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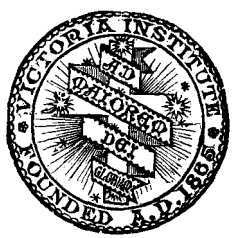

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841ST ORDINARY GENERAL MEETING.

HELD IN COMMITTEE ROOM 19, LIVINGSTONE HOUSE, BROADWAY, S.W.1, ON MONDAY, APRIL 7TH, 1941, AT 6 P.M.

WILSON E. LESLIE, ESQ., IN THE CHAIR.

The Minutes of the previous meeting were read, confirmed and signed.

The CHAIRMAN then called on Mr. Douglas Dewar, B.A., F.Z.S., to read Professor A. Pierson Kelley's Paper entitled "Some Hiatuses in the Plant Kingdom and their Significance."

The Meeting was then thrown open to discussion in which Mr. W. E. Leslie and Mr. Douglas Dewar took part.

SOME HIATUSES IN THE PLANT KINGDOM AND THEIR SIGNIFICANCE

By Prof. ARTHUR PIERSON KELLEY, M.A., Ph.D.

(Being the Dr. A. T. Schofield Memorial Paper).

INTRODUCTION.

IN the year 1859, made memorable by two epochal events in Britain; namely, the coming of a great evangelical revival to the British Isles as recorded by John Shearer, and the publication of the *chef-d'œuvre* of evolutionism, the *Origin of Species*, by Charles Darwin, there appeared another volume which has had a more modest circulation. This book, entitled "*Botany and Religion*, or Illustrations of the Works of God in the Structure, Functions, Arrangement, and general Distribution of Plants," was written by Dr. John Hatton Balfour, F.R.S. (1808–1884), then Regius Professor of Botany in the University of Edinburgh. In reality a textbook of botany, its scholarship and insight into botanical science places the modern crop of botanies in a very unfavourable light; but of greater interest to us is Balfour's incisive analysis of the "transformation theory." He pointed out that there is no evidence of the evolution of one species into another—a fact admitted by a number of modern biologists; he saw that variations always fluctuate about a mean and that

there are boundaries beyond which there is no variation; he asserted that the "evolutionary series" is "very arbitrary"—a fact that is patent to every one who has given serious attention to the subject. He stated that fossil floras do not give any evidence of evolution; and we in our generation are in an even better position to make the same assertion. As a climax to his analysis, Dr. Balfour wisely saw that the real aim of evolution is to rule God out of the Universe.

Much water has ebbed and flowed in the Firth of Forth since Adam and Charles Black published Balfour's book in 1859, but the passing years have only served to emphasize the points made in that volume. It is the object of the present paper to consider the boasted evolutionary line of organisms and to set forth the facts upon which a judgment of that hypothetical line must be based. As a background for the discussion, let us review briefly the natural system of classification as used in systematic botany.

Natural versus Artificial Classification.

The first great system of botanical classification that could be called modern was devised by an Englishman, John Ray; but Ray's system was overshadowed by the System of Linnæus which was based principally on the numerical parts of the flower. The Linnean system was called *artificial* because it was based on arbitrarily chosen characters, while the later systems of the de Jussieus, and the present Engler-Gilg system are called *natural* because based, it is said, upon actual relationships. To-day the natural classification is tacitly assumed to be the tangible expression of evolution and a vital proof of the evolutionary history. It is deemed a fruit of evolutionism while the artificial system is considered the best that Creationism could produce.

Thus Schaffner called classification "the systematizing of evolutionary progressions." (Ohio Journ. Sci. 24: 146–160, 1924.) But the artificiality of all classification is perceived by Dobzhansky, who admirably expresses the true situation thus: "Since the post-Darwin period a 'natural classification' has meant in biology a classification based on the hypothetical common descent of the organisms. This restriction of the meaning of the term is unjustified. The actual mode of descent has been ascertained, and can be ascertained, only for very few

groups. But even granting the possibility of establishing the complete phylogenetic history of every organism, it has never been adequately proven that the degree of similarity between the organisms is always proportional to the closeness of their blood relationships. Some palaeontological data cast a grave doubt upon this point." (Phil. of Science, 2 : 345, 1935.) But the truth is, the natural system was produced under Creationism ; and, again to cite Balfour : " By a careful and extended examination, botanists have arrived at certain facts regarding the symmetry of plants. They have found that, in the arrangement of the organs or parts of plants, the Creator has adopted marked laws as regards number and position . . . (and) . . . their existence has been turned to most important uses by man in his attempts to follow the great plan of Creation—the *natural system*, as it is called " (*l.c.*, p. 288).

Linnæus himself regarded his " artificial system " as a mere convenience, to serve until a " natural system " could be established ; for he called the natural system the " *primum et ultimum in botanicis desideratum.*" And it is significant that at a joint meeting of the Linnean Society and the Association for the Study of Systematics in Relation to General Biology, held on 11th April, 1940, Dr. E. I. White said : " primarily, taxonomy and the study of phylogeny are distinct and not necessarily interrelated," while Dr. Hamshaw Thomas stated : " We are at the parting of the ways ; the rejection of the classical concepts of floral morphology has reduced current phylogeny to a mass of ruins, and a century may elapse before it can be rebuilt. Meanwhile, supposed phyletic trees based on typological morphology have no place in taxonomy, which should follow the principles of a natural classification laid down by Lindley more than a hundred years ago " (Nature, 145 : 636–637, 1940).

In spite of a century of effort to put forth and clarify the " natural system," there is far from any unanimity regarding it. Differences of opinion will be found in regard to many groups within the system, and a number of attempts have been made in recent years to readjust the system and to redistribute the groups, particularly the Amentiferæ and other portions of the subclass Apetalæ.* An unprejudiced observer can hardly escape the conviction that the " natural system " differs from the

* *Cf.* John Parkin (*Nature*, 115 : 340–342, 385–387, 1925). Also Gundersen (*Brooklyn Bot. Gard. Record*, 29 : 63–64, 1939).

"artificial system" in degree more than in kind; that is, that both systems are based on human conceptions, but that the "natural system" is more logically arranged and perhaps, as Dr. Balfour remarked, approaches the Plan of Creation. Dobzhansky (q.v., p. 345) says: "It is customary to make a distinction between natural and artificial classifications. A critical evaluation shows that the 'naturalness' of a classification may vary quantitatively: more and less natural classifications are possible."

Systematic Botany and Evolution.

Does the Natural System show evolution? Did plant life evolve from a primordial slime; or did God say: Let the earth bring forth grass, and the herb yielding seed after its kind, and the tree whose fruit is in its self? Consider the evidence.

The Organic Kingdoms.

The manifest distinction between plant and animal could not be admitted as real by evolutionary biology. Since all life has evolved from less evolved antecedents, according to the disciples of Transformism, it cannot be conceded that plants and animals are actually distinct; there must be intermediate organisms, genetically linking the two. Ernest Haeckel, with characteristic percipiency, invented a new kingdom of *Protista*, or, if we choose,* two new kingdoms of *Monera* and *Protista*, to subsist between the plant and animal kingdoms. Unfortunately for these puppet states, their guarantors have been unable to establish their authority, and their shadowy existence has depended more upon ignorance of the microscopic organisms composing them than upon any actual linkage value they may possess as intermediary between plants and animals. For as Bergson justly remarked ("Creative Evolution," trans. A. Mitchell, 1911, p. 106): "The group (Protista) must not be defined by the possession of certain characters but by its tendency to emphasize them. From this point of view, taking tendencies rather than states into account, we find that vegetables and animals may be precisely defined and distinguished, and that they correspond to

* See also H. F. Copeland (*Quart. Rev. of Biol.*, 13 (4): 383–420, 1938).

two divergent developments of life." And the distinctness of each group within the pretended kingdom of " Protista " becomes more apparent as we study the members of each group. Thus, the establishment of a kingdom or subkingdom of *Monera* to include Bacteria and Blue-green algæ is little justified, because, as has often been pointed out, the Bacteria and the Cyanophytes have more differences than resemblances. Moreover, the formation of a kingdom proper of Protista out of such radically diverse groups as Flagellates, Diatoms, Red Algæ, Brown Algæ and Fungi, borders on the absurd.

For convenience we will consider the Plant Kingdom as comprised in four major divisions of Thallophytes, Bryophytes, Pteridophytes, and Spermatophytes; but we will first briefly touch on the hiatuses amongst the so-called " Protista."

Hiatuses Amongst the " Protista."

These organisms were said to be neither plant nor animal, but an evolutionary complex of unevolved creatures similar to those which æons ago were ancestors of the present algæ and protozoa. But it must be observed that the Flagellates which are thus made to stand sponsor for the alleged common ancestry are less studied organisms, and all the " Protista," as they become known, are seen to be remarkably distinct and unrelated. Not that such distinction is essential to Creationism: we are merely interested in fact.

Bacteria and their neighbours, the Viruses, have been studied intensively in recent years and are seen as discrete and unrelated groups. Each has its own special characters, as ably set forth by Sir Patrick Laidlaw in his *Virus Diseases and Viruses* (1939). Bacteria form a natural group and perhaps we might call them a phylum; but as to their " relationships," only wild guesses have been put forward, as that they are derived spermatia of fungi (Hallier) or that they have come from plastid cells of fungi. In reality, the bacteria form a world of their own, being complex in structure, vastly numerous in kinds and functions*; and entirely without known relationships. The viruses are considered bodies degenerated through a parasitic life; and speculation is rife concerning their origin and being.

* See O. Rahn (*Zentbl. Bakt.*, 2 Abt. 100 (18/23): 369–372, 1939).

The Bacteria and the Blue-green algæ (Cyanophyceæ) have been grouped together as Monera, but that grouping took place in the days of crude microscopes and biological ignorance. One could as well group the Walrus and the Royal Palm together because both are alive! The Cyanophytes form a remarkable and highly individual assemblage of plants and have no known relationships.

An instructive volume could be written about those microscopic organisms called the Flagellates. They comprise several natural groups, and as they become known they appear as remarkably distinct and unrelated. The *Chrysophytes* are plants possessing vegetable structure and mode of nutrition, but are unusual in some of their habits. West says ("British Freshwater Algæ," p. 320): "the Chrysophyceæ exhibit an astounding diversity of development for which there is perhaps no parallel in other classes." Yet the group is little studied, its classification is highly artificial, and we cannot be sure but that it includes, as it now stands, organisms which are not Chrysophytes. The *Chloramonads* are a small group of fresh-water flagellates, their relationships, if any, unknown. The *Cryptophytes* are mostly marine and are not very well known. The *Euglenoids* are common in water that is rich in organic matter. By inclusion of the holozoic *Peranemaceæ* in the group, the Euglenoids are made to do duty as a bridge between the Protozoa and Plants; but by separating the true Euglenoids from organisms that superficially resemble them, it may be that the bridge disappears. Similarly, the *Dinoflagellates* are usually made to include the holozoic *Gymnodiniaceæ*, which probably should be separated off. Thus in all cases the Flagellates have a questionable linkage value; and as they become better known their uniqueness becomes more apparent.*

Hiatuses amongst Thallophyta.

We cannot detail in this paper all the natural groups of the algæ and the fungi, those tissue-less plants that together form

* The recent report made by Dr. S. O. Mast at the 8th Pan-American Scientific Congress of autotrophism in the flagellate, *Chilomonas*, only emphasizes the uniqueness of the flagellated organisms. According to Dr. Mast, *Chilomonas* is able to build starch in the absence of sunlight and chlorophyll. While unconfirmed, the report is quite in line with what has long been known of other autotrophic organisms. It is evident that much work must yet be done on the Flagellates before we can draw rigid general conclusions about them.

the one-time division of the Thallophytes. The Algæ include the Cyanophytes, Diatoms, Conjugates, Green algæ, Charophytes, Brown algæ, and Red algæ; while the true fungi comprise Algal fungi, Sac fungi, and Basidial fungi. The Slime molds (Myxomycetes) are another markedly individual and isolated group, while Lichens (composed of alga and fungus living in symbiosis) are simply a conundrum to the developmentalist. Consider these groups in more detail and realize that according to modern biology they *must* be in some way interrelated. The Creationist does not have to interrelate organisms, but the evolutionist is *compelled* by his hypothesis to suppose that *all organisms* are *blood-kin.* Hence evolutionary biology solemnly sets itself to the task of finding clues to the suppositious interrelationships, and much of biological work is directed—openly or furtively—to the finding of the connecting links which are so hopefully desired. Unfortunately for evolutionism, after nearly a century of search, *not a single undoubted connecting link has been found.*

The Green algæ (Chlorophyceæ) have long been the object of evolutionist attention and several imaginary genealogies have been proposed for the members of the group, based on a suppositious transformation of a flagellate into a polyblepharid which in turn became coleochætoid and from thence the distinctly thalloid algæ are supposed to have arisen. Such a genealogy is purely suppositious, based on a mental progression and having no factual evidence or experimental proof.

In fact, the Green algæ do not show evolution, as Dr. F. O. Bower frankly admits: he speaks of the "conservatism of the Green Algæ" which prevented their "evolutionary advance. . . . Here lies the gap between green aquatic and green amphibian life. There is no use in ignoring it, nor yet in filling it by hypothetical transmigrants that no one has seen, and that we are expressly told we shall never see" ("The Origin of a Land Flora" (1929)).

The Brown and the Red algæ are still more difficult for the evolutionist, the latter group being rather avoided or dismissed as a "specialised offshoot." The Red algæ are decidedly involved in their life history; and in production of non-motile, passive egg and sperm, and elaborate reproductive organs, they are quite suggestive of "higher" plants. As to their "origin," the best that modern science can say is that it is "uncertain."

The Fungi are even more puzzling. The task of assigning an origin for any fungal group presents tremendous difficulties and when we consider that many fungi are symbionts of the most intimate sort, and that the symbiont and the host must have evolved together in the most intimate relation, according to evolutionism, the task of assigning an evolutionary origin to the fungi becomes almost insuperably difficult. The few guesses as to the origin are not even plausible : thus, the suggestion that Rusts were derived from Red algæ is palpably absurd because it would involve the evolution of an extremely complex " finished " group " from a complex, " finished " group. Evolution is supposed to proceed from young nascent stocks, not from old, highly-evolved end branches of the process. There is no suggestion regarding the origin of any group of fungi that has met with any general credence, and we can still agree with Strasburger's old assertion that the origin of the fungi is " uncertain."*

Surveying all that numerous assemblage of organisms which older botanists rejoiced to call Thallophyta, we find not one single case of linkage amongst the discrete groups. From viruses of 10 micromu up through bacteria to the highest alga or fungus we find only discontinuity and unbridged hiatuses. The wood is indeed full of trees but the branches always adhere to their own trunk ! If the " lower " plants evolved, they have kept their secret well, for not even the modern microscope has compelled them to interrelate ; but on the contrary, research has simply emphasized the isolation and discreteness of the lower plant groups.

Bryophytes and their Origin.

Coming next to Liverworts and Mosses, we meet the division which botanists of a former generation called Bryophyta. Here again we find only doubt and uncertainty amongst evolutionists as to the origin of the liverworts, while as to mosses, the phylogencist metaphorically throws up his hands in despair. Thus O. E. Jennings writes : " The origin of mosses is one of the most

* Dr. J. Ramsbottom states " that in the fungi the fossil evidence is quite unreliable and that the so-called phylogenetic trees from time to time published for this group are based on such superficial evidence as to be sheer nonsense " (*Nature*, 145 : 637, 1940).

interesting mysteries of paleobotany" (*Bryologist*, 31: 10–15, 1928). A generation ago Strasburger write: "There are difficulties in the way of the phylogenetic derivation of the Bryophytes from any definite group of Algæ," and the difficulties have not decreased with passage of time. Naturally the evolutionist must derive Bryophytes from Algæ, and Strasburger would have derived them from Brown algæ. But D. H. Campbell—no mean authority—said: Not so; it is from *green* algæ that the Archegoniates (Liverworts and other plants provided with an archegonium) arose. Yet Campbell was uncertain, he wrote: "The direct origin of the simple gametophyte of such a liverwort as *Aneura* or *Anthoceros* from some Confervoid type is readily conceivable, but the very great difference in the complexity of the reproductive organs between even the simplest Liverwort and any known Alga forbids the assumption of any but a very remote connection between them" ("Mosses and Ferns" (1918), p. 564). Not a very positive knowledge of the origin of Liverworts, we should say. And no one has come much closer. As to Mosses, Campbell deems them "one of the most sharply defined and specialized groups of plants known to us. . . Their relationship with other forms is at best a somewhat remote one." That judgment is correct and is generally agreed to by botanists.

Two of the four older divisions of the plant kingdom have been reviewed from the pinnacle of Twentieth Century knowledge; and nowhere in the Thallophyta or the Bryophyta have we found the boasted evolutionary line. On the contrary we have met anxious botanists with furrowed brows and faces lined by arduous study, still searching for the illusive connecting links and phylogenetic progressions that *must* exist since evolution is true. How much those self-sufficient younglings are to be envied who make up a chart of the phylogeny of some vegetable group and proceed to believe it, not just thinking it might be true but actually *believing* that their cerebration is a *fact*!

Isolation of the Vascular Plants.

Since two divisions of the plant kingdom fail to show evolution, we must find the *sine qua non* of vegetable existence in the remaining two—Pteridophyta and Spermatophyta. But the

Ferns (Pteridophyta) at once bring us to a pause; they say: Evolution is not with us. As Sinnott (Stirling Professor at Yale) justly points out: "Between Mosses and Ferns there are such fundamental divergences, if one is willing to consider all the facts, as to warrant the statement made by the writer, . . . that in passing from Bryophytes to the Pteridophytes . . . we *cross the widest gap which exists in the continuity of the plant kingdom.*" (Italics by Ed.) If Ferns and "Fern Allies" are not to be derived from the "lower" plants, from whence did they come? There is no answer in evolutionism to the question.

So, too, with the Seed Plants (Spermatophyta), which include the Gymnosperms (pine trees and many others) and Angiosperms (the higher flowering plants). Now, Seed Plants are particularly hard on evolution because, first, the Gymnosperms refuse to be derived from cone-bearing "Fern Allies": they not only decline to be derived on present-day morphological evidence but they settle the question by turning up in the geological record before their reputed ancestors. Then, secondly, the Angiosperms are the most isolated of all plant groups, for, like Melchizedek of old, they are positively without known antecedents. The evolutionist, indeed, obviates the difficulty by citing the alleged incompleteness of the record, as in the following graceful subterfuge* of Pulle ("Remarks on the System of the Spermatophytes" (1937), s. 3). "Strange to say, the phylogenetic tree of the Angiospermæ is a tree of which we know the foliage only; a tree on the crown of which we look from above, but whose dense foliage does not allow us to see the structure of its branches." But it is the inflorescence and not the foliage that is used in the phylogeny of Angiosperms and the attempted derivations are merely comparisons, resting on mental conceptions and not on experimental or other proof. Neither the inflorescence* type, nor the gynœcium,* nor any other organ of the Angiosperm has yielded evidence of relationship. Similarities exist, it is true, and markedly so within "families." For example, R. P. Wodehouse found (Ann. Bot., 42, 891–934, 1928) groups of species within the family Compositæ having similar pollen grain characters; but whether these similarities actually prove genetic affinity or not is an highly debatable question.

* W. Zimmerman (*Beih. z. bot. Centralbl.*, Abt. A, 53: 95–121, 1935; Hagerup, O (*D. kgl. Dansk. Vidensk. Selskab. Biol. Med.*, 13 (6): 1–60, 1936.

Discontinuity in Germinal Phenomena.

Thus the Plant Kingdom from beginning to end shows only unbridged gaps, and the earnest student who cares to delve still deeper into the evidence will find confirmation of the analysis so briefly made here. Not only in the larger groups, but likewise in the smaller, there is seen distinctness and not a gradual progression from one sort to another. The discontinuity of the Plant Kingdom is its chief character. And now that discontinuity is so patent that it cannot be denied, transformist biologists, who sought for decades to establish *continuity* as a fact and searched unceasingly for connecting links, suddenly deploy with Jesuitical duplicity and say: Ah yes; we knew all along that the larger groups of organisms are discrete and discontinuous, but the discontinuity is only apparent and organisms are actually interrelated through a previous mechanism of mutation and isolation.

Realizing the undeniable discontinuity of organic groups, evolutionists have been compelled to fall back on possible transformism occurring in the germ plasm. Evolutionism has been marked by masterly retreats; and it is the newest flair in biological research that is always about to reveal the long expected solution of the evolutionary problem, just as the hopeful prospector is always just about "to strike it rich" in some newly discovered vein. To-day it is the germ plasm that is to be the El Dorado, and there has been a marked revival of interest in the "species problem" in recent years. It is not possible here to discuss the species question; but we may say tersely that the consensus of biological opinion agrees with what any layman can see with his eyes, that there are species of organisms.* These species depend for their existence upon the genetic constitution of the germ plasm.

* "It has been contended by many authors that the grouping of individuals into species is merely a matter of convenience, since species have no existence apart from the mind of the investigator. As a proof of this contention, it has been pointed out that such criteria of species distinction as the production of sterile hybrids sometimes break down because some forms which are classed as species can be crossed experimentally and can produce semi-fertile or fertile hybrids. This point of view is fallacious, and is based on a failure to understand that the fact that some species can be crossed and can produce fertile hybrids does not prove that these species cross regularly in nature. *Species is a dynamic rather than a static entity* (italics by Ed.), and the essential feature of the process of species differentiation is the formation of discrete groups of individuals which are prevented from interbreeding with other similar groups by one or more isolating mechanisms" (Th. Dobzhansky in *Amer. Nat.*, 71: 404–420, 1937).

The tremendously important question for biology is this: Are species discrete, or are there "cases of continuous variability"? The evolutionist points to series of species arranged in neat progression, as those of the famous horse genealogy amongst fossils, or the distinct and incomparable cases offered by such genera as Rosa, Rubus and Hieracium. It would take us far afield to consider the fossil record of the horse, but even admitting the record as factual it is obviously of a different category to that of living "species complexes" which are separable into a number of allied forms. Since these complexes have offered a fascinating field for cyto-genetics, they are being much studied and the hopeful cry has been raised that in polyploidy and related phenomena the origin of new species has been discovered. These hopes seem doomed to disappointment,* for with continued research the adamant stability of species remains evident.

Stability of species depends in the last analysis on those metaphysical entities known as "genes." Since the characteristics of an organism seem manifestly determined by the genes, evolution must perforce commence with the gene. Thus Gulick remarks (*Quart. Rev. of Biol.*, 13: 164, 1938): "As part of the evolutionary process, a gene *must be* (italics by Ed.) credited with a liability for undergoing chemical alteration to produce a new gene substance with a slightly different molecular constitution, capable of autocatalysing itself, including the new item in its constitution." In other words, because evolution is true, the genes (ultimate determiners of organic kind) *must* transform! But how such an hypothetical transformation could take place is still a puzzle: "All genetic evidence accumulated so far indicates that the gene offers an efficient mechanism

* Compare the following: "The evidence from the plant kingdom as a whole, therefore, suggests that polyploidy has been most important in developing large, complex and wide-spread genera; but that in respect to the major lines of evolution, it has been more important in preserving relics of old genera and families than in producing new ones" (G. L. Stebbins, *Amer. Nat.*, 74: 54–66, 1940). So, too, with supposed evolution by chromosomal change, Brink, *e.g.*, stating in regard to supposed origin of new species in Crepis and Datura by loss of chromosomes: "We cannot positively assert that the process in question is significant for evolution until one case at least is shown to be of this (positive selective) nature" (*Amer. Nat.*, 69: 97–124, 1935). And even ecological variants seem governed by a strict definity, as pointed out by Bergström (*Nature*, 145: 316–317, 1940).

for the evolutionary progress of living organisms. *Just how this progress is accomplished is not known* (italics by Ed.), but Genetics may soon be in a position to offer a more definite evidence on this subject than is available at present" (M. Demerec, *Amer. Nat.*, 69: 125–138, 1935).

Assertions of continuity in the organic world are still premature. The "syngameon" may have larger boundaries than have yet been indicated, but within the true species apparently the genetic constitution remains fixed and hiatuses still persist unbridged even in minute groups of the classifier. The possibility of genetic continuity is not ruled out but it *is* unproved. The actual state of the plant kingdom is this: the larger taxonomic groups are unlinked, and so, too, are the smaller; the species are dynamically discontinuous, and the "genes" form unique systems which determine a discrete specific form.

We have examined the living plant kingdom and found hiatuses between the taxonomic groups, between the species, between the gene-systems. Let us now turn to fossil plants and learn what they teach us.

The Value of the Geological Record.

The importance of Historical Geology to the Hypothesis of Organic Evolution has long been recognized, and T. H. Huxley's famous dictum is widely quoted: "The primary and direct evidence in favour of evolution can be furnished only by palæontology. The geological record, so soon as it approaches completeness, must when properly questioned, yield either an affirmative or a negative answer: if evolution has not taken place there will lie its refutation."

A very important question which is basic to the whole subject of Historical Geology has yet to be adequately treated by modern science, namely, the worth of Lyellian chronology. Devised in an early day by partial methods, it has universal acceptance by custom rather than by an intelligent appraisal of its worth, and the whole of Historical Geology must be viewed with reserve by one who demands a critical evaluation of the truth. It is unfortunate that the only modern critics of Lyellian geology should be content with setting up a rival hypothesis which has

far less basis in observational data; and that a thoroughgoing critique of Lyellism* is still lacking.

For the present, all that the botanist can do is to utilize the terminology of current geological science, and to make the best use he can of the fossils, for *they* at least have tangible existence.

Accepting Lyellian chronology, however, at face value, we find its records most contradictory to current evolutionary teaching. Evolutionism teaches† of a slow gradual progression from "primitive" marine algæ to beach or marsh plants, and thence to the vascular land flora. Lyellian geology tells us that land plants appear with the "primitive" aquatic plants, that "highly evolved" vascular plants preceded in appearance their reputed ancestors. The fact that Gymnosperms with structure as "highly evolved" as any living today existed "as early" as Lower Devonian is too well known to require discussion. Further, that land plants were "highly evolved" and widely distributed in the Silurian is also too well established to be questioned; thus, we may cite the Psilophyton-like plants of Gothland and England, and the leafy stemmed plants of Australia, all found in the Silurian; and still more impressive is the discovery of an Annularia-like plant from the Ordovician of England. And thus, if we accept Lyellian geology, we must (if we are truthful) accept what that geology teaches, namely, that the vascular plants appear about as early as the non-vascular plants, and that there is not even a hint of evolution to be discerned in the record.

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* G. R. Wieland says (*Nature*, Feb. 10, 1940): "there came Huxley's famous fling: 'Geologists had imagined that they could tell us what was going on at all parts of the earth's surface during a given epoch; they have talked of this deposit as being contemporaneous with that deposit, until from our little local histories of the changes at limited spots of the earth's surfaces they have constructed a universal history of the globe as full of wonders and portents as any other story of antiquity.'" And note that Wieland also says: "As justly though, Huxley admitted that, 'It was Lyell who had smoothed the road for Darwin'."

† A recent book on palæobotany presents the following summary of its content: "it has been amply demonstrated in this survey that in the near past there lived many species and genera which have now become extinct, and that, as one searches the record of the remoter past, families, orders, classes, and phyla gradually 'disappear' until in the mid-paleozoic only marine thallophytes are known to us. Yet this record is not merely the elimination of types; it is the directed course of evolution—specialization, modification, and diversification" (quoted from W. C. Darrah's "Principles of Paleobotany," 1939, a book which reflects far more credit on the printer than on the author). The quoted paragraph is egregious falsehood, inexcusable in a profession palæobotanist.

The doubts and tergiversations of the Lyellian palæobotanist are pathetic. For example, there can be little doubt that Sir William Dawson's *Prototaxites* (*Nematophyton*) was a well-developed land plant of the Silurian; but, since according to evolutionism land plants could not exist in the Silurian, Lyellists stand before Nematophyton with perplexed bewilderment. Witness Seward, who states ("Plant Life Through the Ages" (1931), pp. 119–120) that the plant was found in an obvious peat bog and hence could scarcely have been a marine alga; it can be argued that it was a land plant; "It probably grew on swampy ground and must have reached the dimensions of a tree"; and the structure of the stem "reminds one of the stem of a conifer that has been partially destroyed by the ravages of a fungus." But, since conifers could not have existed in the Silurian (else evolution would be untrue), Nematophyton is considered a queer anomalous fossil which cannot be explained.

Fossil Record of Thallophytes.

There is little need to pause over the fossil algæ. Being either minute or rarely possessed of hardened structures, their preservation has been difficult or wanting except in certain cases. Yet in an indirect way they have shown their presence by the formation* of ironstone, graphite, and in some cases, limestone; and thereby have precipitated a discussion as to the earliest appearance of life on the earth. It is not in our province to enter a discussion of possible pre-Cambrian fossils, nor to enquire into their comparative age.

With preservation of algæ so difficult, we cannot draw very positive conclusions from a consideration of their fossils. It would be unfair to point out that the fossils from Cambrian to Silurian that are considered algal belong to the "more evolved" groups of the algæ, and that Green, Brown, and Red Algæ appear to be contemporaneous, because we cannot be certain of anything about these fossils, and especially as reproductive organs are wanting, it is therefore best to leave the fossil algæ out of the discussion.

As to fungi, traces of them are found from the Devonian onwards. While the records of fossil fungi are in cases called

* *See* Julius Pia, "Pflanzen als Gesteinbildner," 1926.

into question, there are certain examples which are too well known to be doubted, *e.g.*, the fossil mycorrhizæ of the sort first described by Weiss (Ann. Bot., 18, 255–265, 1904). A mycorrhiza is a fungus-root; *i.e.*, the root of a vascular plant harbouring a symbiotic fungus in intimate union; and it is well known that many forest trees and other plants owe their vegetable existence to the lowly partners of their subterranean life. These mycorrhizæ are highly organized in a definite morphological structure. It is significant that exactly the same structure, in all its complexity has been found, not alone in Weiss's Lower Coal Measure plants, but in other fossils, that indicate the existence of an extremely high degree of "evolutionary progress" in the earliest fungi. The first fungi (if we accept current geology) had a structure as "highly evolved" as the fungi of today, and lived in as complex a symbiosis as their modern "descendants." Theorize as we please, we cannot escape these hard facts. Accepting Lyellian geology at face value, we find it showing us that the fungi when they "first appear" were as "highly evolved" as those of the present day.

Fossil Record of Bryophytes.

Liverworts are described from the Coal Measures or Carboniferous of Scotland and England. The fossil liverworts greatly resemble modern liverworts in apparent structure and appear in the rock strata fully formed as liverworts, and without known antecedents.* Indeed, Dr. F. O. Bower himself emphasizes the gap which separates the Liverworts from Algæ and says: "There is no use in ignoring it, nor yet in filling it by hypothetical transmigrants that no one has seen and that we are expressly told we shall never see" ("Origin of a Land Flora" (1909–1929), p. 13).

Nor do Liverworts lead gradually to Mosses, for, as we have noticed, the Mosses exist as one of the most isolated of plant groups. There are not many fossil mosses known except that more recent peat-bog studies are bringing to light some of the older bog mosses; and the earliest authentic moss, according

* Prof. John Walton describes liverworts from coal balls that closely resemble modern examples of *Fossombronia* and *Treubia* (cf. Seward, *l.c.*, 1931, p. 211–212). In other words, when liverworts first appear, they are like present-day liverworts: they do not show evolution.

to current geology, is described from the Tertiary. Seward, however, thinks that *Muscites* was a true Carboniferous moss. (*l.c.*, p. 212).

Ferns and Fern Allies in Fossil Record.

Linkage of fossil Thallophytes with fossil Bryophytes could depend only upon the happy find of rare fossils which are yet unknown, because of the rarity of preservation and difficulty of study of fossil algæ. But linkage of Bryophytes and Pteridophytes rests on a more hopeful basis because there is a sufficiently abundant supply of fossils in these groups to make a reasonable study possible; and meticulous studies have in truth been made. In the Rhynie Chert of the Old Red Sandstone (Devonian) in Scotland, fossils occur which are apparently not vascular plants as we know them nor yet exactly liverworts; and these Rhynie fossils were hailed as the long hoped for links between liverworts and vascular plants. "In my view," exclaimed Seward ("Plant Life through the Ages," p. 4, 1931), "nothing of such importance to plant morphology has appeared since Hofmeister's 'Vergleichende Untersuchungen' was published in 1851." Yet *Rhynia* and *Hornea* (*Rhyniaceae*), these would-be linkage plants, appear more as an isolated and unrelated group than as evolutionary connecting links. D. H. Scott has been careful to state ("Extinct Plants and Evolution," 1924, p. 192): "It is possible to interpret the family (*Rhyniaceae*) as a synthetic group, related to both the Vascular Cryptogams and the Bryophytes, while still retaining some of the characters of an original Algal stock. Such a conclusion is justified on the facts actually known; but, on the other hand, we can scarcely feel quite certain that the remarkable simplicity of the Rhyniaceæ was wholly primitive. The peat habitat, as already pointed out, was not a very favourable one, and it is possible that plants growing under such conditions may have already undergone a certain amount of reduction. However that may be, the fact remains that the Rhyniaceæ are the simplest and among the most ancient of land plants known to us."

If the *Psilophytales* (to which the *Rhyniaceæ* belong) are, as they appear to be, an isolated group of plants and not a "primitive" linkage group, the liverworts and ferns are still unlinked. And even were the Psilophytales shown to be genetically inter-

mediate between Liverworts and Ferns (and how could the genetic affinity of fossils be shown ?), there would still be significant gaps between these three groups.

In fact, Ferns appear established and without known antecedents in the middle Devonian, before Mosses and Liverworts appear, unless the Psilophytales were truly Liverworts. If the Ferns developed from the Psilophytales, the evolution must have taken place in a hurry ! Even in the Devonian, the Ferns (like *Archæopteris*) had large and pinnately compound leaves with sporangia dotted singly or in groups over the frond. They were very definitely Ferns when they " first appear " in the geological record, and as far as fossils enable us to determine, they have always remained ferns.*

It is interesting to note that evolutionary botany has argued stoutly for the " primitive " character of the Adder's Tongue and Grape Fern Order (*Ophioglossales*) because of the comparatively simple structure of these plants. It was long taught that the Ophioglossaceæ are more or less intermediary between Liverworts and Ferns, and similar to the (hypothetical) ancestors of the Ferns. But the Ophioglossales are almost unknown as fossils, and the few, like *Botryopteris*, that are known, occur long after ferns had appeared in full maturity (accepting current geology). We must now argue as stoutly, therefore, that the Ophioglossales are highly evolved and that they have been severely reduced in structure !

Isolated and extinct groups such as the Lepidophyta need not detain us since no one considers them as anything but isolated and outside any possible " progression." But it is instructive to contemplate others among those groups often called the Fern Allies ; thus, the plants called Horsetails (Arthrophyta) appear " abruptly " in the Carboniferous as Arthrophytes, and the Arthrophytes have remained in existence ever since, unchanged in their principal characters. Indeed, the *Equisetites*, which appear in the Palæozoic seem much like the Equisetums of today ; of their origin, nothing is known.

Similarly, our present Lycopods are antedated by the *Lycopodites* which closely resemble† the living Lycopodiums, although

* Seward says (*l.c.*, p. 147) that only one fern, in a strict sense, is recorded from Devonian rocks ; namely, *Asteropteris*, from New York.

† According to Seward (*l.c.*, p. 187) in the Lower Carboniferous *Lycopodites Stockii* we have an example of what is apparently an herbaceous lycopod *agreeing closely with modern forms.*

found in the Lower Carboniferous. It is strange that in a world of continuous progression we find group after group of plants appearing "suddenly" in the geological record and continuing virtually unchanged through the various rock strata! Selaginella is also represented in the Carboniferous. As to the origin of the Lycopodales, it is unknown.

Summarizing our knowledge of the fossil spore-plants, then, it is evident that isolation and discontinuity prevail amongst them. Nowhere is there evidence of genetic continuity or "continuous variation": on the contrary the student of palæobotany finds only laboured disquisitions that attempt to explain how the isolated groups of fossils *might* be related. Learned writers bewail the paucity of students of fossils; but judging from the entire lack of agreement amongst the aforesaid learned authorities as to how the evolution has taken place, they should be glad that so few labourers enter into their field lest confusion be worse confounded.

Fossil Seed Plants versus Continuity.

It is very generally taught in the schools that fossils, when arranged in the order of their age, show a steady progression from the simplest to the most complex; that, starting with traces of simple algæ, the fossil record indicates the evolution of liverworts, ferns, simple seed plants (half fern and half seed plant), gymnosperms like the cycads, and finally a grand culmination in the beautiful Lily and the Rose. The idea, which is believed by both teacher and pupil, is a bald-faced lie—or a disingenuous prevarication, if the reader is sensitive to rhetoric.

No gradual progression is shown amongst fossil plants; at least, *None is shown by evolutionary geology.* On the contrary, this geology shows the Seed Plants to be as old as, if not older than, the Spore Plants that are supposed to have produced them. As a matter of fact, according to evolutionary geology, the Gymnosperms (not Cycads but "highly evolved" Coniferous-type wood) appear among the earliest known plant fossils.* The "highly evolved" symbiosis of fungi with gymnospermous

* It is a curious point that we have such inadequate evidence for the existence of Ferns in Early Devonian time, while, as Hugh Miller's discovery showed, plants of a much higher grade, very probably of Gymnospermous affinities, were already represented" (Scott, 1924, p. 198).

roots also appears for the first time, unexplained, among the early plants; while Ferns appear along with them, also for the first time and "highly evolved"; and Liverworts make their first appearance afterwards and in the same state of "evolution" in which they exist today. Thus the Liverworts, ancestors in type of the Ferns, and through the Ferns of the Seed Plants, appear in the geological record millions of years after their descendants are fully matured, the latter having awaited serenely and unvarying for millions of years for their forefathers to be born and to assume an earthly existence. Truly, the world is fearfully and wonderfully made, and evolutionism abounds in delightful mysteries and fascinating uncertainties!

Considering first among the Seed Plants those fossil forms known as Seed Ferns or Pteridosperms, we may quote H. N. Andrews: "Probably no group of plants, fossil or living, has ever created as much combined interest for the botanist, geologist and layman as the rather heterogeneous assemblage of vegetative and reproductive "species" included within the Palæozoic Pteridospermæ (*Ann. Missouri Bot. Gard.*, *27*, 51–118, 1940). Andrews quickly dispels the hopeful evolutionist view that Seed Ferns linked the Ferns with Seed Plants by saying: "The view that the Pteridosperms represent an intermediate group between the ferns and the cycads is no longer tenable. Rather we must look to an (hypothetical, Ed.) common psilophytalean-like ancestor with terminally borne sporangia, a solid protostele and primitive secondary wood for the origin of ferns and pteridosperms (the secondary wood being usually lacking in the former. . . ."

The origin of the Seed Ferns is lost in obscurity. Nevertheless Andrews states (*l.c.*, p. 53): "The evidence supplied by fructifications is overwhelmingly in support of the common origin of the ferns and pteridosperms from plants with terminally borne sporangia. . . ." Scott, however, says: "There is not the most distant likeness between the seed of any known Pteridosperm and the sporangium of a Fern."

Incidentally, while considering the Seed Ferns, it may be pointed out that evolutionary botany has long sought to link flowering plants with ferns through possession of scalariform-pitted tracheids. The Magnolia group is the "primitive." group of the Dicotyls; it has scalariform pitting of the xylem But now it is recently shown that the Seed Ferns, the "transi-

tional linkage group," have "no indications of scalariform pitting in their (tracheid) side or end walls." How unkind to discover such an untoward fact after the evolutionary progression was *so* nicely established!

Coming to Seed Plants proper, only the most violent discontinuity is to be found throughout. These groups illustrate a general principle which obtains throughout biology, that the better known the organisms are the more distinctively unique they appear. There may be lengthy arguments about the microscopic Flagellates, of which the structure is not too well known and the habits are obscure; but with large and evident organisms it is obvious that they are discontinuous. It is scarcely necessary to detail the various groups of Seed Plants, since their uniqueness is too well known to need much repetition. Yet phylogenetists continue their persevering attempts to prove community of descent (which must exist because evolution is true) with as much pertinacity as Philip the Second pursued his letter writing in the Escorial through long dreary years.

The Gymnosperms are an odd group (from a phylogenetic standpoint): Phylogeny struggles to make the Cycads "primitive," although evolutionary geology teaches us that Conifers are more ancient. And within the Cycadophyta there are two leading groups, the ancient Cycadeoids and the modern Cycads, both of which are tantalizingly obscure as to their origin and relationships. The strange Gingko dates from the Permian and has been stubbornly opposed to evolution all its days, besides appearing in history without either ancestors or relatives. *Sequoia* and other genera of conifers also appear "suddenly" in the geological record and have failed to show evolution, remaining with little change through their history as recorded by evolutionary geology.

Distinct and unrelated as the Gymnosperms are, they are as nothing to the Angiosperms which appear with the most extreme abruptness in the Cretaceous without known antecedents. This fact does not dismay the disciples of Transformism because there are two obvious explanations of the curious isolation of the Angiosperms, viz. (1) There was a long, long time when fossils were not preserved, and all the ancestors of the Angiosperms (which developed in this remote age) were thus accidentally lost; or, (2) the Angiosperms jumped suddenly into existence by a strange mutation. . . . But the serious student of plants is under no obligation to accept either explanation.

Not only do the various genera of Angiosperms appear in the geological record most abruptly, but they continue through the record with little change to the present.*

Conclusion.

Fossil plants, like living plants, show only hiatuses and discontinuity: there is no organic progression. True, one may *arrange* fossils in any desired order and produce a fancied progression; but no careful botanist, with a regard for the truth, would assert a "continuous variability" amongst fossil plants when the direct evidence for the assertion must necessarily lie totally outside any possible human knowledge. The quest for organic continuity must be made amongst *living* plants (in spite of the late Mr. Huxley); and to date such continuity has not been found. Discontinuity rules amongst plants, and plants still breed true, the grass and herb "yielding seed, and the fruit tree yielding fruit after his kind."

* In a study of the London Clay fossils, Reid and Chandler conclude that the Angiosperms of the London Clays belong to living families but not to living genera; although they admit that the question is one of individual judgment (Reid, E. M., and M. E. J. Chandler, "The London Clay Flora," 1933).

Discussion.

The Chairman (Mr. W. E. Leslie) said: There is Scriptural admonition to be ready to give a reason for the hope that is in us, but when Dr. Kelley brings such charges as Jesuitical duplicity against those from whom he differs, he forgets that we should give it with meekness.

In the early part of the paper the "artificial" and "natural" systems of classification are discussed. It seems that the "natural" system commends itself to evolutionists, but Dr. Kelley quotes with approval a suggestion that it "approaches the Plan of Creation." Is there, then, any congruity between the evolutionary scheme and the Divine Plan?

Our author insists that there is a sharp line between plants and animals. How, then, is it that we find botanists and zoologists disputing as to whether various organisms belong to their respective

departments ? Whether bacteria belong to a " world " of their own or not, they appear to be intermediate between the two Kingdoms.

It is evident that living things are found in groups in space—in water and on land, in hot climates and in cold, etc. Are they also grouped in time ? Dr. Kelley tilts at " Lyellian chronology," but he does not venture to suggest that all sedimentary deposits were laid down simultaneously. Making every allowance for defective dating, is it not a fact that forms can be grouped in order of time? Our author says (p. 13) that the Angiosperms " appear with the most extreme abruptness in the Cretaceous without known antecedents." He does not accept explanations put forward by evolutionists, but how does *he* explain it ? Further, making every allowance for the numerous discontinuities to which the paper calls attention, we must ask whether there is any tendency, however limited, for simpler forms to appear before complex ones ? If there is, then a constructive attempt to explain the fact would be more useful than an exclusive insistence upon the exceptions.

Mr. DOUGLAS DEWAR said : Prof. Kelley will doubtless be able to deal with the Chairman's criticisms, but, in his absence, I may say that the doubts as to the status of some microscopic organisms indicate that our technique at present is not developed sufficiently to reveal many of the characters of these. As regards Lyellian chronology, Prof. Kelley is a botanist and not a geologist, and wishes to indicate that he must not, by using it as the basis of his paper, be deemed to accept without question some of the extravagant claims made by geologists, such as fifty million years as the duration of the Tertiary Period. In the present backward state of knowledge it is unwise to put forward anything more than a tentative alternate theory. Vavilov wrote last year : " The ocean of knowledge is practically untouched by biologists." Even so, the succession of the fossils in the rocks known to us is better explained by migration than by transformation. We are told that the sun is losing by radiation 360,000,000,000 tons a day. This means that its gravitational pull on the earth is weakening and the earth is slowly spiralling away from the sun, with the result that the climate of the earth is growing cooler. Long ago the tropics must have been far too hot for the vast

majority of organisms now living. All that were then suited to the present climatic conditions must have been confined to the open oceans or to the highlands, except near the poles. As the temperature fell the original denizens of the coastal seas and lowlands must gradually have become extinct and replaced by immigrants from the open seas and highlands, and this process must have been frequently repeated, so that each locality has been populated by successive immigrant populations, much as England has been receiving immigrants from Europe—Romans, Saxons, Danes, Normans. Now, if the oceans and continents have all along retained their present relations, the only primary and secondary fossiliferous rocks known to us are those laid down on low-lying land and in the sea near the shore, because we have no access to rocks deposited in the open ocean, and all land rocks laid down at high altitudes have been weathered out of existence. Thus it happens that all the fossils laid down in the primary and secondary periods discovered by us are those of organisms living in coastal seas or lands at the time of their death.

This theory accounts for (1) the successions of floras and faunas in the fossiliferous rocks ; (2) the sudden appearance in the rocks of entirely new floras and faunas not derived from those which immediately preceded them in that locality, particularly the sudden appearance of a great land flora in the Devonian and the flowering plants in the Cretaceous ; (3) the fact that, apart from Man, no new class of plants or animals has appeared in the rocks since the Eocene period ; (4) the sudden appearance at the beginning of the Cambrian of the earliest known animals and plants in great numbers and in many parts of the world ; (5) almost all the fossils known from the Primary epoch being those of marine animals. The last four of these facts are not satisfactorily accounted for by any theory of evolution.

Dr. Kelley had so much ground to cover that it was not possible for him to dilate upon " the extreme abruptness " with which the flowering plants appear in the cretaceous " without antecedents." Prof. Kelley is not exaggerating when he uses these phrases. Let me quote from a paper by the Swedish botanist, Heribert Nilsson, contributed to " Hereditas " in 1938 : " When then did our recent vegetation arise ? and what are the ancestors of the Angiosperms ?

The first question we can answer almost exactly. The Angiosperms appear in the upper layers of the lower chalk. We can also answer the other question, although no one will willingly give a direct and open reply, *we know of no ancestors of the Angiosperms.* In the older chalk an astounding change in the whole vegetation occurs. The Mezozoic flora still occurs in the lowest strata. In the uppermost appears a wholly different, extraordinarily well-developed plant world, a dominant angiospermous one. Such of our genera as *Quercus, Platanus, Autocarpus* and *Cinnamomum* have been identified here. In the middle and upper chalk, revealed by more numerous and richer finds, there come to light genera from nearly the whole of the existing system : Dicotyledons and Monocotyledons, Choripetales and Sympetales. We find representatives of our families and genera, in certain cases also undoubtedly our species, although the identification of these last is naturally more difficult. This flora presents no primitive forerunners of our Angiosperms. One certainly finds in it species and genera different from those now living, but hardly any different families. The flora of the chalk is *richer*, but *not more primitive* than that now living. It is spread over the whole world. For example, its remains have been found in N. America, Greenland, Siberia, Sakhalin, Bohemia, Portugal, Madagascar, Patagonia. This flora is still living. Our flora, however, exhibits less manifoldness, less variety than the early Tertiary one. Far from a wider development, a *selective reduction* of this has occurred."

Nilsson's assertion that the flowering plant flora of the chalk was richer—more manifold—than it is to-day surprised me, therefore I determined to try to verify it. I have been able to study the fossil records only of Greenland and Sakhalin, and some of those of N. America and Europe, nevertheless I have come upon records of two of the three sub-classes into which Vines divides the Monocotyledons and of all three of the sub-classes of the Dicotyledons, and of all the six series of these classes. These fossils include members of three of the 11 orders into which Engler divides the Monocotyledons and of 21 of the 40 orders of Dicotyledons. In addition to these, there have been found a number of fossils of extinct genera ; on this account and in view of the fact that I have not examined any of the fossil records of Africa, Asia or South America, I think I can safety subscribe to Nilsson's dictum that the Angiosperm flora of the Cretaceous

was richer than it is to-day. This, of course, is not in accord with the doctrine of evolution. That fossils have been found in the records examined by me of all three sub-classes of the Dicotyledons and of only two of the three classes of Monocotyledons, and the fossils of the latter in the Cretaceous are not nearly so numerous does not seem to mean that the Monocotyledons were later in making their appearance, because, in fact, the fossils indicate that the two groups made their first appearance *contemporaneously*. The first known Monocotyledons come from the Lower Cretaceous, viz., the Gault of Portugal (Valanginien) and North America (Upper Potamac Beds). The fact that the Monocotyledons make their first appearance simultaneously in areas very far apart is not unusual. I think I may say that in the majority of the great animal and plant groups this is the case. The Monocotyledons seem to be less often fossilized than are the Dicotyledons. In an enquiry I am making (as yet far from complete) into the extent to which living genera of flowering plants have been fossilized, I have so far found records of fossils of 152 of the 411 living genera of British Dicotyledons or 37·2 per cent., and of 12 of the Monocotyledons, or 10·1 per cent. The difference is in part accounted for by the fact that the grasses, which are not readily fossilized, constitute a considerable percentage of the Monocotyledons.

Author's Reply.

(1) In regard to my use of the term "Jesuitical duplicity," I am amazed that any exception is taken to so candid a statement of fact. One would wonder whether my critic is familiar with the history of Science. I recall, for example, the case of Professor Ernst Haeckel and the judicial findings of the University of Jena. In my own experience, the policy mentioned was (and probably still is) taught in American universities. At one of our principal graduate schools, which I attended, our professor (a leader in biological science) told us that we should abide by the dicta of Science; but, said he, if any of our postulates be proved untrue, then you must say: "Ah, yes, we knew all along that it was so!" If that is not "Jesuitical duplicity," I know not the meaning of the term.

Moreover, it is not an evidence of an unchristian harshness to be properly descriptive. Our Lord spoke of the Scribes and Pharisees as "hypocrites" (Matt. xxiii, 13 *et seq.*), and termed Herod "that fox" (Luke xiii, 32). History has called a world-conqueror of old, Alexander the Great; but God called Alexander "a rough goat" (Dan. viii, 21). Is God harsh? Is Christ lacking in meekness? Recall that the Lord Christ was like unto Moses (Deut. xviii, 15), and Moses was "very meek, above all the men which were upon the face of the earth" (Num. xii, 3). On the contrary, to fail to speak in true terms makes us as guilty of telling a lie as though we had deliberately spoken a falsehood.

(2) My mention of the "Plan of Creation" was the only reference I could make to the views prevailing in the first half of the 19th century. It was to be assumed that the suggestion would recall to the reader all those arguments dear to Professor Agassiz, and set forth in his books. It scarcely requires pointing out that my paper is marked by extreme compression, and to elucidate the subject in an adequate manner would require several volumes. It should also be emphasized that the concept of "progression" is no evolutionist property but was made clear long before the advent of Charles Darwin. No one can doubt that plants and animals show gradation of development, as we examine their respective kingdoms; and this "stair-like progression," as Sir Thomas Browne (1605–1682) described it, must be explained if we are to concern ourselves with Biology. I know of only two explanations that have ever been advanced (at least by those who admit causation): (*a*) that the progression is the result of Evolution; or (*b*) that the progression evidences the plan used by the Divine Creative Mind.

(3) My critic raises the question of linkage of the plant and animal kingdoms, and status of micro-organisms. The distinction between plant and animal activities is admitted as real, I think, quite universally. The point I try to make is that there are always discrete sorts of organisms that function as either plant or animal. No question is raised in regard to this point except in the case of micro-organisms, which are either little studied (in the case of some groups), or are extremely difficult of study in the case of others. Mr. Dewar has already covered this point.

(4) I do not see why any question should be raised as to the necessity for revision of Lyellian Geology. When we recall that Sir Charles Lyell did his work a century ago with limited materials and obsolete methods, it is not surprising that geologists have found difficulty in applying his chronology to rock strata other than those he studied. In point of fact, I understand that in instances it cannot be done. It is almost an axiom in science that scientific "truth" needs constant reworking and restating, and Geology is no exception. But it requires equally emphatic statement that no tyro or pottering amateur can accomplish what only a man of genius, well trained in geological science, could accomplish in a life-time. As a botanist, I decline to be drawn into this work of revision; but I do not see why I should not point out the need of revision, and refuse to accept "Historical Geology" until it is brought into line with modern Science.

(5) Mr. Dewar has replied neatly to the question regarding Angiosperms. The extreme isolation of the Angiosperms is so well known and is so frankly admitted by evolutionists that an answer was hardly required.

(6) Mr. Leslie's last question evidences what I suspect will be a general reaction to my paper; namely, that it involves too technical a subject for most readers to follow. I should explain that the evolutionist hypothesis teaches that all plants and all animals came from preceding organisms by a natural process of transformation of one sort (usually termed a "species") into another sort of organism. Transformation of plants and animals is so basically inherent in the concept of evolution that evolutionism is often called "transformism." Do we comprehend all that is involved in this statement? It means that every man, woman and child in the world is descended from a transformed animal. It means that every kind of animal in the world has been transformed from some other kind of animal, which originally was transformed from a plant or from some ancestor of plants. And naturally, by the same concept, all plants are transformed other-sort plants. Moreover, as transformation can never stop acting (according to evolutionism), we should see transformation occurring under our eyes everywhere, all the time. Rabbits should be littering new

kinds of rabbits (or who knows what ?), Hydras should be budding new kinds of Hydras, Magnolias should be growing into buttercups. And amongst fossils, we should find " connecting links " everywhere in abundance ; because, remember, evolution is a universal, world-wide phenomenon, always going on and responsible for the being of every man, animal and plant in existence. It is totally impossible of every man, animal and plant in existence. It is totally impossible that there could ever be so much as even one DIScontinuity in the whole world, if evolution be true.

Hence the importance of my subject. It is not a question of whether simpler forms appeared first, or more complex ones ; or whether or not we may emphasize discontinuities. The great, overwhelming, stupendously important fact is that *there are NO TRANSFORMATIONS, there are NO CONTINUITIES* to be found anywhere in the biological kingdoms. I examine the plant record and show from plain, everyday botanical class-room knowledge that there are no continuities to be found amongst living plants, nor among fossil plants, nor even among the entities dear to the Geneticist. Discontinuities are ENTIRELY the rule amongst plants.

Where then does Evolution come in ? Evolution cannot exist for a moment without demonstrable transformations. But botanical phenomena provide us with no transformations—not even one. What shall we do ? Shall we destroy all living plants and smash every plant fossil that can be found, in order to live comfortably with Evolution ? Or shall we submit to fact and give up the antiquated philosophy of Evolution which some 19th century atheists dug out of barefoot Greek antiquity ?