years later G. McCready Price, Professor of Geology of Union College, Nebraska, who has read papers before this Society, wrote (*The Phantom of Organic Evolution*, 4th edn. (1924), p. 206): "I do not believe that the various families included in any given order have originated from any common ancestor . . . I am willing to grant that all of the cats over the world may have had a common origin; that all of the bears may have had a common origin; or that all the genera included under the *Canidæ* may have had a common origin. Yet I utterly deny that there is any scientific evidence worthy of the name to intimate that the cats and the bears and the dogs have all sprung from a common generalized type in the long ago."

Mr. Dudley J. Whitney has published views similar to those of Price. It will be observed that Price goes farther than Dacqué, in that he denies that the cats, dogs and bears have descended from a common ancestor, while Dacqué says that such a descent may have occurred, but, if it did, the transformation from non-dog to dog must have been effected by an important change in embryonic life. Dacqué's view seems the safer in the present state of our knowledge, because (although Price will not have it so) the rocks, as interpreted by geologists, indicate that some families appeared on the earth at a later period than others, and if this be the case, the phenomena of embryonic development indicate that new types may have so originated. There is, of course, no proof that creation has ever been effected in this manner; but such a method does not seem to be an impossibility.

The zoologist who has gone the most carefully into the limitations of organic evolution is L. Vialleton, who for nearly half a century before his death in 1929 was Professor of Comparative Anatomy at the University of Montpellier. Vialleton specialized in the anatomy of tetrapod vertebrates and is the author of several volumes and papers on that subject. His greatest work was published in 1924, and bears the rather cumbrous title "Morphologie Générale. Membres et Ceintures des Vertébrées tetrapodes. Critique morphologique du Transformism."

Vialleton's prolonged study of comparative anatomy led him to assert: the theory of organic evolution postulates transformations that are physically impossible. Thus, he writes of the Cetacea (*loc. cit.* p. 394): "In a development such as we have just discussed there is no place for a pelvis, since most of its functions are performed by other organs, and the reduction which it has effectively suffered is easily understood. No more

is there a place for conditions intermediate between those of ordinary and aquatic mammals, because one cannot imagine individuals of which the hind limbs, still well (*assez*) developed, and the tail, already stronger than usual, function simultaneously. It is therefore an illusion to look for intermediaries in which will be found, at one and the same time, an ordinary mammalian pelvis and a tail tending towards the pisciform type." In other words, so long as an animal possesses an effective pelvis, its tail cannot act as a propeller like that of a whale, because the pelvis will not admit of the proper attachment to the backbone of the motor muscles of the tail, and a land mammal with an improperly developed pelvis is incapable of locomotion on land, because the hind limbs lack points on which to articulate; therefore the gradual transformation of land animal into a cetacean is impossible.

Vialleton's anatomical researches led him to believe that the various groups into which the animal kingdom is divided are not all based on the same criteria. Phyla, classes and orders are founded on the modalities of the organization of their component parts, while the lesser groups are based particularly on form. In consequence, Vialleton described the former as Types of Organization (*Types d'organisation*) and the latter as Formal Types or Types of Form (*Types formels*). He asserts that there is a fundamental difference between these two classes. Types of Organization consist of types (*loc. cit.* p. 675) "that differ from one another in their very nature, because each of them results from a peculiar development of the embryonic rudiments (*ébauches*) of the phylum; in consequence it is not merely the perfection or reduction of a neighbouring type, but something different." On the contrary the formal types are "all of the same nature, of which the different terms are distinguished only by more or less accessory details, or by their form."

Of the phylum, the most comprehensive of the types of organization, Vialleton writes: "The essential characters of this are imparted by the mode of the growth of the embryonic layers and by the architecture resulting therefrom . . . this architecture constitutes the only general character of the phylum—a very precise character despite its generalness, because of the difference between it and the architecture of other phyla. In the phylum form is represented merely by the superposition of the parts, and can be exhibited only by transverse or longitudinal sections, which permit the perception of this superposition, but nothing

more. Thus, it is impossible to imagine the contour of a mollusc, an echinoderm, an arthropod or a vertebrate ; the attempt to do so inevitably leads to representations, not of the general type, but of one of its expressions . . . The class, like the phylum, cannot be characterized by its form, because its features are drawn entirely from the central parts and derive nothing from the peripheral parts, such as the embryonic rudiments of the limbs, which will give rise later, by their specialization and the correlations this entails, to the secondary types of each class—the orders. These embryonic rudiments are at first in an undifferentiated condition, capable of taking various dispositions, so as to produce . . . the wing of the bat, the paddle of the dolphin, the leg of the lion or the horse, the arm and hand of man. . . . In the orders the organization of the class becomes determined as regards the relations of the organs, especially those of locomotion. Thus, the fore-limb of a carnivore, formed for locomotion and seizing prey, will become a paw with five toes ending in claws, the fore-limb of a cetacean will become always a paddle, that of the chiroptera a wing, and so on. But several forms of wings and paddles are possible, that is why the order, equally with the preceding divisions, is not yet characterized by a determined and constant form. In order to define an order it is necessary to have recourse to its organization, that is to say the general characteristics of its chief apparatus, to the dentition or the limbs, closely correlated to the ordinal type. . . . Below the order organization no longer operates in establishing systematic categories, because all these groups have an identical organization—that of the order. On the other hand, they exhibit many well-marked differences, of which the principal is form. By form is meant the exact outline of an organism, stripped of all extravagant tegumentary excrescences. . . . Formal types are represented by general forms known as sub-orders or super-families. These forms are in fact modality types which a given organization can assume to adapt itself to various functions, or the different places it can occupy in nature. As a result of these adaptations the formal types are in turn divided into the secondary categories below sub-orders and super-families. . . . The subdivisions of the formal types do not present among themselves the opposition exhibited by the types of organization ; being composed of organisms of the same nature, they represent quantitative differences, or rather the details of the outer parts and accessories of which nature produces

an exuberance and a prodigality, which, as Cuvier remarked, are beyond our comprehension."

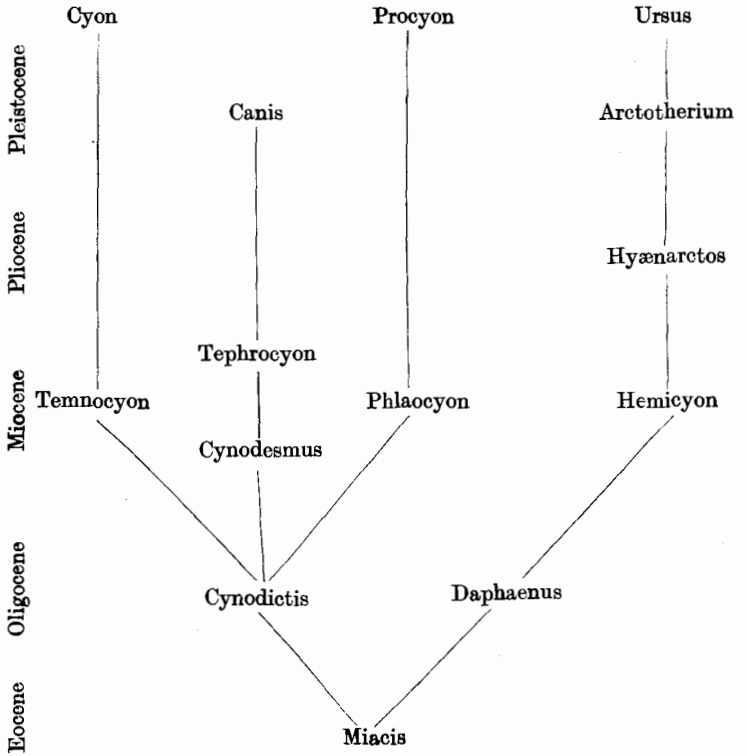
Vialleton's theory may be thus enunciated (*loc. cit.* p. 679); a new Type of Organization can originate only by a special development effected in the egg in the earliest stages of ontogeny, which absolutely excludes the process of phylogenic development required by the doctrine of evolution. On the other hand, many of the Types of Form may originate in the latter manner; but certain of these, very sharply defined and very isolated, may have originated independently of their nearest neighbours by a change in an early stage of embryonic development, as in the case of the orders. Others, not so clear cut, and the secondary subdivisions owe their origin to less profound transformations depending on conditions and functions, as evolutionists incorrectly imagine in the case of the bigger groups.

Thus Vialleton, as the result of prolonged study of comparative anatomy and embryology, became convinced that none of the orders or greater groups of animals can have originated gradually as the result of the accumulation of variations or mutations, but he considered that some of the sub-orders and super-families, if not very sharply differentiated from some other group, may have had such an origin.

Vialleton may be right, but I am inclined to think that he credits evolution with having effected transformations beyond its powers. As regards mammals, at any rate, the fossils known to us do not seem to favour the theory that most of the sub-orders and super-families have originated as the result of the gradual modification of earlier types. I contend that it is not possible with these fossils to construct a single phylogenetic series linking a member of any mammalian family with a member of any other family. So far every attempt to construct such a series for any genus has failed. No single pedigree has been constructed which is not open to severe criticism. In the present paper it is not practicable to criticize all such pedigrees. It must suffice to deal with that which is put forward with the greatest assurance, purporting to trace the *Canidæ*, *Ursidæ* and *Procyonidæ* back to a common ancestor. The greatly paraded pedigree of *Equus* is not relevant, because, as now set forth, it does not purport to show from what earlier family the *Equidæ* have sprung; it merely deals with the evolution or differentiation of the horses since the appearance of *Eohippus*, the earliest known member of the family. So far as I am aware, every zoologist to-day rejects

the earlier pedigree that traced the descent of *Equus* from *Phenacodus*.

The latest bear-dog-raccoon pedigree is that published by Professor W. J. Matthew in the *Journal of Mammalogy*, 1930, vol. 2, p. 117.




Before pointing out the most palpable errors in this pedigree it is necessary, in justice to Matthew, to say that he himself is not sure of its correctness. He writes : " It is probable that some of these (the genera that compose the pedigree) are derived from imperfectly known allies of *Cynodictis* rather than from this genus itself." Of the bear line of descent he remarks : " Until complete skeletons are known and studied it is uncertain how close it (the pedigree) is to the direct line of descent." On p. 129 he writes : " How near *Phlaocyon* really stands to the ancestry of *Procyon* will also remain uncertain until an inter-

mediate series is discovered. If it was not the ancestor it is just like what that ancestor must have been as adjudged from a critical study in the light of all the known evolutionary series among the Carnivora." The above pedigree is not accepted even by many evolutionists. While Matthew derives *Temnocyon* from *Cynodictis*, Osborn (*The Age of Mammals* (1910), p. 230) asserts that *Temnocyon* is a descendant of *Daphaenus*.

*Daphaenus*, which Matthew makes out to be an ancestor of *Ursus*, belongs to the Canid sub-family *Amphicyoninæ;* but, on p. 67 of vol. iii (1925) of von Zittel's *Text-book of Palæontology* it is asserted that the bears are an offshoot of another sub-family —the *Cynodontinæ*.

Further, Schlosser insists (*Palæontographica*, vol. xlvi (1899), p. 142) that *Hyænarctos*, while in a measure parallel to the bears in evolution, is not in the direct line of *Ursus*.

*Arctotherium*, which Matthew shows as the direct ancestor of *Ursus*, is not known until the Pleistocene, whereas *Ursus* occurs in the earliest Pliocene! As regards *Procyon*, the raccoon, Teilhard believes this animal to be derived from *Pachycynodon* and not from *Cynodictis*.

Disagreements such as the above occur in the case of all other pedigrees. As Dacqué sarcastically remarks (*Palæontologie, Systematik- und Descendenzlehre* (1911)): "Two pedigree-makers never construct the same tree, and usually just where theory requires a liaison there the pedigree is interrupted, obscure, or has to be made up."

There is, however, one point on which all the pedigree-makers agree, viz., that the modern Carnivores are derived from the Creodont family known as the Miacidæ. This unanimity is due to the fact that the Creodonts are the only known earlier animals that show any resemblance to the modern Carnivora, and the Miacidæ is the only Creodont family having in common with the Carnivora the fourth upper premolar and the first lower molar modified as carnassial teeth. The argument is: premise, the Carnivora evolved from some earlier group. Of the earlier groups the Miacidæ bear the greatest resemblance to the Carnivora, *ergo* the Carnivora evolved from the Miacidæ. In view of the fact that those who have adopted this argument have invariably blundered in the past, it is somewhat surprising that it is still resorted to.

One of the earliest biologists to fall into the error of believing that resemblance denotes blood relationship was the talented

author of *Vestiges of the Natural History of Creation*, who dogmatically asserted that the seals (Phocidæ) gave rise to the bears and these, in their turn, gave birth to the Canidæ. He, like his successors, based his genealogies largely on the form of teeth, which are particularly unsafe criteria, because their form depends largely on the food eaten by their possessors.

Such a procedure may have been excusable in Darwin's time. Since then our knowledge of histology has increased greatly. In the present state of knowledge to frame pedigrees based on the form of teeth, without considering the minute structure of these, is, to say the best of it, indiscreet. Sir John Tomes and his son, C. S. Tomes, many years ago, studied the minute structure of the teeth of a large number of animals. In 1906 the latter, who was a Vice-President of the Zoological Society of London, read a paper before that Society entitled "On the Minute Structure of the Teeth of Creodonts, with especial reference to their suggested resemblance to Marsupials." This paper is printed in vol. i of the *Proc. Zool. Soc.* for 1906. In this paper Tomes wrote (p. 45): "It might have been expected that there would be but little variety of structure in the teeth of animals belonging to the same great groups, for it is not easy to see how this should be affected by the ordinary processes of selection. It might have been thought that so long as a tooth was strong enough, sharp enough, and well adapted in external form to its work, its structure would matter little and would remain constant. But it was shown by my father, the late Sir John Tomes, that by a mere examination of sections of the enamel it was possible in the case of rodents, not merely to pronounce that the enamel was that of a rodent, but, in a large number of instances, to refer it correctly to a particular family of rodents, or to a group of rodents. . . . Similarly, my father showed that the enamel of Marsupials presented characters very unusual in placental mammals, and therefore almost characteristic of Marsupials, whilst the Carnivora also presented well-marked enamel characteristics."

In view of the above C. S. Tomes thought it "well worth while" to examine the enamel of some Creodont teeth. To him the result of this examination was very disappointing, as this enamel was found to be not intermediate between that of Marsupials and that of modern Carnivores. He writes: "so far as the structure of their enamel may be taken as evidence, with one exception, no Creodont presents any greater resemblance

to Marsupials than do the recent Carnivores." This exception is afforded by the Miacidæ. Tomes was surprised to find that the enamel of the only Miacid he examined—*Didymictis*—is actually simpler than that of other Creodonts and of most recent Carnivora. This means that the family from which evolutionists are agreed that the Carnivores have originated is the one in which the enamel is the least like that of the Carnivora. Nor is this all. Tomes found that the enamel of *Cynodictis* is very like that of *Didymictis*. He was thus forced to conclude that " as *Cynodictis*, at all events, appears to be nearer to the true Carnivora than are the Creodonts, the simplicity of its enamel, as compared with theirs, may point to its lying not quite in the same line of descent."

The above discovery does not accord with the theory that *Cynodictis* is the common ancestor of the dogs and the raccoons.

Did Matthew know of Tomes's discoveries when he drew up the above pedigree ? It is quite likely he did not, because, to quote Vialleton, " For the past fifty years the text-books are a simple illustration of evolution, bringing to light only that which is favourable to it, passing over in silence all that is outside it or contrary to it."

If Tomes's discoveries had been favourable to the doctrine of evolution, if the enamel of the Miacidæ were intermediate between that of the other Creodonts and that of the Carnivora, this fact would have been hailed with delight, recorded in the text-books and have found its way into the scores of popular works which sell by the thousand, as has happened in the case of Nuttall's blood-serum experiments, which at one time were deemed to be favourable to the evolution theory.

A posthumous edition of C. S. Tomes's *Manual of Dental Anatomy* has appeared since the above observations were recorded. The editors—Dr. W. H. M. Tims and Mr. A. Hopewell-Smith—have inserted in this nothing about the peculiar enamel of *Cynodictis*, nor have they included Tomes's paper in the list of authorities at the end of the chapter dealing with dental tissues. The paper in question is mentioned in chapter xvi, as is the fact that in respect of enamel the Creodonts stand no nearer to the Marsupials than do the true Carnivores, but nothing is quoted regarding the enamel of *Cynodictis* and *Didymictis*.

As C. S. Tomes died shortly after he had made the above discoveries he had little or no opportunity of further investigating the structure of various enamels. No one else seems to have followed up this line of investigation. It may, I think, be safely

asserted that had Tomes's discoveries been favourable to the doctrine of evolution, scores of histologists would have devoted much time to the investigation of the structure of enamels in order to furnish proofs of evolution.

In view of facts such as these and of the eclectic nature of the lectures attended and the text-books read, is it surprising that all the younger zoologists are evolutionists ? As no one has succeeded in tracing, by a phlyogenetic series of fossils, the descent of one family from another, it is scarcely necessary to mention that this is the case with the larger groups. As Dacqué puts it : " Never yet has it been possible methodically and faultlessly to trace to a common origin two types or two larger groups."

As to genera, the fossils have afforded remarkably few cases of one genus becoming gradually transformed into another genus.

" Only rarely," writes Vialleton (*loc. cit.* p. 671) " has it been found possible to trace a genus, step by step, and without artifice into an earlier genus ; moreover, when this can be done, it is never a case of two creatures essentially different in their organization, but of neighbouring forms of which the organization continues in the same line."

Most evolutionists recognize that such facts as these must be accounted for unless the evolution theory in its ordinary form is to be abandoned. Some allege the imperfection of the geological record and of our knowledge of it. As regards mammals, at any rate, this allegation is incorrect. I have been taken to task by a German zoologist for having applied mathematics to Palæontology in my volume *Difficulties of the Evolution Theory*. He asserts that such calculations are based on purely subjective suppositions and that, of all sciences, Palæontology is the last in which mathematical calculations should find place. The first assertion may be correct, but the second certainly is not. Moreover, it is not necessary to make any suppositions ; inferences must of course be drawn.

How far the views of my critic are sound may be judged from the table given below compiled by me. I believe that the figures given of the fossils of non-volant land mammals found in various periods of the Tertiary of Europe and North America are fairly accurate. In case of genera now living, in those continents I have adopted the nomenclature of Lydekker, as being more suitable for comparison with fossil genera than that which is in vogue to-day.

I submit that the figures in the table show that, as regards the mammals in question, the geological record is not very incomplete, indeed it reveals to us the majority of these.

*The number of genera of non-volant land mammals known to have lived at various stages of the Tertiary and in the Quaternary and now living in Europe and North America.*

| Stage | Europe | North America |
|---|---|---|
| Basal Eocene ... ... ... | 14 | 40 |
| Lower Eocene ... ... ... | 24 | 52 |
| Middle Eocene ... ... ... | 38 | 69 |
| Upper Eocene ... ... ... | 68 | 37 |
| Lower Oligocene ... ... | 80 | 58 |
| Middle Oligocene ... ... | 41 | 44 |
| Upper Oligocene ... ... | 43 | 57 |
| Lower Miocene ... ... ... | 52 | 51 |
| Middle Miocene ... ... ... | 59 | 35 |
| Upper Miocene ... ... ... | 81 | 52 |
| Lower Pliocene ... ... ... | 87* | 42 |
| Middle Pliocene... ... ... | 47 | 18 |
| Upper Pliocene ... ... ... | 45 | 28 |
| Pleistocene ... ... ... | 66 | 84 |
| Now Living ... ... ... | 48 | 72 |

* Includes fossils from the Maragha beds of Persia.

Thirty-seven families of non-volant land mammals are known to have lived in the Pleistocene of Europe and North America, none of which occurs in the Basal Eocene. In addition, twenty extinct families are known to have inhabited those continents. Allowing for the fact that some of the above families may have originated outside Europe and North America, in some locality not geologically explored, and migrated from there to Europe or North America, the inability to trace the descent of any of the above fifty-seven families does not accord well with the evolution theory. Some Palæontologists appreciate this. In consequence the theory of centres of evolution has been formulated. To my mind this hypothesis is eminently unscientific, because it assumes that evolution has taken place only in certain localities, not one of which has yet been palæontologically explored. This assumption involves the belief, either that the forces which cause

evolution are confined to certain areas, or that their activity has been inhibited in all localities in which numerous mammalian fossils have been found.

The great majority of living biologists infer, from the possibility of change within the type, that of change from one type to another. A few of us, more circumspect or cautious, distinguish carefully between these two things. Time will show whether they or we are right. The facts at present known seem to be in our favour.

## Discussion.

The Chairman (Mr. G. A. Levett-Yeats, C.I.E., I.S.O.) said: It has given me pleasure to introduce Mr. Douglas Dewar, who has had a distinguished career in India as a Member of the Indian Civil Service, and nevertheless found time to keep up his interest in those scientific studies that he pursued with credit at the University of Cambridge. He made time in the midst of arduous and responsible duties to keep fully apace with the trend of modern thought on the evolution theory—a subject which he has studied critically for the last forty years.

It was my good fortune many years ago to be employed in the same station as Mr. Dewar in India. A community of tastes led to the formation of a lasting friendship. We were both interested in ornithology, and spent many pleasant mornings on the sandbanks of the Ganges at Ghazipur, investigating the habits of the terns and other birds. I then realized how close and keen an observer of nature Mr. Dewar was. On Indian birds he is an authority, and has written and published numerous works on this subject.

The subject of to-day's lecture has occupied Mr. Dewar for many years, and to it he has brought wide reading, close observation, and the powers of a well-trained, keen, and analytical mind. He is also an authority on this subject, regarding which he has lately published a powerful and illuminating book entitled *Difficulties of the Evolution Theory*, a book which may be best described as a searchlight into the darkness of confused thinking.

From the evidence produced by Mr. Dewar, it is clear that the fossils themselves, so far as we know them, appear to call for a considerable revision and modification of the currently accepted theory of evolution. The story unfolded by the fossils may support

the idea of variation or differentiation within certain limits, but it does not afford evidence supporting a theory of evolution on the grand scale.

This is not because the fossil record is very poor. On the contrary, the fossil record is by no means poor. Mr. Dewar has mentioned that in another paper, in the preparation of which I collaborated with him, it is shown that 45·63 per cent. of the living genera of mammals alone are known as fossils. Such facts are eloquent.

I ask you to join me in offering a hearty vote of thanks to Mr. Dewar for the preparation of the paper he has just read—a paper that will rank as a most valuable contribution to scientific thought and scientific method. I hope that Mr. Dewar will continue his labours in the field he has chosen, and will add still more to the startling array of facts that render the modern theory of Evolution incompatible with the truth.

I have one more pleasing duty to perform, and that is to hand to Mr. Dewar the honorarium of £10 which is awarded to him as the author of the Alfred T. Schofield Memorial paper.

At the call of the Chairman, a cordial vote of thanks was awarded the lecturer.

Rev. C. Leopold Clarke said: I would draw attention to the new evidence provided in the paper of the continuing drift of Biological research away from the theory of Organic Evolution, as properly so called. I wonder, indeed, if the name can at all be given to those processes of transformation or variation described by the lecturer, which by common consent "confine themselves within the ambit of the natural family." Organic Evolution seems to denote so much more than that. The bone of contention between Evolutionists and non-evolutionists is precisely whether or not Biology has the material to show the rise of new types from existing types—or the "rise of any new type apart from a specific act of creation."

The lecturer has offered important authorities, who in veiled language are admitting what amounts to this "act of creation"—some even use the very term. Dehaut, for instance, says that "the species is the true unit of the organic world, and that it requires a particular intervention of creative power." Vialleton predicates "a special development effected in the egg in the earliest stages of

ontogeny " or individual history ; which seems only a very polite circumlocution for creation. Dacqué says that " an important transformation in embryonic life is requisite for new types, because they are always specialised." Then there is the lecturer's own conclusion, that " the fossils do not exist from which a phylogenetic series can be constructed linking any mammal with the other family."

All this shows the flow of the tide away from the idea of any automatic species production, if I may so phrase it. I suggest that the more the positive evidence of Biology disproves the theory of Organic Evolution, the more insistent becomes the necessity to overhaul the supposed evidence of geology in its favour. This is largely based upon evolutionary prepossession, and its imagined proofs are almost entirely presumptive. The reason recently assigned by Prof. D. M. S. Watson for the " universal acceptance of evolution " was that " the only alternative ' special creation ' was clearly incredible " (British Association Meetings, Capetown, 1929). Huxley said that half a century ago, and I think I am right in saying that he had in mind creation of a piecemeal kind, extending over incredible ages, which the present system of geological interpretation presumes. But that is not the view of creation revealed in the Bible—and I know of no other source of revelation of Creation. Prof. G. McCready Price, in his *New Geology*, offers a mass of evidence that the fossils simply show an older state of our world which perished, and not the theory of an " ordered life-succession," corresponding rigidly with a universal succession of rocks and strata, on which estimates of unlimited geological time are based and the evolution of endless species. Geological facts ought first to be explained, especially the fact that the great geological changes and the upheaval of the mountain ranges must have taken place since the thousands of living species of plants and animals, including man, came upon the earth. So long as Biology could sustain even an equivocal negative in the matter, the geological evidence has been neglected. The Biblical view of a vast number of types created within near distance of each other, perhaps, is being substantiated by the conclusions of Biologists, as well as by the altered view of Geology, and is establishing the credibility of Creation, and removing Organic Evolution from the scheme of things.

Rev. H. Temple Wills, M.A., B.Sc., said he was reminded of an experience he had in the late 'eighties when he heard Huxley lecture on the skull of a recently discovered mammal at the Geological Society. The Professor, after a masterly description of the remains which he held in his hands, showed that they proved to be more highly developed than those of other animals of the same class which had been previously found in newer strata. Realizing that this might be taken to be an argument against his pet theory, he said that Evolution must be true, and therefore we must find a way out of a difficulty. This he proceeded to do by drawing a large Y on the blackboard. At the foot of the Y he said there must be a common ancestor as yet undiscovered, and then he put the form he had been describing on the one arm and the other forms on the other arm, saying that it was clear that there had been separate development. This was so palpable a shift that many scientists present looked at one another and smiled, and Prof. T. McKenny Hughes of Cambridge said after the meeting was over, "Oh, that was Huxley all over."

Mr. G. F. Claringbull said: Mr. Dewar has made a really valuable contribution to the Transactions of the Institute. He has shown that the pendulum is swinging back from Darwin and selectionism toward a polyphyletic origin of similar organic forms. One is inclined to agree with him that Dacqué's view is perhaps safer at the moment than that of Price. His conclusions are wonderfully supported by Berg (*Nomogenesis*, 1926)—*e.g.* (p. 341): "To create a new name is not a very difficult matter. But a fact remains a fact. Similar forms have been produced from various stems, and that is what we mean when we speak of a polyphyletic origin. Every new class, sub-class, order, etc., established on the assumption of it being derived from a separate root, is yet another proof of the inadequacy of selectionist views and a confirmation of the truth of nomogenesis.

"If we turn to the history of the classification of plants and animals, we shall see that the number of phylæ, classes, orders, etc., continually increases, and this increase is in an overwhelming majority of cases due to authors realizing that they are unable to derive one group from another, *i.e.* it testifies in favour of polyphyletism."

Again (p. 343): "A strict adherence to the monophyletic principle is generally bound to lead to absurdity. For in that case we should have to admit that all mammals (or even all vertebrates) or all angiosperms are derived from one individual. For, if they owe their origin to many individuals, their development would be governed by analogy and convergence, not by homology. *But it is quite inconceivable that all vertebrates, for instance, should be derived from a single pair.*"

Further on, p. 347, showing that the plea of incompleteness of the fossil record is only the bluff of the evolutionist:—

"It is truly remarkable that palæontology in no way displays transitional forms between phyla and classes, and, possibly, not even between orders. Thus, we are ignorant of transitional forms not only between vertebrates and invertebrates, fishes and tetrapods, but even between cartilaginous (chondrichthyes such as sharks, etc.) and higher fishes (osteichthyes); in spite of a wonderful affinity between reptiles and birds, no transitional forms between them are known hitherto. Formerly, this circumstance was accounted for by the imperfection of the geological record; but it is none the less surprising that the deeper our knowledge penetrates into the domain of fossils, the further back recede genetic interrelations, which, as it were, ever elude our grasp."

Incidentally, it is interesting to note that Prof. D'Arcy Thompson, at the last meeting of the British Association, said, that while not denying the evidence for evolution, he thought that any attempt to trace the passage from invertebrate to vertebrate was doomed to failure. Lastly, Berg, who is not a theologian, but a scientist of the first order, says (p. 358): "To support the view that animals descended from four to five progenitors is now *impossible*, the number of the primal ancestors must be computed in thousands or tens of thousands." Remarking that Belogolovy speaks even of "millions of initial points."

Mr. GEORGE BREWER said: The doctrine of Evolution, as taught by many scientists, is at best a purely speculative theory, which seeks to account for radical differences of structure and modes of life by spreading them over immense periods of time, thus postulating a gradual development from the lowest forms of life, for the origin of which no account is given.

As Dr. Etheridge, the fossilologist and Curator of the Natural History Museum, is reported to have said: "In all this great museum there is not a particle of evidence of the transmutation of species: nine-tenths of the talk of Evolutionists is sheer nonsense, not founded on observation, and wholly unsupported by facts. The museum is full of proofs of the utter falsity of their views."

Prof. L. S. Beale, Professor and Fellow of King's College, in his book *Vitality*, says: "We have had during many centuries modifications in pigeons, dogs and men, and the powers of variation are by no means exhausted, although the widest departure from the original type does not pass beyond pigeon, dog or man."

Forms of vegetable or animal life which man is able to vary by human selection and environment revert repeatedly to type as soon as man's directive skill is withdrawn, proving that there are certain types and species which can sometimes be widely extended within the strict limits of the species, but that no further change can take place by either natural or artificial selection. The fixed law of sterility in both vegetable and animal realms, each species yielding seed after his kind, is fatal to the Evolution theory, while it supports the clear record given in the first chapter of Genesis.

By careful selection and environment man can develop the wild rose into many beautiful varieties; or the rock dove into many varieties of pigeons; but it is significant that these varieties do not continue to increase, or even persist, but will revert to their original state when left to themselves.

Rev. Dr. H. C. MORTON said: It is a long time since any contributor to our Proceedings has put us more deeply into his debt. The *independent* scientific voice is what Britain deeply needs. We have one here this afternoon, and I breathe the fervent prayer, May Mr. Dewar's tribe increase!

I am not a zoologist or a research worker, but just a member of the jury of the intelligent public whose verdict of "proven" or "not proven" will weigh heavily in the Evolution controversy. Mr. Dewar has given advocates of evolution a great deal to answer in this paper, and his arguments appear to me to have an unanswerable cogency. The mathematical argument impresses me strongly. The plea that the imperfection of the geological record

and of our knowledge of it accounts for our failure to trace to a common origin two types or larger groups, can hardly survive the table given by our lecturer. In Europe and North America there are 122 genera of non-volant land mammals, known and now living. In each of the other thirteen stages, leading back to the Basal Eocene, Mr. Dewar shows that there were on an average one hundred genera of such non-volant land mammals. One hundred and twenty-two known living to-day, and in each preceding stage upon the average one hundred known! In other words, our knowledge of the geological record in this respect is *not* slight, but very considerable indeed; and in spite of this wide knowledge the evolutionists fail to trace the descent of any one of the 37 non-volant land mammals which existed in the Pleistocene, but did not exist in the Basal Eocene! In view of their wide knowledge of the geologic records, that failure needs a great deal of explanation, and will go far to convince the jury of their failure.

May I refer to two things which arouse deep indignation in my mind whenever I think of them? One is the continued repetition by responsible men of discredited evolutionary "proofs." To-day it is Recapitulation and the embryological argument; to-morrow it is the Blood-Test, which, if it proves anything at all proves vastly more than any evolutionist can allow; next week some one will again be making dogmatic statements about Pithecanthropus or Hesperopithecus or the Taungs skull. It is the ceaseless repetition of Haeckel's offence of faking illustrations, and is simply a disgrace. The second thing is this, that in practically all our elementary and secondary schools this unproven, and increasingly disproven, hypothesis is being taught to the young. That is an outrage upon truth and justice.

We ourselves need to be continually on our guard against "thinking evolutionarily." That is a besetment of our day, full of peril; and Mr. Dewar is really helping us to think independently and to think clearly.

Lieut.-Col. L. M. DAVIES, R.A., said: I welcome Mr. Dewar's paper, every word of which is true to fact. The more detailed series of the evolutionist always lie between very narrow limits. It has repeatedly been pointed out, by the more serious minded

evolutionists themselves, that the greater taxonomic groups—the phyla, classes, etc.—always appear suddenly, with little or nothing to link them to other forms. To postulate an embryonic change in order to account for such things is plausible, but wholly unprovable, and itself amounts to an admission that even ontogeny affords no link between very different types, or any suggestion of a gradual transformation from creatures of one great group to those of another.

The possibility of a broadly graded classification exists, it is true ; and I have recently heard a very eminent University lecturer on evolution claiming that the very fact that creatures can be systematically grouped into different degrees of resemblance itself proves the fact of evolution, and is incompatible with creation. I could not follow his argument. There is, to my mind, no reason why creatures should not have been created either like or unlike in any degrees or ways conceivable to an infinitely intelligent Creator ; but the *anomalies* of classification (of which such lecturers generally say little) are extremely hard to account for on any basis of evolution.

Mr. Dewar has instanced the anomalies brought to light by Sir John Tomes and his son in regard to the details of dental structure ; let me quote another instance of the same sort. All living Mammals are supposed to have had a common origin in some ancient non-placental form. After long ages, the Placentals are supposed to have separated off from the others (Marsupials, Monotremata, etc.). Then it is supposed that certain of these Placentals, after a further prolonged period, succeeded in developing a typical dentition, consisting of 44 teeth (including 12 molars), to which all existing orders of Placental Mammals conform, except the Edentates and the Cetacea. Teeth may be lost ; but such as remain fall within the limits of the typical dentition. Then came another long interval, by the end of which our Typidentate Placental Mammals had further subdivided into two groups—the Deciduata and the Indeciduata. Then (to shorten the story) the Deciduate Typidentate Placental Mammals themselves finally divided into two groups, the Zono-Placentals and the Disco-Placentals. After this, the Zono-Placentals still further split apart into Carnivora of various types, etc.

So far, so good. But what are we to say when we find that one of our living Carnivora of to-day—the OTOCYON, an African animal allied to the dogs and foxes—is seen to possess 46 to 48 teeth, instead of the orthodox permissible maximum of 44—a fourth molar being always present on each side of the lower jaw, and often of the upper jaw as well? According to evolution, this animal has not yet succeeded in acquiring the Typidentate formula, and is thereby more primitive and more nearly allied, *e.g.* to the Whales, than the very earliest and most generalized known members of his order; although in every other known respect he fully conforms to the family characteristics of certain most up-to-date members of that order.

Geology, as Flower and Lydekker pointed out (*Mammals*, pp. 554–5), knows of no suitable ancestors for Otocyon. No shuffling of the classification scheme can account for him. On an evolutionary view of nature, he is, so to speak, a twin-brother, whose only possible common ancestor with his fellow-twin is their (always theoretical) great-great-great-grandfather; for all the nearer ancestors of his twin-brother entirely disown him. Classification abounds with such anomalies, which our text-books and official lecturers keep well out of the way of the rising generation of scientists, lest their young and plastic minds be adversely affected to the great doctrine of evolution.

As regards Mr. Dewar's "mathematical" argument, it seems to me to be eminently reasonable, and much to the point. It suits the evolutionist (it always has done so) to draw unlimited drafts on the unknown. This is all the more necessary to his credit when it is seen what a remarkably small way the geological record will take him along the road to completing his multifarious genealogies. But is he really entitled to suppose that the geological record is actually as incomplete as its failures to support him compel him to plead? Mr. Dewar gives us—it seems to me—very good reason for denying this.

Genera are relatively long lived. The Horse, in his modern form, goes back to the Pliocene; Bears and Camels to the Miocene, etc. In other words, genera are capable of existing through several of the stages into which Mr. Dewar has divided the Tertiary. When, therefore, he shows that the fossil genera discovered in each of these

several stages are about equal in numbers to the genera known to be alive to-day (and our knowledge of existing forms of life is pretty complete by now), on what grounds can we claim that the fossil record (so far as *genera* are concerned) is hopelessly incomplete? By the amount that we propose to multiply the known numbers of fossil genera, we propose that life in the past was richer in types than it is at present. Here the evolutionist is seen to abandon his own favourite principle of extending the present into the past. If his opponent did such a thing—*i.e.* postulated abnormal conditions in the past in order to escape from a theoretical impasse—the evolutionist himself would be the first to protest. When he himself requires to do this, his objections to the practice seem to melt away. But that would seem to be no reason why *we* should ignore Mr. Dewar's exposure of the "imperfection of the record" plea.

### Lecturer's Reply.

I find myself in the happy position of agreeing with all those who have taken part in this discussion, most of whom have dotted the i's and crossed the t's of my paper, and added valuable notes to it. I agree with Mr. Clarke, that in the present stage of knowledge, it would be advantageous to distinguish between differentiation and evolution; if changes within the ambit of the natural family were described as differentiation and only greater changes called evolution, it would be seen that there exists no proof that *any* evolution has taken place.

A number of biologists hold views similar to those of Berg, to which Mr. Claringbull has called attention, *e.g.* Bather, Sergi, Kleinschmidt, Dacqué and Clark. Theories of this type get over the difficulty of the lack of fossils linking the great groups of organisms, but seem to me to be even less compatible than the Darwinian type of theory with the fossils we know. For example, the theories of the latter type demand only one series of fossils leading from the invertebrates to the vertebrates, while the former require as many series of fossils as there are lines of descent.

Mr. George Brewer emphasizes the stability of species. This has been demonstrated by the experiments of Morgan and his associates, who, since 1910, have bred selectively over 500 generations of the fly, *Drosophila melanogaster*, and found that every one of

the innumerable varieties they have bred is fertile when crossed with the parent form. On the other hand, this species will not cross with other species, such as *D. virilis*, although these often differ in appearance from *melanogaster* far less than do many of the forms bred by Morgan.

I agree with Dr. Morton's statement that Britian deeply needs the independent scientific voice, at any rate as regards biology. The reason why this voice is so rarely heard is that evolution has become a scientific creed. Those who do not accept this creed, are deemed unfit to hold scientific offices; their articles are rejected by newspapers or journals; their contributions are refused by scientific societies, and publishers decline to publish their books except at the author's expense. Thus the independents are to-day pretty effectually muzzled.

Those who believe evolution to be a law of nature are convinced that there must be something wrong with experiment or observation that tends to discredit evolution. Facts are to-day deemed to be of little value or worthless unless they afford evidence of evolution. Recently Mr. Levett-Yeats and I spent much time in collecting statistics of mammalian fossils. We embodied the results in a short paper in which we confined ourselves to facts, and avoided all comment. We sent this paper to the Zoological Society of London, in the hope that it would be published in the Proceedings of that Society. The Secretary returned the paper with the following remarks: "I am sorry, but the Publication Committee cannot accept your paper. We got the opinion of a first-rate palæontologist and geologist about it, and he told us that although it must have taken a very long time to compile it, he thought this kind of evidence led to no valuable conclusion."

From his point of view, the palæontologist was right in advising the Society not to publish the paper: the facts the paper contained being unfavourable to evolution. Those who are not confirmed evolutionists will, on the other hand, think the evidence valuable, so that if the Victoria Institute agree, the gist of the statistics will be printed in their *Journal* as an appendix to my paper.

*Otocyon*, cited by Col. Davies, is an excellent instance of the kind of difficulties which the evolutionist meets at every hand. The tendency is to brush aside such difficulties as trivial matters of

which doubtless solutions will eventually be found. In my humble opinion this persistent disregard of inconvenient facts is a great stumbling-block to the advancement of the biological sciences.

STATISTICS COMPILED BY G. D. DEWAR AND G. A. LEVETT-YEATS.

## TABLE I.

### LIVING GENERA OF MAMMALS OF WHICH WE HAVE FOUND RECORDS OF FOSSILS.

[* *Denotes not known earlier than the Pleistocene.*]

*Primates.*—Of the 40 living genera, 40 per cent. are known as fossils, *i.e.* the following 16 :—Anthropithecus, *Brachyteles, *Callicebus, *Cebus, Cercopithecus, *Hapale, Hylobates, *Indris, Lemur, Macacus, *Mycetes, Papio, *Propithecus, Rhinopithecus, Semnopithecus, Simia.

*Insectivora.*—Of the 35 living genera, 42·86 per cent. are known as fossils, *i.e.* the following 15 : *Blarina, *Centetes, *Chrysochloris, Crocidura, Crossopus, Erinaceus, *Macroscelides, *Microgale, Myogale, *Ptilocercus, *Scalops, *Scapanus, Scaptonyx, Sorex, Talpa.

*Edentata.*—Of the 13 living genera, 60 per cent. are known as fossils, *i.e.* the following 8 :—*Chlamydophorus, Dasypus, Manis, Orycteropus, *Priodon, *Tatusia, *Tolypeutes, *Xenurus.

*Rodentia.*—Of the 157 living genera, 59·24 per cent. are known as fossils, *i.e.* the following 92 :—Abrocoma, *Acodon, Acomys, Aconaemys, Alactaga, *Arvicanthis, *Atherura, Aulacodus, *Bathyergus, *Cannabatomys, *Capromys, *Carterodon, Castor, *Cavia *Cercolabes, *Chaetomys, *Coelogenys, *Colomys, *Conilurus, Cricetus, Ctenomys, Cynomys, *Dactylomys, *Dasyprocta, *Dicrostonyx, Dipodomys, *Dipus, Dolichotis, *Dryomys, *Echinomys, *Ellobius, Eliomys, *Erythrizon, *Evotomys, *Fiber, Geomys, Gerbillus, Haplodon, Holochilus, *Hydrochaerus, *Hypogeomys, Hystrix, *Ichthyomys, *Kerodon, Lagomys, Lagostomus, Lepus, Lophiomys, *Loncheres, *Lophuromys, *Marmotta, *Mastacomys, *Megalomys, *Mesomys, Microtus, Mus,

Muscardinus, Myocastor, *Myodes, Myoxus, *Nectomys, *Nelomys, Neofiber, *Neotoma, Nesokia, Ochotona, *Otomys, *Oxymycterus, Pedetes, *Perodipus, Perognathus, Peromyscus, *Phyllomys, *Plagiodon, *Platycercomys, *Pteromys, *Rhipodomys, *Rheithrodon, *Reithrodontomys, Rhizomys, *Scapteromys, Sciuropteris, Sciurus, *Sicista, Sigmodon, Siphneus, Spalax, Spermophilus, *Synaptomys, *Tamias, Thomomys, Xerus, Zapus.

*Carnivora.*—Of the 55 living genera of terrestrial carnivores (Fissipedia), 66·66 per cent. are known as fossils, *i.e.* the following 33 :—Ailuropus, Ailurus, Arctogale, Arctonyx, Bassariscus, Canis, Conepatus, *Cryptoprocta, Cyon, Cynaelurus, Felis, *Galictis, *Genetta, *Gulo, Herpestes, Hyaena, Icticyon, Ictonyx, Latax, Lutra, *Lycyon, Meles, Melursus, Mellivora, Mephitis, Mustela, *Nasua, Procyon, Putorius, *Spilogale, Taxidea, Ursus, Viverra. Of the 9 living genera of aquatic carnivores (Pinnipedia), 77·78 per cent. are known as *i.e.* fossils, the following 7 :—*Chrystophora, Halichoerus, Monachus, Ogmorhinus, Otaria, Phoca, Trichechus.

*Hyracoidea.*—Fossils have been found of the only living genus, Hyrax, *i.e.* 100 per cent.

*Proboscidea.*—Fossils have been found on the only living genus, Elephas, *i.e.* 100 per cent.

*Ungulata.*—Fossils have been found of the three living genera of the odd-toed ungulates (Perissodactyla) ; Equus, Rhinoceros, Tapirus, *i.e.* 100 per cent. Of the 61 living genera of the even-toed ungulates (Artiodactyla), 77·05 per cent. are known as fossils, *i.e.* the following 47 genera :—Alces, Antidorcas, *Antilocapra, *Antilope, Auchenia, Bos, Boselaphus, Bubalis, Camelus, Capra, Capreolus, *Cariacus, *Cephalophus, Cervulus, Cervus, *Choeropsis, Cobus, Connochaetes, Damaliscus, Dicotyles, Dorcatherium, Gazella, Giraffa, *Haploceros, Hemitragus, Hippopotamus, Hippotragus, *Hylochaerus, Moschus, Nemorhaedus, Okapia, Oreotragus, *Oryx, *Ovibos, *Ovis, Pantholops, Phacochoerus, Rangifer, *Redunca, *Rupicapra, *Saiga, Strepsiceros, Sus, Taurotragus, Tetraceros, Tragelaphus, Tragulus.

*Cetacea.*—Of the 29 living genera 68·97 are known as fossils, *i.e.* the following 30:—Balaena, Balaenoptera, Delphinapterus, Delphinus, Globicephala, Hyperoodon, *Kogia, Lagenorhynchus, Megaptera, Mesoplodon, Monodon, Orcinus, *Phocaena, Physeter, Platanista, Pseudorcha, Steno, Stenodelphis, Tursiops, Ziphius.

*Sirenia.*—Of the 2 living genera fossils are known of Manatus, also of the recently extinct *Rhytina, *i.e.* 66·66 per cent.

*Monotremata.*—Fossils are known of all 3 living genera *viz.*, Echidna, Ornithorhynchus, Proechidna, *i.e.* 100 per cent.

*Marsupialia.*—Of the 39 living genera 41·03 per cent. are known as fossils, *i.e.* of the following 16:—Æpyprymnus, *Bettongia, *Chironectes, *Dasyurus, Didelphys, *Macropus, *Peragale, *Perameles, *Petrogale, *Phalanger, *Phascolomys, *Potorous, *Pseudochirus, *Sarcophilus, *Thylacinus, *Trichosurus.

*Chiroptera.*—Of the 215 living genera of bats, 17·67 per cent. are known as fossils, *i.e.* the following 38:—Antrozous, *Artibaeus, *Atalapha, Brachyphylla, *Carollia, *Chiroderma, Cynonycteris, *Desmodus, *Glossophaga, Kerivoula, *Lophostoma, *Lonchoglossa, *Molossus, *Monophyllus, *Mormoops, Myotis, *Natalus, Noctilio, Nyctinomus, *Phyllonycteris, Phyllorhina, *Phyllostoma, *Plecotus, Pteropus, Rhinolophus, Rousettus, Saccopteryx, *Schizostoma, Stenoderma, *Sturnira Synotus, Taphozous, *Tylostoma, *Vampyrops, *Vampyrus, Vespertilio, Vesperugo, *Barbastella.

TABLE II.

| Type of Mammal. | No. of Genera now living. | Percentage known as fossils. |
|---|---|---|
| Volant (Bats) .... .... .... | 215 | 17·67 |
| Aquatic (Whales, Sirenians, Seals, etc.) .... .... .... | 41 | 70·73 |
| Land (*i.e.* all mammals other than aquatic and volant) .... | 408 | 57·84 |
| Total .... .... | 664 | 45·63 |

TABLE III.

| Name of Continent. | No. of genera of land mammals now inhabiting it. | Percentage of these known as fossils. |
|---|---|---|
| Europe .... .... .... .... | 48 | 100·00 |
| Asia .... .... .... .... | 134 | 70·15 |
| Africa .... .... .... .... | 145 | 49·65 |
| North America .... .... .... | 71 | 90·14 |
| South America .... .... .... | 86 | 72·09 |
| Australia.... .... .... .... | 48 | 45·83 |

The above statistics indicate that the geological record is fairly complete in the case of mammals other than bats. The latter, owing to their powers of flight, are rarely fossilized.

The continents that have yielded a low percentage of fossils have not yet been well explored by palæontologists.