FURTHER NOTES ON THE GEOLOGICAL RECORD.

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IN the Address which I had the honour of delivering here last year, I prefaced my remarks on the nature and imperfections of the Geological Record by stating my agreement with those geologists who believe in the general permanence of continental areas as mainly land, and of deep oceanic basins as ocean. In confirmation of this view I dwelt on the facts that almost all known formations were either decidedly or comparatively shallow water-beds, and that the Chalk and other limestones formerly supposed to have been deposited in the depths of the ocean, were now becoming more and more recognised as having been formed in comparatively shallow seas, though at a considerable distance from land. After giving some illustrations of the nature and magnitude of the changes in the distribution of sea and land admitted by believers in this theory, and showing how greatly the distribution of animals and plants tended to confirm it, I passed on to consider the extremely partial and imperfect nature of the Geological Record, and the dangers of putting any great trust in negative evidence. On this occasion I propose to dwell on various points connected with the theory of the general permanence of continental and oceanic areas, and its influence on geological speculation.

A fortnight later than the day on which I had the honour of addressing you, the question of the permanence of ocean-basins formed the main subject of the presidential address of Dr. Blanford to the Geological Society. After full and careful consideration, Dr. Blanford thus sums up his view of the case:—

“It will thus be seen that whilst the general permanence of ocean-basins and continental areas cannot be said to be established on anything like proof, the general evidence in favour of this view is very strong. But there is no evidence whatever in favour of the extreme view accepted by some physicists and geologists that every ocean-bed now more than 1,000 fathoms deep has always been ocean, and that no part of the continental area has ever been beneath the deep seas. Not only is there clear proof that some land areas lying within continental limits have at a comparatively recent date been submerged over 1,000 fathoms, whilst sea-bottoms now over 1,000 fathoms deep must have been land in part of the Tertiary era, but there are a mass of facts, both geological and biological, in favour of land-connection having formerly existed in certain cases across what are now broad and deep oceans.”

It is gratifying to me to find that an authority like Dr. Blanford thinks the "general evidence in favour of this view is very strong;" whilst though believers in it probably agree in thinking that, in the main, areas now covered by 1,000 fathoms of water have remained sea since the beginning of the geological record, few, if any, I imagine, look upon the depth named as anything more than a round number giving a good general approximation to the truth. No one is likely to think that Nature abhors the elevation into land of a sea-bottom more than 1,000 fathoms deep, after the fashion of Galileo, when he explained the action of a pump by supposing the abhorrence of Nature for a vacuum of more than thirty-three feet. For it is evident that upheavals and subsidences have been of much greater magnitude and frequency in some districts than in others; regions in which volcanoes abound and earthquakes are common being especially liable to abnormal elevations and depressions. And as the elevation of land into a mountain chain implies a coincident depression of adjacent areas, we may naturally conclude, for instance, that the sinkings and upheavals in some countries of the Mediterranean, during a given period, have been both greater and more numerous than those on the shores of the Baltic. Consequently we may expect to find, here and there, exceptional islands separated from the nearest continent by seas of 1,000 fathoms or more in depth, and yet possessing indigenous mammalia, or other animals, having limited powers of crossing the sea; 1,000 fathoms in regions of exceptional volcanic activity appearing to be equivalent to 300 or 400 fathoms in quieter districts. I will now briefly refer to some of these exceptional cases.

Last year I alluded to the depth of the channel between Africa and Madagascar, as showing the remoteness of any connexion between the two countries, and thereby explaining the absence of the most characteristic of the continental mammalia from the great island. Madagascar is an exception to the general rule that sea now 1,000 fathoms or more deep has remained permanently sea, the Mozambique Channel exceeding that depth.¹ Consequently, though the Comoro Isles may have afforded convenient stepping-stones for birds, it is a more remarkable fact that Madagascar should possess any indigenous mammals than that they should be few compared with those of Africa. But on referring to Mr. Baron's paper on the geology of Madagascar (*Quart. Journ. Geol. Soc.*, vol. xlv., p. 308, 1889) we find, as might have been anticipated, that the great island was a centre of remarkable volcanic activity in Tertiary and post-Tertiary times, though now quiescent. The central portion consists of an elevated plateau from 80 to 160 miles wide and from 3,000 to 5,000 feet high, from which rise

¹ The depth of the Mozambique Channel, given in my address last year (*Proc.* vol. xi, p. 311) was from Wallace's "Island Life." In "Darwinism," p. 348, Mr. Wallace states that recent soundings have shown the figures in "Island Life," to be wrong.

peaks and domes of various crystalline rocks to a height of nearly 9,000 feet. The region occupied by these crystalline rocks has a length of about 730 miles, and an average breadth of perhaps 150. The strata have been thrown into numerous anticlinal and synclinal folds, their general line of strike corresponding with that of the longitudinal axis of the island. The dip of this great dorsal ridge is towards the west. The remains of many volcanoes exist, some dating their activity from some part of the Tertiary period, others being still more recent, though now extinct. From one of the older volcanoes vast floods of lava have been poured out. "Some of the lava streams are probably no less than twenty or twenty-five miles in length, and before they thin out, from 300 to 500 feet in thickness." Many of the newer volcanoes must have been active in comparatively recent times.

Evidently in the case of Madagascar we have a region in which we might expect an unusual amount of upheaval and depression. In early Tertiary times the island was probably smaller and less elevated than it now is, and the Mozambique Channel proportionately shallower, a land passage by way of the Comoro Islands existing, and allowing the lemurs and other mammals to cross over from Africa. Then, at some later Tertiary period, the mountain chain forming the backbone of the island gradually became elevated, and was accompanied by a corresponding depression of the bed of the Mozambique Channel, having a direction parallel to that of the mountain chain.

New Zealand is another instance of a group of islands whose fauna indicates that they were once part of a continent; they are nevertheless separated from Australia by sea more than 1,000 fathoms deep. But though New Zealand is now separated from Australia by deep sea, there are signs of a former connexion in the existence of a narrow submarine plateau stretching from New Zealand to, and beyond, Lord Howe's Island, and in the presence of others around New Caledonia. Both the North and South Island are mountainous, the North being distinguished by the large number of volcanoes upon it. Only one of these volcanoes is now active, the others having become extinct in times geologically recent. Similarly, among the mountains of South-eastern Australia (the part of the great island nearest to New Zealand) there are craters which have but recently become extinct. Thus (as in the case of Madagascar) it would seem that unusual activity in the forces upheaving the mountains of New Zealand and South-eastern Australia has been accompanied by a remarkable amount of depression in the area between them.

Again, the case of the Galapagos Islands seems to point to similar conclusions. This group of islands, which lies about 600 miles from the western coast of South America, is surrounded at a short distance from the existing shores by a sea 2,000 fathoms deep. Though, as Mr. Wallace informs us, there are no truly

indigenous mammalia, there are many reptiles, some of which seem as unlikely as land-mammals to be able to cross 600 miles of sea, as they consist of land tortoises, lizards and snakes. The tortoises are of a very large size. All these reptiles appear to be closely allied to South American forms. There are also thirty-one true land-birds showing affinities to those of tropical America. Darwin ("Voyage of the *Beagle*") describes two large tortoises which he saw on Chatham Island (one of the Galapagos group) each weighing more than 200 pounds, and in Albermarle Island were many great black lizards, between three and four feet long. The islands are all volcanic, many active volcanoes still existing in the western isles, while in Ecuador, the nearest region of South America, there is an unusual abundance of extinct or active volcanic peaks in the lofty chain of the Andes. Here, then, we have another of those cases in which we might expect an abnormal activity in the forces working for elevation and depression. We can hardly suppose the huge land tortoises to be more capable than land mammalia of crossing any considerable breadth of sea, while the South American affinities of the fauna also indicate a former connexion of the Galapagos Isles with that continent, though possibly a very narrow one may have sufficed. At the same time the fact that there are no indigenous mammalia seems to point to the conclusion that the separation dates from a very remote period, one probably as much more ancient than that of Africa and Madagascar as might be deduced from the lower nature of the land fauna and the greater depth of the intervening sea.

Dr. Blanford mentions a very curious instance, in Barbados, of the discovery of deep-sea Radiolarian ooze, resting upon "Sandstones and clays with coal, believed to be of older Tertiary age, and which are evidently littoral, estuarine, or fluvial in origin," Barbados being at present surrounded on all sides by seas over 1,000 fathoms deep. However, it seems to me that though Radiolarian ooze is a characteristic deposit of oceanic abysses it cannot be assumed to be an abyssal formation simply because it is Radiolarian Ooze. Radiolaria, being pelagic creatures, their remains might sink and be deposited on sea bottoms of very various depths. It is true that the "Challenger" and other observers have found that at depths of 3,000 fathoms calcareous organisms disappear and that in these deep abysses the sea-bottom consists of fine clay, unusually red in colour, and frequently containing in great abundance siliceous shells or spicules of Radiolaria. But though clay with Radiolarian remains seems to be invariably found in the deeper abysses of the ocean, far from land, and consequently beyond the reach of deposits resulting from land-denudation, it cannot be recognised as a deep-sea deposit when found elsewhere simply because it is red clay with Radiolaria. It is highly probable that Radiolarian remains alone

are found in the oceanic abysses simply because calcareous shells are dissolved away before reaching those vast depths, while siliceous shells or spicules are not. And should this be the case, it is evident that under circumstances unfavourable to the preservation of calcareous foraminifera in a shallow-water clay, silicious organisms might yet remain undissolved. Circumstances unfavourable to the preservation of calcareous remains would exist wherever deposition was extremely slow, and this would probably be the case where any very fine clay resembling that of the oceanic abysses was being formed. Thus it seems to me that the question at what depth this Radiolarian Ooze of Barbados was deposited is to be decided only after a full consideration of the whole of the evidence in that particular case.

Dr. Blanford remarks that Radiolarian Ooze is said to exist on other West Indian Islands. On looking at a physical atlas we may notice that a considerable area of the Gulf of Mexico and Caribbean Sea is more than 1,000 fathoms deep; and a study of North American geology² reveals the fact that in the Gulf of Mexico we have the southern end of a broad tract which formed an area of depression (like that occupied by the Mediterranean Sea) during the evolution of the American Continent. At one period a midland sea, east of the Rocky Mountains, allowed of communication between the Gulf of Mexico and the Arctic Ocean, and so late as Middle Tertiary times the Gulf of Mexico extended northward as far as the junction of the Ohio and the Mississippi. It may be remembered that a considerable proportion of the Mediterranean is more than 1,000 fathoms deep. It is probable that this southern portion of the American Mediterranean has varied very much from time to time, both in size and in the extent of its connection with the Atlantic. But it seems very unlikely that Radiolarian Ooze can ever have been deposited on any of the islands now forming its eastern boundary, really corresponding to that of the abysses of mid-ocean, without its character as an abyssal formation being attested by the presence of other deep-sea beds. These, however, as we have seen, are entirely wanting, the beds on which the Radiolarian Ooze rests being "evidently littoral, estuarine or fluvial."

In "Nature" for February 14th, and April 25th, 1889, we learn from Mr. Jukes-Browne and Professor Harrison that this Radiolarian Ooze of Barbados varies much in composition, being in some places almost purely siliceous, and in others largely calcareous. Some of the latter beds "have all the essential characteristics of typical (Cretaceous) chalk;" while other samples combine the characters of chalk and Radiolarian earth. The series of beds is said to contain "many interesting varieties of rock." This Barbados deposit is thus, though of varied composition, largely composed of material almost if not quite identical

² See Dana's Manual.

with English Chalk, which is now generally recognised as a comparatively shallow water formation. On the other hand Mr. J. W. Gregory in his interesting paper, "*Cystechinus crassus* from the Radiolarian Marls of Barbados," (Quart. Journ. Geol. Soc. vol. xlv., Nov. 1889) remarks that *Cystechinus* is one of the most typical of deep-sea Echinoids, and adds that "Neumayr quotes it with a few other genera as never found above the 1,000 fathom line." But the Echinodermata generally, though far more abundant in shallow than in deep water, have a much greater bathymetrical range than most other marine organisms, consequently the presence of any Echinoid genus in a given deposit cannot have the significance in determining its deep or shallow-water character which might attach to a form of life known to be of more restricted range. And the negative evidence that the genus in question has hitherto never been found above a certain level, furnishes in itself a presumption the importance of which may easily be overrated considering the extreme slightness of our knowledge of the faunas of sea-bottoms between 100 and 1,000 fathoms in depth. I am therefore inclined to think that in this case the evidence, on the whole, points to the conclusion that the Radiolarian Ooze is a comparatively shallow-water formation. With regard to the formation of red clay, I learn from Dr. Geikie that: "The east coast of South America supplies a peculiar red mud, which is spread over the Atlantic slope down to depths of more than 2,000 fathoms." (Text book, Ed. 2, p. 423.) Finely divided sediment of this kind might be carried along for immense distances by oceanic currents, and be slowly deposited here and there in the shallow water around islands very variously situated.

The small group of islands known as the Bermudas are about 700 miles from the nearest land, and have evidently been once more extensive than they now are, a layer of earth with remains of cedar trees having been discovered forty-eight feet below the present high-water mark. The islands are surrounded by reefs, the sea outside them being more than 2,000 fathoms deep, and are largely composed of limestone of variable character which has been formed by the blowing up of the fine calcareous sand resulting from the disintegration of coral, shells, &c., and its consolidation through the percolation of rain-water.³

In some parts of the island there is found a layer of red earth or clay resembling both in colour and composition the red clay of the oceanic abysses, which is known to exist a short distance away all round the Bermudas. Mr. Matthew Jones, the author of a Guide to Bermuda, quoted by Mr. Wallace, thought that the analysis of this red clay tended to show that it was identical with that of the deep ocean, and supposed that it had reached its present position by having been raised by volcanic agency from the ocean depths. But as Mr. Jones elsewhere mentions that this

³ Wallace "Island Life."

same red earth had been found, two feet thick, under some coral rock forty-two feet below low-water mark, and resting on a bed of compact calcareous sandstone, it is evident, Mr. Wallace remarks, that the similarity of its colour and composition to that of deep-sea clay goes for nothing in determining its origin, and that, as it is seen at various levels in this calcareous sandstone, it probably owes its origin to the decomposition of that rock or of the minute organisms abounding in the blown sand. But, whatever its origin, it is clear that it is not a deep-sea deposit.

These two cases seem to me to afford excellent instances of the danger of dwelling too exclusively on lithological similarity as an index to the nature of a deposit, and neglecting other evidence of much greater weight and deeper significance. In the Bermuda example the position of the rock was found to be decisive against the origin which had been assigned to it on the evidence of its composition. And in the Barbados case, the more fully the deep-sea theory is considered the greater appear to be its difficulties. In our Silurian series we have examples of enormous thicknesses of strata made up of shallow-water rocks deposited in a slowly-sinking area. But in this Barbados deposit we have—on the deep-sea theory—shallow-water deposits which have sunk in Tertiary times to a depth of 2,500 fathoms or more without having been covered during their descent by any strata but the abyssal ooze; which lies, as we have seen (according to this view), directly upon the shallow-water rocks. During re-ascent the Radiolarian Ooze again remained uncovered by any strata giving evidence of its descent to the depths of the ocean. We have, therefore, no direct evidence whatever of the supposed descent, a want in itself sufficient, I think, to discredit the deep-sea theory. And even apart from the want of any evidence of deep descent and re-ascent it appears to me extremely improbable on other grounds. For if we look at the history of any connected series of geological formations, what do we find? Not alternations of even comparatively deep-water and littoral deposits, but a tendency from sands to clays, and from clays to limestones in a sinking area, and the reverse in a rising one. In short, no better examples could be given than these two of the danger of not looking fairly at the whole of the evidence available, but deciding on the presumption furnished by a single item.

But the time at my disposal will not allow the discussion of any more real or alleged examples of exceptions to the general rule that areas now continental have been since the beginning of the geological record mainly land areas, and seas of more than 1,000 fathoms wholly marine. As I have already remarked, the immense differences between the amount of vigour shown by the forces promoting upheaval and depression in different regions render the existence of some exceptions almost or quite certain. Nevertheless, this theory of the general permanence of continental

and of deep-sea areas seems to me sufficiently well-established to form a sound basis from which to speculate on the various problems offered by the distribution of past and present faunas and floras. And when we look at a physical map showing groups of islands like those of the East and West Indies, and notice the curiously intricate arrangement of the areas occupied by deep and shallow water, it seems by no means improbable that more detailed marine surveying may some day demonstrate the existence of small submarine plateaux, once land, between Africa and Madagascar, South America and the Galapagos Isles, and Australia and New Zealand.

In order to test the evolution of geological opinion on this question no better means can be employed than a comparison of the map of the world given in Mr. Wallace's "Darwinism" with certain maps in the tenth edition of Lyell's "Principles of Geology;" the last-named work having been published in 1867, the first in 1889. The object of Lyell's maps was to show the distribution of land and water which would produce either extreme heat or extreme cold unaided by any other influences; that of Wallace to mark the general nearness of the 1000-fathom line to the shores of existing continents. Lyell's maps show that he felt quite safe in ignoring the present positions of the various continents and oceans, though he assumed that the proportion of land to sea had remained permanently the same from the remotest periods. He also recognised the great average depth of the sea as compared with the mean height of the land, and remarked that this would cause changes of climate to be exceedingly slow, from the immense time thereby required to alter the position of continents and of great oceanic basins. While Lyell, therefore, took the greatest care to ensure the same proportions of sea to land as those now prevailing, we also notice that there is no sign whatever that the comparative depths of the sea in various places were considered to afford any guide to the past extensions of a continent. His maps show continents and oceans changing their places in the most fundamental way. The one intended to illustrate that arrangement of land and water which would in itself produce a very warm climate shows the land mainly grouped within the tropics, and water prevailing towards the poles; while the coldest periods are thought to have followed the massing of land around the poles, with the presence of nothing but water within the tropics. For when Lyell was writing on this subject the Chalk was supposed to have been formed precisely as the Globigerina Ooze is now being deposited in the Atlantic Ocean. And when Chalk was supposed to be simply Globigerina Ooze that had been elevated into land, and to imply the former extension of the Atlantic over the greater part of Europe, it might well be thought to be the eastern part of a great formation, the centre portion of which remained beneath the ocean, while the

western might be represented by some American deposit. Similarly with other formations of comparatively shallow-water origin. If the bottom of the Atlantic might be raised into land in the case of the Chalk there could be no strong presumption against the former existence of a broad shallow sea, suited to the deposition of most other formations, where there is now much deeper water. With these views naturally went a practical belief in the almost world-wide extension of geological formations, and a tendency to correlate far distant deposits in countries like Europe and America, not as occupying somewhat parallel positions in two independently-formed series, but rather as easterly and westerly extensions of the same rocks. The various geological deposits of America, however, are evidently as purely local as those of Europe. But many years will probably elapse, and the permanence of continental and oceanic areas be much more thoroughly and generally accepted than it now is, before geologists, in their unguarded moments, cease to betray a practical belief in the universality of geological formations.

I now propose to refer, as briefly as possible, to the general nature of the geological systems of North America, more especially to those of the United States east of the Rocky Mountains.⁴ Above an immense thickness of contorted crystalline rocks, classed as Archæan, which are found chiefly north of the great lakes, there lies to the southward a great series of sandstones, shales, slates and limestones, which are naturally considered to correspond to our Cambrian and Silurian formations. The sandstones of the period styled Lower Silurian show every sign of having been formed close to the shore, and the lowest rocks of this system are mainly sandstones, while above them limestones prevail. No kind of rock, however, was formed simultaneously over the whole continent, the nature of the deposits depending on local circumstances. It is interesting to note that in this era, as in most of those succeeding it, the chief area for the deposition of limestone, especially of limestone formed in comparatively deep water, was the Mississippi basin. In that district, as I have already mentioned, was an inland sea, a northward continuation of the Gulf of Mexico, which disappeared only in times geologically recent. Professor Dana remarks that the limestones of Canada and New York belonging to this period may have been formed in comparatively shallow water, but that the Lower Magnesian Limestones of the Mississippi basin were possibly deposited in a deeper sea.

Between the Lower and Upper Silurian periods there was an epoch of disturbance. The fossils of the Trenton (Lower Silur.) period do not show the admixture of European forms found in the preceding and succeeding periods, a fact pointing to the conclusion that the limestones of this age were deposited in an

⁴ See Dana's "Manual of Geology."

interior sea, a land barrier preventing the introduction of European species. Professor Dana notes that the limestones of the Quebec epoch (Lower Silurian) contain a different assemblage of fossils from those of the interbedded shales, the species migrating as the nature of the sea-bottom changed. The beds classed as Upper Silurian consist mainly of limestones, sandstones and clays. Their fossils are all invertebrata and similar in general type to those of the Lower Silurian age.

The American "Devonian Period" comprises limestones, shales and sandstones. The most important limestone is in the lowest division, the next, in ascending order, is mainly composed of shales, the third of shales and shaly sandstones, while the fourth consists of shales and sandstones of various colours, red predominating. This last has a general resemblance to the Old Red Sandstone of Europe. It is evident that in the rocks of this period we have a gradual change from open sea to shore and lacustrine conditions. Between the Silurian and Devonian rocks of America we learn that there is "an easy transition." Between the Devonian and Carboniferous formations there is much unconformity in certain localities.

The lowest rocks of the American Carboniferous series are called Sub-Carboniferous. In Illinois, Kentucky, Iowa and Missouri they are mainly limestones, while eastward the quantity of limestone diminishes, the beds in Pennsylvania being mainly sandstones and shales. Dana remarks that, as during the first half of the Upper Silurian and in the earliest part of the Devonian period limestones were being deposited over a large part of the interior continental basin, so during the Sub-Carboniferous period there was in the same region an open sea, in which limestones were formed and which must have been connected with the ocean through the Gulf of Mexico. The Coal Measures of America overlies the Sub-Carboniferous rocks and are usually unconformable to them or to the Devonian or Silurian rocks on which they rest. They consist of sandstones, shales and coals, limestones being thin and exceptional.

Rocks that have been classed as Permian are confined in America to the interior continental basin, in parts west of the Mississippi, especially in Kansas. They are conformable to the Carboniferous beds and make one series with them. They consist of limestones, sandstones, marls, gypsum and conglomerates. They have been divided into Upper and Lower, but the latter have been considered to be Upper Coal Measures by some geologists.

In the course of some general remarks on the Palæozoic period, Dana states that on the northern border of the interior basin, near the Archæan rocks, the thickness of the Lower Silurian beds "indicates a great subsidence in that era, which was not afterwards continued." And the prevalence of limestones over the

interior basin marks that region as an extensive reef-growing sea, communicating with the Atlantic by the Gulf of Mexico, and possibly also with the Pacific during the greater part of the Palæozoic period.

We next learn that the formation styled Triassic in Eastern North America may belong in part to the Jurassic period. It generally consists of red sandstone, and in certain places there are valuable beds of bituminous coal in it. Few marine species are found in beds of this age, which were probably estuarine or lacustrine. Many mudcracks, raindrop impressions and foot-prints appear. The strange absence of marine deposits along the Atlantic border may be accounted for, says Dana, by supposing that the dry land stretched further eastward than now, and that shore deposits were formed which are now submerged. A change of level of 500 feet would take a breadth of eighty miles from the Atlantic and add it to the continent. During this period, while in the east the continent probably stood above its present level, over much of the Rocky Mountain region the land was barely emerging from the waters, or was covered by interior salt seas. In the succeeding Jurassic era there was a somewhat deeper submergence in this region, the waters communicated directly with the ocean, and marine life again appeared in it, though on the Atlantic border the land probably continued to extend further eastward than it now does. True marine Jurassic beds exist about the eastern ridges of the Rocky Mountains and elsewhere.

The strata known as Cretaceous rest unconformably on the preceding rocks. They are very varied in composition and are very well developed in the Upper Missouri region and the country bordering on the Gulf of Mexico; also, to a less degree, on the Atlantic border from Florida to New Jersey. The beds of the Missouri district are shallow-water deposits, but the limestones of Texas indicate a clearer and deeper sea. In the closing part of the Cretaceous era in the Rocky Mountain region there was a change from a general submergence under salt water to one of oscillations, which continued into Lower Tertiary times. In Wyoming, Utah and Colorado there is, in the upper part, a thick coal-bearing formation, which may be classed either as Cretaceous or as Lower Tertiary; the fossils, when compared with those of European formations, suggest partly Cretaceous, partly Tertiary, affinities.

After the deposition of this coal-bearing series there was more subsidence in the district around the Gulf of Mexico and up the valley of the Mississippi. In Eastern America there is little limestone belonging to the Cretaceous era except in Texas.

The marine beds of Tertiary age are in general deposited in districts in which the Cretaceous beds are found. They cover a narrow district on the Atlantic coast and the border

of the Gulf of Mexico and extend for a considerable distance up the Mississippi Valley. During this period the principal mountains of America received approximately their present prominence.

In the British territory and the north-eastern parts of the United States there is much glacial drift, probably the result of cold arising from the elevation of the land in the northern part of the continent.

This brief consideration of the geological structure of North America, east of the Rocky Mountains, reveals the following facts. We learn that while the Archæan rocks occupy a very large area, mainly north of the great lakes and west of Nova Scotia and Newfoundland, the newer formations lie southward and eastward, either in the Mississippi basin, along the course of the Appalachians, or towards the borders of the Atlantic and Gulf of Mexico; the latest deposits occupying the Atlantic seaboard, from New Jersey southward, the land bordering the Gulf of Mexico and the lower part of the Mississippi basin. Their position in the last-named district marks the time at which the great inland sea, so remarkable as an area of depression and deposition throughout the geological record, at last disappeared. And just as we find that our important European limestone, the Chalk, was formed in a broad midland sea, so we learn that in the great midland sea of America were deposited almost all the comparatively deep-water limestones. The other beds, as in Europe, were the products of much shallower water, and frequently, in the case of the sandstones, show that they were formed under shore conditions. No one series of beds was deposited alike in the midland sea, along the course of the Appalachians, and on the Atlantic seaboard, but in each area of deposition was formed something local and special. Not only were the American formations the products of a great area entirely distinct from the continents east of the Atlantic, but at any given period each submerged American district was producing something more or less differing from that of other regions.

It was, perhaps, inevitable that even geologists who fully recognised the absolute independence of Europe and America as areas of deposition should nevertheless be determined to correlate the various formations east and west of the Atlantic with each other; and, as geology is a specially British science, that American systems should have British names applied to them, with the result of a much closer practical identification of the two series than is really tenable. Even the amount of broad general parallelism between American and European formations may be easily exaggerated. On each side of the ocean we start with an unknown thickness of Archæan rocks; in each series we have, after the lapse of an immense interval of time, extensive Coal Measures; and, finally, among the latest geo-

logical deposits we have, on both sides of the Atlantic, widely distributed Glacial beds. It is easy to understand the existence of the quiet assumption that the lowest formations resting on the Archæan rocks were being deposited simultaneously in European and American areas, though there is neither evidence nor presumption in its favour. When, again, it was supposed that the Coal Measures were formed during a period remarkable throughout the world on account of the vast amount of carbonic acid gas in the atmosphere, it naturally followed that the Coal Measures of Europe, America and elsewhere were supposed to date necessarily from one epoch. This belief in the synchronism of all Coal Measures would naturally, in itself, lead to the opinion that the beds above and below this fixed point were also approximately contemporaneous, an opinion which would be little, if at all, weakened by any disparity in the thicknesses of the beds in question. While, coming to more recent times, it is evident that at least the astronomical causes which tended to produce the Glacial period must have affected the whole of the northern hemisphere. But, on the other hand, it is manifest that, while the lowest of the beds resting on the Archæan rocks may have been forming in American seas, the European areas on which Cambrian and Lower Silurian rocks now lie may have been mainly land or *vice versa*. Indeed, probability inclines towards the hypothesis that while little deposition was taking place in the eastern continental area, owing to the large proportion of unsubmerged land in it, the western was largely submerged, the normal proportions of sea to land being thus preserved. As regards the Coal Measures, few, if any, geologists now believe that there was any unusual amount of carbonic acid gas in the atmosphere of the world generally during their deposition. And any general similarity between widely-distant Coal Measure floras loses much of its significance when we remember that we have in each case not a fair representation of the flora of the period, but only of the flora growing in certain peculiar swamps, which was also best adapted for preservation therein. Consequently the Coal Measures of Europe and America may have been deposited at very different periods.⁵ And though astronomical causes evidently had something to do with the production of the Glacial period, the distribution of land and water must have been still more important in determining both the quantity of ice produced, and the character of the work done by it in any particular spot at any given time.

Indeed, in considering the geological record of any two widely-separated regions, we may naturally expect to find in each, marine

⁵ In the discussion on Mr. Penning's paper, "A Contribution to the Geology of the Southern Transvaal," at the Geological Society, February 25th, 1891, Dr. Blanford remarked that in India it had been found that the rocks associated with coal-bearing strata formed an enormous system, ranging from Upper Palæozoic to Upper Mesozoic.

limestones, clays and sandstones with, more rarely, estuarine, fluviatile, lacustrine and terrestrial formations. In other words, we may expect to meet with evidence of periods in which more or less shallow-water marine beds have been deposited in slowly-subsiding areas, as in the case of our Silurians; with formations that, like our Coal Measures, Permian and Triassic beds, indicate swampy or lacustrine conditions with many slight oscillations of level; also with fresh-water beds like those of our Wealden district. And the fossils of marine deposits formed under similar conditions on each side of an ocean like the Atlantic, which has a continuous shallow sea along its northern border, from which patches of land have emerged from time to time, might naturally be expected to show a strong general resemblance. But a very marked resemblance in the fossil remains of two widely-separated areas by no means implies synchronism as to the deposition of the beds in which they are found.

The tendency to a practical belief in the world-wide character of geological formations was denounced more than thirty years ago by Mr. Herbert Spencer in his well-known essay on "Illogical Geology," and by Professor Huxley in his address to the Geological Society on "Geological Contemporaneity and Persistent Types of Life," in 1862. Mr. Spencer pointed out that it was in the highest degree improbable that, while any series of strata was being deposited in one area, an equivalent series should be forming in a distant region under similar conditions, since subsidences in different places began and ended with utter irregularity: that between countries which were far apart there could be no evidence as to order of superposition, and that the test of mineral character was worthless considering the great changes in that respect in equivalent strata within the British Isles alone: that similarity in the facies of two faunas as evidence of synchronism was an assumption contrary to all probability, as it involved the postulate that there had always existed a similarity in the animal and vegetable life of the various parts of the earth, and that the forces tending to modify this life had always acted everywhere so as to produce parallel changes. Now the causes which have changed faunas, says Mr. Spencer, have been local, not universal; hence the animals and plants of some regions have changed largely, of others slightly during a given period.

Professor Huxley, in the address to the Geological Society in 1862, states that he agrees with Sir Henry De la Beche that "even absolute identity of fossils is no proof of the synchrony of deposits, while absolute diversity is no proof of difference of date;" and adds that Edward Forbes asserted that similarity in the fossils of distant formations was *primâ facie* evidence of difference of age. For Forbes held the doctrine of single specific centres, and considered it in the highest degree improbable that a species, whether it originated in one of the two districts or in some intermediate spot,

would appear in two distant regions simultaneously. But Professor Huxley adds that whether the hypothesis of single or of multiple specific centres be adopted, a similarity in fossils is compatible with the lapse of a vast period of time between the deposition of two formations. It had been stated, he remarked, on good authority, that between thirty and forty per cent. of the species of Silurian Mollusca were common to both sides of the Atlantic. But supposing 60 per cent. to be common to both, that would be no proof of contemporaneity. For should a geologist, a million of years hence, after Britain had been submerged and again upheaved, apply this standard in comparing strata now at the bottom of St. George's Channel with all that might then remain of the Suffolk Crag, he would decide that they were contemporaneous, though, as we happen to know, there is a vast interval between the two. What, then, could be the weight attaching to a much smaller identity of species in the case of two far distant formations? All, then, that geology could prove was the local order of succession. Homotaxis, or similarity of arrangement, could be demonstrated; but, Professor Huxley adds: "for anything that geology or palæontology are able to show to the contrary, a Devonian fauna and flora in the British Isles may have been contemporaneous with Silurian life in North America and with a Carboniferous fauna and flora in Africa." Geology provides us with most valuable topographical records, but cannot work them up into a universal history. But a time will come when the maze of the world's past history "will be securely threaded by the clue furnished by the naturalist."

Since the delivery of Professor Huxley's address there has been a slow and gradual recognition of the soundness of his views on the fallacy involved in the identification of homotaxis with synchronism. Nevertheless, we find him stating in his address to the Geological Society in 1870, that the use of the word homotaxis had not found much favour in the eyes of geologists. Even now it cannot be said that the old belief that a resemblance between the fossils of two widely-distant formations necessarily involves an approach to synchronism corresponding to the amount of the resemblance is practically extinct, though it may be theoretically abandoned. And if we cannot yet say that the time has come when the obscurity of the world's past history has been completely illumined by the labours of the naturalist, yet to naturalists like Mr. Wallace, who have made a special study of the distribution of life on the globe, we are largely indebted for additional light.

In treating of the synchronism of geological formations Professor Heilprin* decidedly disagrees with Professor Huxley as to the possibility of the co-existence of a Devonian fauna and flora in the British Isles with Silurian life in North America and a Carboniferous fauna and flora in Africa. He thinks on the other hand

* "The Distribution of Animals," p. 228.

that, "taking the facts in their entirety, the conclusion appears irresistible that formations characterised by the same or very nearly related faunas, in widely-separated regions belonged, in very moderate limits, to approximately the same actual age, and were practically synchronous or contemporaneous. The singular uniformity in the lithological character of many of the equivalent formations also favours this conclusion." And with regard to the extreme case suggested as *possible* by Professor Huxley, he asks why evidences of inversion in the order of deposit are not common, or at least well established, as they certainly would be, he thinks, were Professor Huxley's views correct. Why, he inquires, has it never happened that a fauna with an earlier *facies* has been imposed upon a later one, as the Devonian of Britain upon the Carboniferous of Africa or the American Silurian upon the British Devonian? It appears to me that if we follow the sound principle of endeavouring to explain the past by the aid of the present, we shall discover that Professor Heilprin has no right to expect the inversions he demands from the believers in Professor Huxley's views. We should confine our attention to some single class and the various forms of life belonging to it, both to avoid the inevitable confusion arising from any attempt to deal with a whole fauna at one glance, and because what is true of any given class is, in principle, true of all. In cases at the present day in which we have a primitive fauna belonging to a class existing contemporaneously with much higher forms of the same class elsewhere, it often appears that the more primitive group still predominates in a certain region because circumstances have enabled it to dwell in isolation, protected from the competition of more highly developed animals of the same class. Where there has been only comparative isolation, there we see a mingling of the more primitive representatives of the class with others of a more advanced type. While in regions of vast extent, and free from any formidable barriers to migration, we find either a decided diminution, or an entire absence of the primitive type. But should the barriers that have surrounded the primitive type, where it has been predominant or alone, be removed, what would be the probable result? Surely nothing can be more unlikely than that, on the opening of a new field for occupation, it should be able to dispossess the higher forms of its own class if they are already in possession. Even if able to hold its ground in the hitherto protected district which it had so long occupied, and to which it had become thoroughly adapted, the lower division could hardly be expected to spread far outside unless to occupy vacant ground. And in that case the geological record would not show the recurrence of an earlier type, but simply its unusual persistence in that locality, a very different and very common thing.

As an embodiment of the foregoing remarks in a concrete form no illustration can be better than one drawn from the pre-

sent distribution of the Marsupial order of the Mammalian class. Having evidently originated in the Eurasian continent, Marsupials existed in Europe in Triassic times, were common here in the Jurassic period, and evidently lived on this continent in considerable numbers as late as the Miocene era.

Since that date they appear to have become extinct in the Eurasian continent owing to the competition of higher forms, though in Australasia (with some insignificant exceptions) they are both the highest and the most numerous of the indigenous Mammals. This result, as I need hardly point out, is due to the isolation of Australasia from Asia as regards the passage of land Mammalia, an isolation which the great depth of the sea between the islands of the Asian and those of the Australasian groups shows to be of ancient date. Then, in Central and South America, especially the latter, we again meet with Marsupials, but in conjunction with higher Mammalia, and by no means so numerous either in species or individuals as in Australia. Thus the animals of this order, once so numerous and ubiquitous, are now restricted to the two quarters of the globe most remote from each other. The Australian Mammals have been described as having a Jurassic *facies*. Similarly the Mammals of South America might perhaps be considered to have a Miocene *facies*, according to our European standard. And it is obvious that the European palæontologist, called upon to decide as to the probable age of deposits abounding in equally recent mammalian remains from Australia, South America and Europe respectively might, if he judged solely by the European standard, state that the Australian remains presented a Jurassic, the South American a Miocene, and the European a Recent *facies*. Yet, on the other hand, deposits of equally late geological date fairly representing the Mammals of Central Europe, Eastern Asia, and Southern Africa, would all appear equally Recent. Thus contemporaneous deposits in different quarters of the globe, all abounding in the remains of animals of the same class, may, as regards the orders represented, resemble each other, differ to a very considerable degree, or be so distinct in character as to suggest on the one hand a Jurassic, or on the other a Recent fauna. And as the causes influencing the present distribution of animals of any class are evidently those which have acted throughout the ages included in the Geological Record on animals of every class, we have in the above example an ample vindication of the soundness of the remarks of Mr. Herbert Spencer and Professor Huxley, made so many years ago.

Professor Heilprin notices the very curious examples brought together by Dr. Blandford* of the contradictory dates suggested in certain cases by one class of fossil remains as compared with another. The Mammalian beds of Pikermi in Greece, those of the

* Address to Section C, Brit. Assoc., 1884.

Siwalik Hills and the marine deposits associated with them, and other Indian examples are given. But perhaps the most remarkable case is that of the Australian Carboniferous rocks, in which beds containing plants of decidedly Jurassic aspect are interstratified with marine deposits containing fossils resembling those of the Lower Carboniferous beds of Europe. Professor Heilprin remarks that it is not very surprising that there should be discrepancies of this kind between the marine and terrestrial faunas and floras of widely distant areas, considering the broader and more even distribution possible to marine as compared with land animals and plants. He therefore looks upon marine faunas as affording much more decisive evidence than other fossil remains.

It appears to me that the one great advantage possessed by marine fossils is that as marine beds are both more numerous and important than other deposits, marine fossils constitute a much more frequently available means of comparison than other forms of life. As tests of the real equivalence in time of widely separated groups of strata they are liable to be just as deceptive as any other remains, and can possess no special authority. Between countries like Western Europe and the eastern parts of the United States, where—granting the permanence of oceans and land areas—water communication has always been broad and easy, and land communication either non-existent or comparatively difficult, marine fossils certainly seem to offer the better guide. But this would not be the case if, for example, taking the geological record of Western Europe as our standard, we compared it with a more or less similar series of rocks in China. For between China and Western Europe there has existed, in all probability, more or less land communication throughout the greater part of geological time. On the other hand, communication by sea must have involved in certain periods migration (eastward or westward) round the southern end of Africa, and in others a very much shorter journey across the sea which appears to have stretched across Northern Africa in Eocene times, or by some still more northerly route. The communications, in short, between the continents east and west of the Atlantic have always—granting the theory of the permanence of continental and oceanic areas—been an almost constant quantity, when compared with the utter irregularity of the facilities for migration between the Atlantic and Indian Oceans. Hence we need not be surprised that Dr. Blanford has been able to bring forward many instances in India and other countries bordering on the Indian Ocean, of faunas of one age (according to our standard) associated with floras of another, and that we do not hear of similar cases in the United States.

Professor Heilprin thinks that in early times the distribution of the Mollusca was even broader than it now is, a circumstance for which he accounts by supposing the former existence of greater facilities for dispersion, and the prevalence of a more

equable climate. But it seems to me that we might naturally expect apparent evidence of a former broader distribution on other grounds. For the earlier the period, the simpler, lower in organisation and less varied was its fauna, as a whole, and the less it tended to vary during a given time. And the molluscan (or other) genera found most widely distributed would naturally be the most numerous and consequently persistent forms of the groups to which they belonged.

Though the distribution of marine faunas is, on the whole, less irregularly restricted than that of land animals and plants, yet it is evidently by no means unrestricted. An isthmus like that of Suez or Panama may form a barrier between the Mollusca of adjacent oceans almost as effective as the interposition of a narrow sea-channel in the case of land animals. Professor Heilprin notes many cases in which molluscan species occupy an extremely limited area, and states that almost every oceanic island or island group has its own distinctive molluscan fauna. Thus it would appear that in certain cases they may have their genera and species distributed as peculiarly as land mammalia themselves, owing to the periodical elevation and removal of land barriers. Professor Heilprin also (speaking of the Mollusca) notes that very surprising and entirely inexplicable "is the distribution of the fifteen or more species (including a dozen species of the genus *Triton*) whose common habitat is the Indian Ocean and the West Indian Sea, when no connecting representative is found in the intermediate area." But in this instance it would seem that the periodical appearance of the sea connecting the Indian and the Atlantic Oceans by way of Northern Africa would sufficiently explain this peculiar distribution. Allowing the probable development of the species in question on the shores of the Atlantic, the opening out of this mid-African sea would permit their extension eastward to the Indian Ocean. Their distribution west of the Atlantic may possibly be accounted for—as Professor Heilprin explains the appearance of certain other forms common to the Caribbean province and the west coast of Africa—as the result of "remarkable vital tenacity on the part of the embryo."

The most singular example of "homotaxial perversity" brought forward by Dr. Blanford in his British Association Address is that, already mentioned, of certain Australian formations. The Coal Measures of Southern and Eastern Australia have a flora of an essentially Jurassic *facies*, while the marine beds interstratified with them are just as decidedly Carboniferous in aspect, according to our British standard. Of course the Australian series of rocks must have been formed quite independently of those of Europe; nevertheless it is interesting to consider briefly the bearing of this evidence on the probable antiquity of the Australian Coal Measures when compared with those of Europe. It is evident, from the distance between those two quarters of the globe, that

the Carboniferous marine fauna can hardly have been predominant in the Atlantic and in the Australian seas at the same time. It is possible that it may have flourished in Australian seas before it abounded in the Atlantic. But this seems less probable than the hypothesis that it predominated around Australia at a considerably later period than in the Atlantic ; inasmuch as in Australia it is associated with a flora of much later date than in Europe. The age of this Australian Jurassic flora as compared with that found in the Jurassic strata of Western Europe is a question on which we have absolutely no evidence to guide us. But (in conjunction with the marine fauna) it is at least as likely to be of later as of earlier date.

Of course, in cases like this, in which one kind of fauna gives evidence—according to our European standard—opposed to that of another, or in which the fauna and flora give contradictory results, it is impossible to decide that any one kind of fossil invariably offers a better criterion than other classes. The evidence given by land and marine faunas must necessarily show discrepancies when judged by any local standard, whether European or Australian, inasmuch as facilities for the dispersion of one group form barriers to the diffusion of the other. It may be convenient to decide solely according to the evidence of marine faunas as to the age of a formation, marine fossils being the more invariably attainable ; but when a decision can be obtained only by neglecting the whole of other important branches of the available evidence, it is extremely likely to be reversed in some future court of appeal.

In conclusion, I will briefly mention one or two points bearing upon the subjects I have been discussing which were alluded to by Mr. Walter Gregory, in his very interesting paper on the Tertiary Echinoidea, which we had the good fortune to listen to a month ago. We have seen that Mr. Herbert Spencer, Professor Huxley and Sir Henry de la Beche unite in stating that similarity in the facies of two faunas as evidence of synchronism is contrary to all probability, as the causes which have changed faunas have been local, not universal. Consequently “even absolute identity of fossils is no proof of the synchronism of deposits, while absolute diversity is no proof of difference of date.” Mr. Gregory notes in his paper that when we compare the Echinoidea of our (Pliocene) Crag with examples from the Mediterranean Pliocenes, we find a very marked difference. On the other hand, he quotes a remark of Mr. Clement Reid, to the effect that our Crag fauna resembles, not the Pliocene Mediterranean fauna, but that now existing in that sea. Again, Mr. Gregory remarks that there are very striking resemblances between the Echinoid faunas of the English Pliocene and of the present West Indian seas. It appears (he adds), from the absence of the West Indian genera in the Belgian Pliocenes, that they came from the west ; and a curious point in

their distribution is that there is no trace of their migration by the shallow water bounding the Atlantic on its northern margin. Mr. Gregory tells me that he does not think it at all likely that the free-swimming larvæ of these West Indian forms may have been transported across the Atlantic along the course of the Gulf Stream. However, it occurs to me that the simplest explanation may be that, while absent in formations now land, they may very easily be present in others now submerged, but which an elevation of the coast to the amount of some 50 or 100 feet might expose. In any case it seems important to note these instances in which the theoretical conclusions of Mr. Herbert Spencer and Professor Huxley, which have hitherto been disregarded in practice, have been verified by actual experience.

On retiring from the office of president, to which you did me the honour to elect me two years ago, I can only say that my two years of office have but increased my appreciation of the many advantages offered by the Geologists' Association, and my kindly feelings towards those of its members whom I have met at its various meetings. In my successor, Professor J. F. Blake, you will have not only an eminent geologist of great and varied experience, but an old member of the Association, who has always been most active and useful both in reading papers and directing in excursions. It would indeed be difficult to suggest any one better qualified for the post of president. I can but wish him as agreeable an experience as mine has been.