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ORDINARY MEETING, MARCH 16, 1885.

D. HOWARD, ESQ., F.I.C., IN THE CHAIR.

The Minutes of the last Meeting were read and confirmed, and the following Elections were announced :—

ASSOCIATES :—Rev. C. B. Bowles, Tunbridge Wells ; Rev. S. Bowers, A.M., Ph.D., United States ; Rev. J. C. Caldwell, D.D., United States ; C. A. Barclay, Esq., F.R.G.S, Folkestone ; W. Lester, Esq., J.P., F.G.S., F.C.S., Wrexham ; J. Spriggs, Esq., F.S.S., Market Harborough.

HON. LOCAL SEC.—W. Lester, Esq., J.P., F.G.S., F.C.S., Wrexham.

ON THE RELATION OF FOSSIL BOTANY TO THEORIES OF EVOLUTION. By W. P. JAMES, Esq., F.L.S.

1. WHEREVER the word Evolution comes in, it is well to begin with stating in what sense it is used. For the present purpose it will be limited to its proper biological meaning, for it is only in the province of life that it can be considered as anything more than a hazy synonym for development. What process it can possibly express in the inorganic world I am at a loss to conceive. But, as understood by Zoologists and Botanists, it is perfectly intelligible ; to them it is equivalent to the Theory of Descent,—that is, to the hypothesis that the forms of animal and vegetable life which surround us have descended by modification from their predecessors in time. In itself this is a most interesting and fascinating question, and no thoughtful student of nature can

dream of answering it off-hand. It may be partially true or not, but the evidence at present available cannot be considered as warranting a verdict that will satisfy everybody. The solution, if solution there be, must lie in the fossil-bearing strata. If the record of those strata be accepted as hopelessly imperfect, it seems almost useless even to discuss a problem for which sufficient data are wanting. But it may be questioned whether the geological record can fairly be considered as uniformly imperfect,—at any rate, to such an extent as to preclude any inferences for or against Evolution. It is from this point of view that I propose briefly to set before the Institute the facts of Fossil Botany in their bearing upon the Theory of Descent.

2. *Divisions of the Vegetable Kingdom.*—But before entering upon the subject it will be useful briefly to indicate the principles upon which the larger groups or sub-kingdoms of the vegetable world are constituted. It would be rash to take for granted any general acquaintance with the subject, as Botany has always had less attraction for the outside public than her zoological sister; and this assertion may be extended to Fossil Botany. The extinct races of plants have no surprises for the untrained eye so great as the monstrous Icthyosaur or the weird Pterodactyl, no series of forms so splendid as the long array of Ammonites and Encrinites. Some acquaintance with insignificant plants still living is required before the mind grasps the meaning of Club-mosses and Horse-tails, which reached the stature of forest trees, or understands that in their way they are as surprising as the giant Sloth or the Mastodon.

Plants are divided, in the first place, into two vast series, those with and those without flowers,—Phanerogams and Cryptogams. Old and obvious as is this distinction, it is eminently natural. Not only does it still hold good, but is, if possible, only brought out into stronger relief by our increase of knowledge. A wide gulf still yawns between the seed-bearing Phanerogam and the spore-producing Cryptogam. The assertion that it is at all affected by modern research is at variance with obvious facts. True seeds, containing an embryo plant with rudimentary axis and appendages, are strictly confined to Phanerogams, and are exclusively the result of the fertilisation of ovules by pollen-grains through the immediate agency of the air. On the other hand, fertilisation, properly so called, in Cryptogams invariably demands the presence of water, and never results in a seed. Again, the asexual spore so frequent in Cryptogams is totally absent from Phanerogams; in the fern, for instance, it is the antherozoids of the prothallus

and not the spores of the mature plant which correspond to pollen-grains. Even in the *Selaginella*, which has sexual differentiation in its microspores and macrospores, the microspores give origin still to true antherozoids requiring the intervention of water. Apart, then, from the valid mark involved in the distinction between Flowering and Flowerless plants, Phanerogams and Cryptogams may also be accurately described as air-fertilised and water-fertilised, in doing which we indicate a gap which no theory can bridge over. But, when we have thus got our first great division of Cryptogams, we do not know what to do with it. It is, in fact, an unmanageable aggregate of groups separated from each other by such tremendous intervals as, for instance, that between the Diatom and the Tree-fern. The botanist is obliged to treat it as the zoologist has treated the cognate term Invertebrate, that is, to break it up into more natural series. It is a mere question of names whether these should be called sub-kingdoms or not. As to their independent value and wide divergence there is no difference of opinion. Provisionally we may establish three of these sub-kingdoms, the *Thallophytes*, *Muscineæ*, and *Pteridophytes*, or, speaking roughly, the Algal type, the Moss type, and the Fern type. First comes the *Thallophytes*, including the *Algæ*, *Fungi*, and *Lichens*, the *Characeæ* being considered as *Algæ* in deference to the preponderance of authority.

Perhaps no other division of plants includes such vast diversity in form, size, and mode of re-production. It links the minims of the vegetable world, the Diatoms, Micro-fungi, and *Oscillatoriaceæ*, with the huge kelp of the Pacific Ocean, one of the longest stems in the present epoch. But they all agree in consisting of cellular tissue to the exclusion of fibro-vascular bundles, in the absence, more or less complete, of a differentiation into root, stem, and leaf, and in the great complexity, with few exceptions, of their reproductive processes.

Those not acquainted with natural science and more familiar with mathematical methods may consider this a very vague definition. But this difficulty is inherent in the subject. Nature, or rather living nature, abbors hard-and-fast lines. She refuses to run into our moulds, and shuts her eyes to our neat systems of classification. With reference to plants in general, there is scarcely a single statement which can be affirmed of them all without exception. We can say little more of them collectively than that they live and grow. For the fungi prevent us from predicating of *all* plants that they feed upon inorganic materials, that they contain starch, that

they break up the carbon dioxide of the atmosphere by means of chlorophyll-bearing cells, and so on.

Instead of vainly striving to cramp nature in the bonds of logic let us recognise this excessive elasticity of living forms. The late Professor Harvey has made such excellent remarks on this subject in the introduction to a book, now become rare, *Manual of the British Marine Algæ* (1849. Van Voorst), that I shall take the liberty of quoting them :—

"Whoever has paid the slightest attention to the classification of natural objects, whether plants or animals, must be aware that, if we desire to follow natural principles in forming our groups,—that is, to bring together such species as resemble each other in habits, properties, and structure,—it is a vain task to attempt to define, with absolute strictness, the classes into which we are forced to combine them. At least, no effort to effect this desirable object has yet been successful . . . . But it fortunately happens that these difficulties are much more formidable on paper than in the field. . . . . The search into structure and affinities among the works of creation is something like that after first principles. We can distinguish and analyse up to a certain point; there we are stopped by that invisible and intangible, but impassable veil, behind which the Creator hides his operations. At this point we must rest satisfied with differences which we can *see*, but which we cannot *know* or *define*" (pp. ix. and x. of Introduction).

The second great group of Cryptogams is the Moss alliance. Tiny as are most of its members, they generally possess a distinct stem and leaves, and are invariably separated from Thallophytes by what is known as an alternation of generations, that is, by the occurrence of one form of the plant producing *antheridia* and *archegonia*, and of a second form arising as a peculiar result of the fertilised *archegonium*, the spore-capsule, familiar to us in *Bryaceæ* as the elegant Urn-fruit. Morphologically, this fruit is, as it were, a graft on the mother plant, and constitutes a phenomenon so isolated as to give a high value in a systematic point of view to the *Muscineæ*. Dr. Goebel, in a recent monograph on the mosses (Schenk's *Handbuch der Botanik*, vol. ii. p. 401), says :—"We must accordingly be contented with affirming that the gulf between Mosses and Pteridophytes is the deepest that we know in the vegetable kingdom, and it is not made less by being bridged over by hypotheses and surmises."

The third great group, the Pteridophytes or Fern type, is of immense importance from its prominence in geological history. It is best divided into three classes, formed

respectively by the Ferns and their allies, Club-mosses and their allies, and the isolated Horsetails, now reduced to a single genus. In this group first occurred forms of *terrestrial* vegetation, which would now be called trees. We must lay stress upon the word *terrestrial*, for no one can now tell what glorious and luxuriant algal forests may have grown in primeval seas, without leaving a trace behind them, except amorphous masses of graphite. The Pteridophytes are also known as the Vascular Cryptogams, in opposition to the two preceding groups, which may be called Cellular Cryptogams. They possess true roots and fibro-vascular bundles, and the capacity of taking on a woody structure. Dissimilar as the outward habit of a fern, a horsetail, and a club-moss may appear at first sight, they are all connected together by the character of their prothallus. This is a kind of nurse plant or preliminary stage, in which a cellular expansion arises from the germinating spore, and in time produces the *antheridia* and *archegonia*. From the fertilised *archegonium* springs the form which we call, in ordinary language, the fern or the horsetail, and this form, in its turn, gives rise exclusively to asexual spores. In the small group of Heterospores the extension and duration of the prothallus are so abbreviated that the two kinds of spores, the microspores and macrospores, approach in function very near to pollen-grains and ovules. But to the last antherozoids occur, and require water: a mark distinguishing the highest Heterospore from Phanerogams.

Advancing now to Flowering plants, we have the advantage of being able to appeal to common knowledge. Everybody has some notion of a flower and its parts. The sub-kingdom of Phanerogams is divided into two classes, of equal systematic importance, but very unequal in extent. Here, as in earlier instances, we must distinctly bear in mind that the vegetation of the present epoch is only a temporary phase of the development of plant-life. Palæontology teaches us that classes now small in extent were once more important, and it is only by taking a broad view of past as well as of present life that we understand the relative value of the higher groups. In natural as well as in political history the present has its roots in the past, and is now determining the future. It is thus with the two classes of Phanerogams, Gymnosperms and Angiosperms. If we considered only the actual state of affairs, the Gymnosperms would appear to be what they were considered in pre-geological times, a subordinate group. But, when we know that they date as far back as the Devonian beds, we see their importance in the great plan of creation. The Gymnosperms

include the Conifers, the Cycads, and *Gnetaceæ*. Their flower is a true flower, but of a very simple type: a perianth is nearly always wanting, the sexes are always separate, the floral axis is often a real shoot and sometimes even branched, and finally the ovules are not contained in an ovary. The woody stem, however, of the Conifers is of a higher type than anything we have yet met with, having annual rings of growth and a distinct bark. It is usually said to approach the dicotyledonous type; but, as it is incomparably the older, it would be more strictly correct to say that the dicotyledonous type represented by our oaks and elms is a more highly differentiated form of the gymnospermic. Lastly, we have the Angiosperms, in which the ovules are enclosed in an ovary. They are divided into Monocotyledons and Dicotyledons, and comprise all the familiar flowers, shrubs, and trees which surround us, and on which we need dwell no further.

3. *General Inference from Fossil Plants.*—The order in which we have taken these four groups is that of their respective simplicity, Thallophytes, Muscineæ, Vascular Cryptogams, Phanerogams. As far as the evidence of the rocks goes, it is also, on the whole, that of their first appearance in past time. To speak quite exactly, the remains have been found as follows:—Algæ are the earliest; Vascular Cryptogams then appear in company with Gymnosperms and a few Monocotyledons; then comes the culmination of the Gymnosperms in the Cycads; finally, the Dicotyledons emerge abruptly in the upper chalk. Fungi lichens and mosses are too soft to stand any chance of being preserved in the older rocks. So far then, as the record goes, it agrees with the natural arrangement given above. Now the Theory of Descent requires that the varied plants of the present epoch, trees, shrubs, and herbs, ferns, mosses, and seaweeds, should all alike be lineally descended from the algæ of the most remote age, and, moreover, ultimately from the simplest forms of the algæ, the *Oscillatoriaceæ*, which alone, as far as our knowledge goes, can live in hot water, and could, consequently, have flourished in the half-boiling ocean of the dim past. The rocks, accordingly, should present us with a series, more or less complete, of these supposed ancestors of existing plants. Is this the case? To this question there is only one answer. Had we to consider only the fossil plants of the rocks, so far as known, no one in his senses would have been led to such an hypothesis. It would never have suggested itself to a botanist. No transitional forms are known between Algæ and Mosses, between Mosses and Vascular Cryptogams, between Vascular Cryptogams and Phanerogams. Even if such links were found,

they would prove nothing as to their origin. The only fossil evidence that can prove that one species has been transmuted into another would be a vast number of intermediate forms *between two species,* shading off imperceptibly into one another. It is a matter of common knowledge that such a series is not yielded by the rocks. So tremendous is the force of this negative answer at first sight that it requires some very strong counter arguments to rebut it.

4. *Imperfection of the Record.*—As is well known, the evolutionist's reply is to dwell upon the undoubted imperfection of the record. He can, for instance, very fairly say that, as no mosses have been preserved before the chalk, a great series of intermediate links between algæ and mosses *may* have perished. Similar remarks apply to the lichens, fungi, and many other lowly plants. Who knows, he may say, what the lost pages of the great Stone book may have contained? Intermediate forms would naturally be humble, insignificant plants, and it is not surprising that they have not been preserved. There is something in this, and we would wish it to carry its full weight to the hearer's mind. Are we, then, to leave the question entirely open as far as fossil botany is concerned?

5. *Occasional Completeness of the Record.*—The best answer to this seems to be that, whilst admitting the general incompleteness of the fossil history of past life, we must take care not to exaggerate it. For we cannot deny that here and there, at any rate, we have isolated pages, to continue our metaphor, which are crowded with illustrations. One of these occurs in each of the three great divisions of geologic time: in the Kainozoic we have some singularly complete memorials of Miocene date; in the Mesozoic we find similar though less abundant representatives of the cretaceous land flora; and, finally, in the Palæozoic we have the confessedly rich remains of the coal measures. Surely, if it can be shown that each of these extinct floras is wonderfully illustrated in local strata, we shall be justified in drawing all the inferences we can from them. Three times the veil is withdrawn from the past, and three times we catch a glimpse of the character of the rich and beautiful vegetation then flourishing.

6. *Miocene Flora.*—In speaking of the Miocene flora it will not be necessary to repeat the information on the subject to be found in all the accessible books on geology. Every one who has read Lyell's *Principles,* or even the briefer *Student's Elements,* will remember how fascinating the subject is. It is impossible here to attempt to separate the various subdivisions of Miocene time; we must confine ourselves to the

general bearing of the whole epoch on Evolution so far as its plant-remains go. The most beautiful leaf-beds belong to the Upper Miocene, and are best seen at Oeningen, in the valley of the Rhine, between Constance and Schaffhausen. They have been explored by the late Professor Heer, whose noble work on the Tertiary Flora of Switzerland will form an imperishable monument to his name. At this spot there seems to have been a lake, probably fed by springs with water unusually charged with carbonate of lime. Along the margin of this lake a series of very fine marls were deposited, often as thinly laminated as the pages of a book. In these strata an astonishing number of leaves, fruits, and insects have been preserved. A small collection of them is to be seen at the British Museum, now in South Kensington. To give some idea of the completeness of the record for this particular epoch, it may be pointed out that of a kind of camphor-tree (*Prinos Lavateri*) distinct sprays are found with flowers, fruit, and leaves; that the well-known key-fruit of the maple abounds, together with countless leaves; that on some remains leaf-fungi can be detected just as they now are developed in autumn; and that the time of year when the deposit was made can often be inferred from the shoot being in its vernal or autumnal state, and from the ants having their wings or not. What, then, was the vegetation that surrounded this Swiss lake at a time before the Alps had undergone their last elevation? First of all, not one plant of the present Swiss flora has been found. Secondly, the vegetation was very rich in trees, and on the whole had a resemblance to that of Florida, Mexico, Australia, and Japan. The number of woody plants was very great for so small an area. About 180 are known. These include swamp cypresses, evergreen oaks, laurels, elms, maples, acacias, liquidambar, and seven kinds of palms, including one (Sabal) similar to that now growing in the valley of the Mississippi. On the surface of the lake floated water-lilies, around its margin were reeds and rushes. The ferns are precisely the same as our recent ones, only of a sub-tropical type, such as *Lygodium*, a climbing fern, and *Osmunda lignitum*. But it would be tedious to give anything like a complete list of the still-existing genera which are found in these strata. Every one who examines the remains must be forcibly struck by the extreme distinctness of the generic type; for, great as must be the interval which separates us from these successive Miocene floras, all the genera are obviously as distinct from each other then as now.

More than this, so great is the constancy of type in many cases that Professor Heer gives a list of plants in which

probably the same species have survived to our own times. He considers seventy-two plants as probably ancestral forms actually identical with those now living. The following are some of them :—

MIOCENE FORM.		MODERN FORM.
Woodwardia Rœssneria	=	W. radicans (a Madeira fern).
Aspidium Escheri	=	A. thelypteris (a marsh fern).
Isoetes Braunii	=	I. lacustris (common water-plant).
Taxodium dubium	=	T. distichum (American swamp-cypress).
Glyptostrobus europæus	=	G. heterophyllus (Chinese cypress).
Sequoia Langsdorfii	=	S. sempervirens (redwood).
Sparganium valdense	=	S. ramosum (common water-plant).
Liquidambar europæum	=	L. styracifluum (American shrub).
Populus mutabilis	=	P. euphratica (Asiatic poplar).
——— balsamoides	=	P. balsamifera } American poplars.
——— latior	=	P. monilifera }
Salix varians	=	S. fragilis (common crack willow).
Ulmus Braunii	=	U. ciliata (elm).
Planera ungeri	=	P. Richardi (tree allied to the elms).
Platanus aceroides	=	P. occidentalis (plane-tree).
Laurus princeps	=	L. canariensis (laurel of Canary Islands).
Hakea salicina	=	H. saligna (Australian proteacean tree).
Diospyros brachysepala	=	D. lotus (kind of ebony-tree).

Besides these his list includes also the direct ancestors of three species of maples, of the tulip-tree, and so on. This extraordinary permanence of generic, and possibly even of specific type, is strongly opposed to any theory of variation. If genera, and possibly species, have changed so little in so vast a time, there really is no room for the slow and secular transformation required by the Theory of Descent. Let no one under-rate the value of this kind of evidence founded on leaves and flowers. The microscope is now able to decide points of affinity in plants to an extent never dreamed of in the earlier days of palæontology. The cells of the epidermis, with their shape and arrangement, and the stomates which pierce it with their characteristic forms, are often sufficiently preserved in Miocene leaves to indicate the order, if not the genus, of a mere fragment.

But there is another point of view from which the persistence of these genera is very striking. They have outlived a most remarkable change in the climate of Spitzbergen and Greenland. Genera of plants are still living in the warm temperate zone which once flourished within the present Arctic circle. This is well known as one of the greatest puzzles in geology; but I am not now concerned with its solution. I am only pointing out that beeches, oaks, planes, poplars, and so on, are older than that extraordinary condition of our planet which allowed a vigorous growth of

trees to take place within 12° of the pole. All the Arctic Miocene plants agree entirely with those of the Miocene beds of Central Europe. But this even is not all. Many of the genera found in the Miocene flora go further back still. They meet us in the chalk, the earliest flora of Dicotyledons. Dr. Lesquereux gives, in the *Cretaceous Flora of the Western Territories* (vol. vi. of U. S. Geological Survey, 1874), amongst others the following genera of trees as then existing: the alder, the birch, the oak, the laurel, the magnolia, the plane-tree, the willow, the sassafras, the sequoia, the tulip-tree. With pardonable pride the eminent American palæó-botanist remarks upon the great antiquity of the indigenous glories of the American woods, the magnolia and the tulip-tree. He justly remarks,—"The magnolia, and its relative, the tulip-tree, are wonders of American nature quite as worthy admiration as the great Niagara or the mammoth trees of California" (*Tertiary Flora*, vol. vii. p. 247). But, after describing fragments of tulip-tree leaves from the cretaceous beds, he makes the following most valuable remarks (*Cretaceous Flora*, vol. vi. p. 124):—"Liriodendron, the tulip-tree, has in its characters, its distribution, and its life a great degree of affinity with magnolia. The American species is the only one known now in the vegetable world, and its habitat is strictly limited to this country. It does not ascend higher than the fortieth degree of latitude, except, perhaps, casually, like magnolia, under the protection of favourable local circumstances. The genus does not appear to have any disposition to modifications of its type, and to migrations. We have as yet scarcely any fossil remains of it in our Tertiary formations. In that of Europe, it is represented from Greenland to Italy by one species only. The leaves of different forms, described from the Dakota group as four species, may perhaps be referable to a single one, as the characters, especially the size, of the leaves may be local, and result from climatic circumstances. It has thus passed a solitary life. Even now, by the singular and exclusive form of its pale-green glossy leaves (*i.e.*, four-lobed and looking as if the fifth apical lobe had been cut off, apparently a unique outline); by its large cup-shaped yellow flowers, from which it has received its specific name; by its smooth, exactly cylindrical stem, gracefully bearing an oblong pyramidal head of branches, grouped with perfect symmetry, it stands widely apart from the other denizens of our forests as a beautiful stranger, or rather as a memorial monument of another vegetable world. Either considered in its whole or in its separate characters, the tulip-tree is a universal and constant subject of admiration and wonder. It could be named,—not the king, it is not strong

enough for that,—but the queen of our forests, if the magnolia was not there with it to dispute the prize of perfection by the still grander majesty of its stature, the larger size of its foliage, the elegance and the perfume of its flowers. Our sense of admiration for these noble trees is heightened still by the dignity of their ancient origin."

Now we have heard a great deal lately about the variability of species. Whole books have been written to prove the very obvious proposition that plants and animals if placed under artificial conditions are likely to vary in an artificial manner. We have had enough of this one-sided collection of facts favourable to certain hypotheses. It is time also to say something about the permanence of type to be found in nature. That there is something stable and fixed amidst all the variation of living things is absolutely certain. To pass over species, it is undoubtedly true that many genera are extraordinarily stable, as we have seen to be the case with the maple, the oak, the tulip-tree, and so on, persisting from the chalk. But an illustration from the floras of distant lands in the present day will, perhaps, help us in another way to realise the astonishing constancy of some generic types. Suppose we take ship and get away as far as ever we can from our own island, we shall find ourselves at last amid the waste waters of the vast Pacific Ocean. Among these stormy waves rise almost at our antipodes the small islands known as Lord Auckland's group and Campbell's Island, visited by the present Sir J. Dalton Hooker during the Antarctic expedition of the *Erebus* and *Terror* under Sir James Ross, which lasted from 1839 to 1843. Lord Auckland's group lies in 50° 30′ S. lat. and 160 E. long.; Campbell's Island in 52° 30′ S. lat. and 169 E. long. If we consult the magnificent Flora antarctica, and gaze at the beautiful coloured portraits of the plants executed by the skilful hand of Mr. Fitch, we shall almost imagine ourselves landing upon these steep and desolate islands, formed of volcanic rock, "ever lashed by heavy swells and exposed to a succession of westerly gales." Still, in spite of rain, and snow, and fog, these lonely spots produce a flora rich in beautiful plants, a fact attributed by Sir J. D. Hooker to the comparative mildness and uniformity of their oceanic climate. However, what we wish at present to call attention to is the constancy of generic type. Any novice in botany whilst exploring these lands would be able to name off-hand plant after plant as belonging to genera familiar to him in Great Britain. Nor would this be true only of these islets, but also of all the other fragments of Antarctic land, such as Kerguelen's Land, Falkland Islands, and so on.

We do not say that *most* of the genera are European, but in each island there are *some* genera identical with those of Europe. Our imaginary traveller would at once know the species of *Ranunculus* from their leaves, flowers, fruit, and general habit. He would find *Cardamine hirsuta*, var. *subcarnosa*, only differing from our common hairy Bitter Cress, to be found on any old walls, by its very fleshy leaves. He would see a *Geranium* (*microphyllum*), extremely like our *G. lucidum*, two or three kinds of *Epilobium* or willow-herb, two lovely kinds of scorpion-grass (*Myosotis*), all of which would be familiar to him in a moment as new forms of well-known types.

It does not require a botanist to detect them: any sharp, country-bred lad would say in a moment, "This is a buttercup, that is codlins-and-cream" (the provincial name of *Epilobium*), and so on. It would be wearisome to go through all the European genera that thus reappear in Antarctic lands. I will briefly add two barberries, a ragwort, a cudweed, our own dandelion identical in species, lovely gentians, a butterwort in the Falkland Islands scarcely to be distinguished from *Pinguicula lusitanica*, our own pale butterwort, a great many grasses, some ferns, very many mosses, fungi, and algæ.

This is merely introduced as a single instance of a phenomenon that must be taken into account, the extension of many genera through widely-separated areas, and their astonishing constancy to their type. Let this fact be remembered as well as those of the variability of species. We have, in reality, two series of facts in living nature, some pointing to change and some to persistence, and our task is to reconcile them. It is certainly singular that often where the species are most unsatisfactory, as in the willows, the genus is, on the contrary, eminently natural; and, as we know in this case, it is also a very ancient one, descended from the chalk. Again, where the genera are intricate, the order is wonderfully natural, as in the *Umbelliferæ* and *Compositæ*. However, enough, perhaps, has been said about this subject, and we will proceed to the Chalk flora.

7. *Cretaceous Flora.* — We have already spoken of the antiquity of the genera of dicotyledonous trees which first occur in these beds. We will now confine ourselves to one single point,—their abrupt appearance. It is generally admitted that, as far as our knowledge goes, the Dicotyledons emerge suddenly in the upper chalk, without any previous hint of them in the preceding Jurassic beds, which were especially rich in cycads and ferns, and they occur, moreover, as representatives of the three great divisions,—*Apetalæ*,

Monopetalæ, and *Polypetalæ*. The first that we can find are, to use Dr. Carruthers' words, "not generalised types, but differentiated forms, which, during the intervening epochs, have not developed even into higher generic groups."

To take, for instance, the Dakota group in North America, among its 130 species, as yet known, only one may be doubtfully referred to the Cycads; there are only five Cryptogams, six Conifers, and two Monocotyledons; all the rest are Dicotyledons, distributed into genera, much as now; of *Apetalæ* it has *Amentaceæ, Myricaceæ, Platanaceæ, Salicineæ;* of *Gamopetalæ, Bicornes, Ebenaceæ*, &c.; of *Polypetalæ, Magnoliaceæ, Sapindaceæ, Menispermaceæ*, &c. As Dr. Lesquereux says (*Cretaceous Flora*, p. 38),—"it has representatives of all the classes of plants, without disproportion, in one degree or the other, as compared to what is considered the scale of the vegetable kingdom. This seems to prove a collateral development of different primitive types, and, therefore, the appearance at certain epochs of those original forms which, at each geological period, have changed the character of the vegetable world, and which do not have any connexion with antecedent types." Again, still more decidedly (p. 35), after remarking that it is easy to build up imaginary systems of derivation from supposed simple types, by mere deviations or multiplications of organs, he goes on:—"But until we know more we have to consider the facts. And the conclusion evidently forced, at least in considering the flora of the Dakota group, is that its disconnexion from ancient types is so wide that even the supposition of intermediate, unknown, extinct vegetable types fails to account for the origination of its peculiar characters."

So far as the evidence of the Upper Cretaceous Dicotyledonous remains goes, it is decidedly opposed to the theory of descent. It is opposed to it in two ways. First, by the sudden emergence of the class already differentiated into subgroups it irresistibly suggests some abrupt origin of that class, such as immediate creation. Secondly, by the proof of the persistence of generic types so complicated as that of the tulip-tree from that distant period to the present day without any apparent change, it negatives any theory which is built upon the indefinite variability of systematic characters.

8. *The Flora of the Coal Measures.*—We now come to the most fascinating of all the extinct floras, that of the Palæozoic Coal Measures. The imagination is wonderfully attracted by the picture which science calls up of these old-world forests. Stretching for hundreds of miles along the swampy margins of estuaries, and covering the surface of their low deltas, they

appear to have been uniform all over the world, even as near the pole as Spitzbergen. Club-mosses and horsetails were trees in bulk and stature, though ungainly to our eyes with their angular forked branching, their spiral rows of stiff leaves, and their grotesque fructification. Mingled with these interesting though unlovely exaggerations were the beautiful lace-like fronds of tree-ferns, as well as a thick carpet of the lowlier species, and also scattered Cycads and Conifers. No birds built their nests in this monotonous jungle, no bees or butterflies lighted up a world destitute of colour and fragrance. But life was, nevertheless, abundant in these thickets, though of an unattractive kind, molluscs and myriapods, and wood-boring beetles. Now, the first thing that strikes us in examining the fossil remains of this flora is the extraordinary abundance and perfection of the impressions of ferns. Their state of preservation is often marvellous. It should be remembered also by those who only see them in cabinets that those collected are but a fraction of those noticed by the observant naturalist. Very often the shale in which they lie buried is so brittle that the collector only catches a passing glimpse of a lovely impression before the matrix crumbles to pieces as he tries to grasp it. It seems impossible, in the face of this abundance of remains, to deny that at any rate we have here a fairly complete record of local floras. So far as it goes it can be trusted. As the date of the palæozoic coal measures must in any case be very remote, they evidently supply us with a crucial test for the Theory of Descent. If that theory were true, the lines of vegetable pedigree should be at that time visibly converging. For instance, the three great classes of Vascular Cryptogams ought to be far nearer to each other then than they are now. Is this the case? Notoriously the answer is in the negative. Ferns, horsetails, and club-mosses are not only not converging, but are, if anything, further removed from each other than now. The two latter groups then reached their culminating point both in the size of individuals, the number of genera, and the complexity of structure. The Lepidodendrons and Sigillarias had a kind of woody structure feebly represented in their present herbaceous representatives. So also had the huge Calamites, Calamodendrons, and Equisetites, which have now dwindled down to a solitary genus, *Equisetum*. The peculiar spores of many of the fossil genera are found in vast abundance, and proclaim unmistakably their affinity to the modern survivals.

The ferns still flourish, but at that period they were evidently of greater relative importance than now. At present about forty species grow in the British Islands, but

130 fossil species have been found in the coal shale in the same area. Dr. Carruthers also tells us that a group of ferns has entirely passed away with a stem-structure fundamentally different from any now in existence. All these distinctions are equally prominent in the still older Devonian remains. So far back as we can trace the three great groups of Vascular Cryptogams they move in parallel and not in converging lines. The importance of this fact is so enormous that it seems to dispose of the question for ever; for there is really not time enough left before the Devonian beds to allow a primitive cryptogamic form to vary into three such strongly-marked and highly-specialised groups of descendants. Then, again, as Lyell remarks, it is astonishing how little ferns have altered since their first appearance, so that possibly even the genus *Pteris* is a survival from the carboniferous age. If they have varied so little during such an enormous period of time, why should they be supposed to have varied immensely just before the commencement of that time? And is it not a singular fact that all the remains which would support the theory of the derivation of the three groups from an older form have been lost?

The same story is told by the other vegetable remains of the coal measures: thus the Conifers are represented by the *Taxineæ*, or Yew alliance, a highly-specialised form. For the present the opponent of the Theory of Descent may take up an impregnable position behind his fortress of coal.

9. *Do Synthetic Types prove Evolution?*—Synthetic types, *i. e.*, those which are supposed to combine the characteristics of separate orders or classes, are considered by many as a proof of Evolution. Let us bring this assumption to the test of fact. I suppose the Cycads are a synthetic type. They resemble ferns in the circinate vernation of the leaves and in the sorus-like aggregation of pollen-sacs; in their dicœcious, entirely naked flowers, crowded into cones, they partly resemble Conifers and partly Equiseta. In the processes of germination they resemble the higher Vascular Cryptogams. In their general habit they are like Palms. Here, I imagine, we have what is usually called a synthetic type. Now, according to the Theory of Descent it ought to have been prior in time to the Ferns, Conifers, and Palms, the characteristics of which it combines. As a matter of fact it is later than Ferns and Conifers. We ought, according to theory, to trace a series of diverging forms starting from it. As a matter of fact, we find it an isolated group throughout all its existence. We see the first scattered indications of its coming

in the coal measures, but it is especially in the oolite and other mesozoic strata that it culminates, and then it dwindles away until the present epoch, when it still flourishes in about fifty species, distributed under seven genera. Such is the life history of a synthetic type, and it is no wonder that evolutionists say very little about it.

10. *Conclusion.*—No fossil botanist had a profounder knowledge of the vast Tertiary flora than Dr. Heer of Zurich. On such a subject as this I cannot close my paper better than with his striking remarks at the end of his fascinating book *On the Primeval World of Switzerland*:—

"The deeper we penetrate into the knowledge of nature the more thorough becomes our conviction that only the belief in an Almighty and All-wise Creator, who has made heaven and earth after an eternally-predetermined plan, can solve the riddle of nature as well as those of human life."

NOTE.—The author must state his obligations throughout the paper to Mr. Carruthers' Presidential Address to the Geologists' Association, as reported in the *Geological Magazine*, 1876, p. 560.

Count Saporta's attempt to weaken the argument from the carboniferous flora is hardly successful; indeed, his chapter on Evolution in his interesting book on Fossil Plants is too obviously a *rechauffé* of an article in the *Revue des Deux Mondes*, and hardly does justice to the scientific eminence of that patient investigator of the Aix Cretaceous Flora.

THE CHAIRMAN (D. HOWARD, Esq., F.I.C.).—We have to thank Mr. James for his most interesting paper, which is well worthy of our careful attention. (Applause.) It would appear, from the course of his varied remarks, that in dealing with the whole question of evolution it is, first of all, necessary that we should make up our minds as to what we mean by "evolution." If we simply mean that there is in nature a plan of development, we must, I think, accept that as a self-evident truth. In point of fact, the word "evolution" is often used with the same vagueness that is characteristic of the way in which we employ the word "affinity" in chemistry in order to express the tendency to combine, which is evidenced by two substances that are related as little as possible to

each other. In a similar way, the term "development" is frequently used to express almost anything in the world except that which, grammatically or logically, is meant by that word. But when we come to consider the question of development, with reference to what is commonly assumed to be the case—namely, that the differentiations of nature have taken place by a slow and gradual process continually going on at approximately the same rate, investigations such as those which have been conducted by Mr. James become invaluable as arguments for or against the evolution theory. The whole study of botany is most fascinating, and one cannot but wish that Mr. James had had time to have worked out some of the points he has touched upon more fully than he has been able to do within the limits of this paper. I may just allude to one feature which to me is very striking in the study of botany, and that is, the amazing development of structure evidenced in some of the elementary forms of plant life. A good many people know a mushroom when they see it; but how many are there who know anything of the life-history of that plant? Its apparently simple structure and spontaneous growth are familiar to all of us; but how many have the least knowledge of the elaboration of structure or the extraordinary complexity of the stages of development through which it goes? In a vague kind of way, we know something about a certain object which goes by the name of mushroom spawn; but very few of us know anything of the real bearings of that spawn on the developed plant, or of the different phases through which it has had to pass. And, if this be true in regard to so simple a form of plant life, with how much greater force does it apply to the more elaborate forms? I may say, also, that the fact which Mr. James has pointed out, that the extremely complex processes of reproduction which are noticed in plant life at the present day are to be found presenting exactly the same characteristics in the earliest forms of the great divisions of the natural orders of plants, as shown in the very earliest appearances they evidence in the record of the rocks, is one which it behoves those who believe in the theory of regular evolution to explain, before they call upon us to assume that that theory is proved. (Applause.) Here, in the plant world, we have not merely the great divisions of nature just as widely separated in the earliest appearances found in fossil remains as they are at the present time, and with no intermediate links, but we find special genera, just as distinct from the other genera as their descendants or present representatives are from the different genera which are nowadays found on the earth. For instance, we cannot for a moment doubt, when we regard the first appearance afforded us of the tulip-tree, that in it we recognise the same tulip-tree as now exists, just as we also recognise in the stupendous lizards of the past the same type of lizards we see now. No one doubts that the creatures whose fossil remains we find were lizards. Even the uncultivated countryman, or those not so learned as the countryman in objects of natural history, would recognise the essential characteristics of the early tulip-tree. Do any of us who grow roses know how impossible it is to classify roses? In this case we have a singularly plastic genus,

capable of cultivation into almost infinite varieties, and yet the result is always a rose. We never find a rose developing into anything other than a rose, and yet, within the limits of variation, the variety is almost infinite. If there were no strict lines within which nature is confined, why should not all species of plants be simply varieties of one original, such as we see in the case of the rose? and why should there not be intermediate links which are now absent? It is only by the familiar study of plants that we are able to appreciate the force of this argument; but the argument, in its main outlines, appears to me to be one which any person who knows anything of nature may readily follow, and one also which it would be well to pursue, not merely to the extent to which this paper carries it, but even further, in order that we may be the better able to understand the marvels of creation; for it is evident that nothing but a creative power could have caused the differentiations we see around us. If it be said of evolution that it has taken place very rapidly at one period, and very slowly at another,—that, in point of fact, it has proceeded by fits and starts,—we may very fairly exclaim, That is quite another matter; and here I would broadly say that, if this is what is meant, then we may assert that evolution is simply claimed as a form of creation which as much requires the exercise of a creative power as any other form of creation. It is impossible for us to consider in what forms creative energy can be exhibited, or to limit its possibilities; but such an evolution as this undoubtedly demands a creative energy just as much as is needed by any form of belief in creative power. In saying this, I must not be supposed to deny that, even if the gradual process of evolution were proved, it would just as much require creative energy to account for it as is needed by any other form of creative power. The result is that, do what they will, the evolutionists are utterly unable to escape from the necessity of a Creator; and, therefore, the question is not a vital one for the theist. I will conclude by saying that, in the interests of truth and sound knowledge, papers like this are invaluable as a means of bringing to book those modern theories which are very popularly expounded, but which it is found very difficult accurately to prove. (Applause.)

Captain FRANCIS PETRIE, F.G.S. (Hon. Secretary).—Before this discussion commences, I have to read two letters, their writers being unable to be present; the first is from Sir Richard Owen, K.C.B., F.R.S.

"Sheen Lodge, Richmond Park, East Sheen, *March* 14, 1885.

"DEAR SIR,—I have the honour to return my best respects and thanks to the Council of the Victoria Institute, and regret that my present state of health forbids me to quit the house.

"The 'Unrevised Proof,' which I now return, has enabled me to pass a most interesting and instructive hour with the accomplished author of the 'Relations of Fossil Botany to the Theories of Evolution.'

"I much regret that I cannot listen to the Paper and to the Discussion it will occasion. I shall deem it a favour to have a copy, when issued.—Believe me, faithfully yours, RICHARD OWEN.

"Captain Francis Petrie."

The second communication is from J. Braxton Hicks, Esq., M.D., F.R.S., who would have been present but for a severe cold. He says :—

"The lines followed by the author of this paper seem to be excellent, and with the introductory remarks I quite agree. The great question of evolution is not yet settled ; far from it ; probably it never will be absolutely proved : at any rate, until it is so, opinions on it can only be formed on probabilities ; and the relative value of these can only be arrived at by examining facts bearing on the question, with the thoroughness and patience shown by the author of the *Origin of Species*. Till this is accomplished,—and it is a great work,—and till every point on either side, be carefully balanced, it will be considered that his conclusions have not been answered. The argument based on the imperfection of the geological records obviously cuts both ways ; like as it enables the evolutionist to escape from the demand for demonstration of the transitional forms, so it also enables his opponent to claim that the absence of any ancestor identical with existing species is no proof of its never having existed. And here the argument of Mr. James comes fairly in, and shows that, where the records of the past are copiously revealed, there is a persistence of species and genera, remarkable on the theory that a constant slow change is always occurring.—Most of those who have advocated the theory of evolution, have, so it appears to me, jumped to conclusions not warranted by the evidence; and then, having treated possibilities as proved facts, have overlooked what can be said on the other side, being carried away by the enthusiasm engendered by the apparent squaring of the theory with the facts observed. y this and kindred actions a hasty and spurious philosophy has taken the place of the former painstaking inquiry after knowledge ; and thus true philosophy is discredited. Had all the work on this subject been brought forward as "contributions," and not as final conclusions, we should have advanced sooner towards the solution of the question. To state, as some have done, that the subject is settled, and that all who dissent are the reverse of acute, shows an inadequate conception of the difficult problem before us."

Mr. W. Carruthers, F.R.S.—I have to express the pleasure with which I first read and have just listened to Mr. James's paper, in which I think he has very clearly stated the case he desires to establish. I have but little to offer in the shape of criticism, and still less by way of supplement. I accept, to a great extent, what Mr. James has put before us as a concise statement of the evidence to be derived from plants in relation to theories of evolution. There are, perhaps, one or two slips which I might correct, but they are not of more importance than typographical errors, and are, at the most, very slight. I think he has done well to insist on the permanence of generic, and, perhaps, even of specific types ; because this is what really lies at the root of the whole question. I have traced some species as far back as the glacial period—species that are now living on this globe, but which belong not only to highly-organised plants, but to the lower cellular plants, and about which there cannot be the slightest doubt. This, of course, demands a very long time indeed for the development—if they were developed—of the existent species ; but when we go back, as Mr. James has taken us, to the origin of the various types of plant life, and see that the dicotyledonous plants made their appearance, as far as we know—and, of course, we cannot

argue beyond the extent of our knowledge—in the upper cretaceous beds, that they then suddenly presented themselves in a large number of forms representing all the main sections of this division of the vegetable kingdom, and that their remains can all be referred to existing generic types, it seems to me to be utterly impossible that any explanation can be given that can bear out the theory of evolution by genetic descent. This remark is, I think, equally true with regard to the lower divisions. I think Mr. James has put the position he has taken very clearly in regard to the vascular cryptogams in the coal measures. That those three forms, so widely separated from each other, even in those early times, should have continued to exist and to maintain their differences of character down to the present time, is, I think, a fact which is strongly opposed to the evolution theory. I am, however, only expressing my general belief in the strength of Mr. James's arguments. I might, perhaps, object to the point he makes as to the synthetic types. For my own part, I am not acquainted with a single synthetic type in the vegetable kingdom. I do not know any plant that has been discovered in the rocks of the earth containing a synthetic structure including the characters of several groups of plants, now differentiated; and I am sure that this is not the case with the cycads, which, while they have an anomalous appearance in relation to their allies, are a distinctly-separate type of gymnosperms, with no affinity to the ferns on the one hand, or to the palms on the other. They began life as a group in the secondary strata, and fossils which have been referred by early observers to this group of plants have been shown to be not stems of cycads but of vascular cryptogams. They appeared to form a large portion of the flora of the Secondary period, and there were some types which have disappeared entirely and are not found at the present day. I would only, before sitting down, express my gratification at the clear way in which Mr. James has put the question before this Institute, and my conviction that all the data we have in connexion with fossil botany appear to me clearly to disprove, and certainly in no way whatever to support, the hypothesis of evolution by genetic descent. (Applause.)

Mr. C. Hastings Dent, C.E., F.L.S.—I think that papers like the present are especially valuable as bringing forward some of the weak points of the theory of evolution. Although I have not done more than look into fossil botany, it is very closely allied with zoological studies, which have always had great interest for me. There is one point to which I should like to refer, namely, the sudden appearance of groups of families in the geological strata, which appear to form a powerful argument against the doctrine of evolution. It is, I think, particularly noteworthy when we find the representatives of the same genera existing in a similar condition at the present day. Professor Williamson stated in *Nature*, in the winter of 1881-82, that he thought it doubtful whether it was possible to make clear the process by which the evolution of phanerogams from cryptogams has been accomplished. Darwin, perhaps, would give two general types—one for phanerogams and

one for cryptogams. Here we have two types separated by a vast amount of time—two separate creations; and it may be asked, if there are two, why not a hundred? I would say a word as to the persistence of type, and another with regard to the persistence of species. In reference to the persistence of type, there is the small equisetum (*E. sylvaticum*) occurring contiguous to or in the soil overlying the coal measures, and is found only in such localities, flowering in June and July. It is plentiful in the neighbourhood of Manchester, where it may be found growing in the cloughs and valleys of the coal district. Then, as to the persistence of species, there is the *Salix herbacea*, which I first found on the summit of Snowdon, and afterwards, on a visit to the Lake District, upon the tops of Mount Skiddaw and Scawfell Pike, though I failed to find it on Helvellyn. All these mountains are something over three thousand feet in height. It occurs to me that this plant is a survival from the glacial epoch, and that, as the temperature of the British Isles has increased, this little willow, which is the smallest known species, and only attains a height of two or three inches, gradually found its way from the increasingly warm low ground until it is now isolated on the tops of the highest peaks. A reference is made towards the end of section 6 of the paper to the Falkland Islands, which is specially interesting, as it is very likely a similar case to that which I have noticed with regard to the *Salix herbacea*, the ranunculus form being found in the Falklands, whereas in the Brazils no species of that genus have been discovered; and I may mention that, owing to the enormous preponderance of water in the southern hemisphere, in the temperature of the latitudes there, 40 degrees south represent 50 degrees north. There is one question I should like to ask Mr. James; and that is, what is his opinion as to the dispersion of plants, which he has not dealt with in this paper?—I know it is a very difficult subject to enter upon, but it is one which might have given rise to some interesting remarks by way of debate; whereas I venture to think that no one in this room could find a single subject of debate in this paper.—I should like to know his opinion on this matter, especially as I am not prepared to hold so dogmatic a belief on the question of dispersion from a single centre in regard to plant life as is the case in reference to the dispersion of the human race. Darwin says that the same forms could not be produced—or very probably would not be—by evolution from two different plants; consequently I should like to know how Mr. James would presume the ranunculus appeared both in the Falkland and in the British Islands? In conclusion, I may be allowed to add a few words to the quotation given by Mr. James from the book written by Dr. Heer, of Zurich:—"Let us still erect statues to men who have been useful to their fellow-creatures and have distinguished themselves by their genius, but let us not forget what we owe to Him who has placed marvels in each grain of sand, a world in every drop of water."

Mr. S. R. Pattison, F.G.S.—I am very glad that no occasion is offered for anything in the shape of criticism on this paper, the only ground for

which would have been some omission of fact, or some slip in the reasoning of the author. I do not think that anything of this kind can be charged against the admirable essay to which we have listened, and I am pleased to find that the testimony of our great leader on this subject, Mr. Carruthers, confirms my own impression, as he has nothing to express but admiration. It seems to me that Mr. James has not only abolished the argument deduced from the synthetic form of plants, as it now stands, but that that argument is doubly abolished if, as Mr. Carruthers has said, there is no synthetic form at all; because, in that case, the very basis of the argument is removed. With regard to the permanence of genera, Mr. James has fought that point on every stage of the geological record, and has taken his stand on every platform on which vegetable life is found, the result being that he has shown, in the case of the plants to which he has referred, that they display an entire constancy and permanence from the earliest forms; and that this is not only true of genera, but, to a very great extent, of species also. This seems to me to be absolutely fatal to the dogma Mr. James has combated. Again, the burst of new life in the upper chalk also seems to me to be fatal to the evolution theory. I hold also that the doctrine of the imperfection of the geological record would not be maintained by any one who has at all familiarised himself with the evidences afforded by the coal measures and the shale which is found in contiguity with the coal, for no one can examine one of our numerous coal-pits without being convinced that it affords the fullest possible development of the flora of that particular epoch; and not only is this the case with regard to one coal working, but all round the world the same phenomena present themselves in a manner that must be accepted as quite conclusive. I need not dwell further upon the subject, and have only to add that I am very glad indeed to have had the advantage of hearing Mr. James read so able and interesting a paper. (Applause.)

Rev. F. A. Walker, D.D., F.L.S.—With regard to the question of the permanence or persistence of types, I may state that there is a very interesting case exhibited in the Boulaq Museum which probably some of those now present may have seen, showing the permanence of types in plants, not in the shape of fossil remains, but in those of which we have the earliest historical knowledge. We are there enabled to see the crocus and the lotus, one or two species of moss, and two or three more plants that have been taken out of mummy-cases, and which date back three and probably four thousand years, side by side with specimens of the very same flowers recently gathered and dried in Cairo, the species and varieties of the crocus and lily being the same as are found at the present day—the crocus, as far as I can see, being identical with that which is found in the Campagna, and generally in the outskirts of Rome. I suppose the permanence of this type is to be attributed to the fact that it has always been a non-cultivated species. I may add, that growers in the neighbourhood of Cairo have tried to produce different species. The more I go about, the more am I struck with the

great similarity shown by the fossil remains found in England, and the plants growing in Egypt at the present day. The impressions of the leaves, and the leaves themselves, of the palms and magnolias that are dug up close to Bournemouth are just the same in appearance as those in Egypt now, and serve as evidence of a tropical climate at one time in our own land.

Mr. J. HASSELL.—I thank Mr. James for his interesting and instructive paper. For my own part, I do not claim to know much about fossil botany, but I have taught the young a little about the botany of the present day; and I remember on one occasion drawing attention to a fossil form on the table, and remarking that the nervation of the dicotyledons was different from that of the monocotyledons, and that of the acotyledons different from either of the others, and a child present said, "That cannot be a very old thing, sir, for it is exactly like this leaf," at the same time showing a leaf she had in her hand, the leaf of a recent fern. The more we know of the structure of plants the better are we able to see that no possible means within themselves could have produced the differences that are observable, and, consequently, the more confidently can we take up a position against the fascinating doctrine of evolution. I think it very desirable that the marked distinctions of species, which Mr. James has shown to be presented even from the very earliest ages, should be brought prominently before the young, by their teachers. Those who believe in evolution take advantage of every occasion which presents itself to inoculate the rising generation with their views. Why, then, should not the believers in special creation do the same?

Mr. W. P. JAMES, F.L.S.—I was much pleased to hear Mr. Carruthers say he does not believe in synthetic types of plants, and, if he were still present, I would explain to him that the last paragraph of my paper, headed, "Do Synthetic Types prove Evolution?" is written from an entirely neutral point of view. I do not say that I believe in synthetic types myself; I merely put it hypothetically, and I am very glad to find that Mr. Carruthers believes the cycads are not a synthetic type. I have never seen them except in greenhouses, and have only taken what I have said of them from books; but I think I may say that, if there were a synthetic type, one would imagine them to constitute such a type, intermediate between ferns, palms, and conifers. I think that many excellent geologists have been a little too rash in speaking of types as synthetic, where the evidence does not seem sufficient to justify the term. In reply to Mr. Dent, who asked me how the plants I have spoken of got into the South Pacific Sea, I have nothing to add to what I have already stated. That is a subject that does not belong to the question dealt with to-night; but it is, nevertheless, one of great interest. The reason I mentioned the Auckland Islands is that they are as far from Great Britain as they well could be. It is one of the great puzzles in botany to account for the antarctic species. Sir Joseph Hooker said, when he first explored those islands, and before he joined the evolutionists,

that the remoteness of those parts of the world and their isolation from the nearest land precluded the idea of species having migrated there; but since then, as he has become more or less of an evolutionist, I suppose he imagines a submerged continent along which the migration may have taken place. The question is, as I have said, a very puzzling one; for instance, how the little butterwort, which is a cold-climate plant, got across the tropics. Those who advocate a slow and gradual migration suppose that these plants went over the tops of the Andes; but the difficulty still remains—how did they get to the islands in the Antarctic Sea? The subject is a most interesting one, and those who are not botanists would find, in the great libraries to which they may belong or to which they have access, the *Flora Antarctica* well worthy of attention, as showing surprising constancy of genera, and as containing plates, coloured by Mr. Fitch, which are of astonishing beauty. I do not assert that all genera are constant; some, of course, are variable; but, nevertheless, we have to account for the fact that others are so amazingly persistent; and it should be remembered that, when we say a genus or species is constant, this involves a vast number of uniformities—thousands, in fact—down to the most minute points. (Hear, hear.) There is a plant called *Bidens tripartita*, found in the watercourses in the neighbourhood of London. If you take a specimen and strip off some of the florets that make up the composite flower, the smell of the receptacle at the top of the floer stalk will remind you at once of that of the dahlia, and here we have a very subtle bond of union indicated. Who would expect that this little English composite would show any affinity with a flower so different in appearance, and coming from America? Mr. Hassell made a most interesting remark about a fern. He gave an instance in which a child had recognised at once the likeness between the fossil and the existing ferns, and I can testify to the accuracy of the child's statement. The portion of the coal measures with which I used to have acquaintance was in South Wales, and I have only spoken of what I have myself seen. I never made a collection of the fossil ferns, but they were very familiar to me as a boy, and I remember that there was a district in which the shale was very brittle, and we used in walking about to break a great many pieces, and expose the beautiful impressions, which, however, were too fragile to bear handling, and so were lost. With regard to the theory of descent, I would only say that what I contend against is the doctrine advocated by Haeckel, that we must assume that all animals and plants have been lineally derived from their lowest forms. Haeckel and others have attempted to draw up a genealogical scheme for the vegetable as well as for the animal kingdom, beginning in the former with the lowest algæ, or oscillatoriaceæ, now found in the hot springs. Of course, when we see what tremendous gaps there are in this genealogical system, we are satisfied at once as to the impossibility of making it complete, and all wiser botanists have given up the attempt. In a modified form, perhaps, many have held evolution to be just possible. We might, perhaps, imagine

the creation of a form from which, as a generic type, species may have been produced by modification; but, after all, it is but a guess, and there can be no doubt that there are forcible arguments, especially those derived from the coal formation, against any theory of descent. The evolutionists know very well that this is about the strongest point against their doctrine that can be adduced, and it does not require much ability to put it clearly. (Applause.)

The meeting was then adjourned.